The Long-term Effects of Charter School Exposure on Education and Health Behaviors*

Samuel Asare

Department of Economics
Georgia State University
Atlanta, GA 30302-3992
Email: sasare1@student.gsu.edu

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Abstract

Improving school quality is one objective that policymakers pursue. One policy used in the United States is state charter laws, allowing approved K-12 schools to operate independently. This paper estimates the causal effects of charter school exposure on students' long-term outcomes using the restricted, geocoded National Longitudinal Survey of Youth data and school information from the National Center for Education Statistics. We use an instrumental variable method by constructing charter school exposure in the county of birth as an instrument for actual exposure in the county of residence. Our results suggest strong evidence of charter schools increasing four-year college completion and reducing adverse health behaviors among some demographic groups. While college graduations are more pronounced among females and nonwhites, the reduction in binge drinking and cigarette smoking is higher among highly educated individuals. Overall, our results demonstrate that charter schools improve students' long-term outcomes.

Keywords: Charter school exposure, college completion, cigarette smoking, and binge drinking

JEL Code: I12, I13

Disclaimer: This research was conducted with restricted access to Bureau of Labor Statistics (BLS) data. The views expressed here do not necessarily reflect the views of the BLS.

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1. Introduction

Over the years, policymakers have devoted efforts to increase the quantity and improve the quality of human capital investments.[1] Increasing the quality of human capital investment can induce further investment among individuals with sub-optimal levels (Becker, 1967; Card et al., 2012; Aaronson and Mazumder, 2011). After Becker (1964) established that investment in human capital improves health, raises earning, and increases the person's knowledge about own lifetime, with more evidence from Schultz (1961, 1967) that investment in human capital positively affects aggregate income and economic growth, several studies have focused on it. In this paper, we present new evidence on how efforts to improve the quality of K-12 education in the U.S. influences human capital development and other outcomes.

A recent state-level policy intended to improve the quality of human capital investments in the U.S. was the state charter laws, which allow public schools to operate independently with less supervision from state and local school authorities. Minnesota was the first state to pass the law in December 1991 and opened its first charter school in 1992. Many states passed the laws subsequently and opened charter schools, and as of 2020, 44 states and the District of Columbia had charter schools. Currently, about 7500 charter schools exist, serving 3.3 million students, about 5% of the population of public schools.[2]

Since its inception in 1991, several studies have examined the impacts of charter schools on students' outcomes. However, these studies have primarily focused on immediate and short-term outcomes such as students' performance and school competition (Sass et al., 2016; Booker et al., 2011; Angrist et al., 2016; Ni, 2009), with a few considering medium-term impacts, including health behaviors and earnings.[3] Also, each study focused on a few jurisdictions, starting from one city to a maximum of sixteen states.[4] Therefore, a study that uses national data to assess the charter schools' long-term im-

[1] Policies such as state compulsory school attendance laws (CSLs), which started in Massachusetts in the late 19th century, were implemented to increase human capital investment. Other policies such as classroom size regulations, school lunch, anti-bullying regulations, etc., seek to improve the rate at which human capital investments convert to the desired outcomes. Improving the efficiency of the investment in human capital is analogous to increasing the rate at which every dollar invested in human capital translates into the desired outcome.

[2] See from data.publiccharters.org/

[3] Most of these studies consider outcomes, including college enrollment and a transition from 2- to a 4-year institution. For example, Sass et al. (2016) and (Booker et al., 2011) has college enrollment as the main outcome in their Florida and Chicago studies.

[4] Notably, studies have focused on Texas, Chicago, Michigan Denver, New York, Boston, Florida, North Carolina, and a few others ((Sass et al., 2016; Booker et al., 2011; Dobbie and Fryer, 2020; Bettinger, 2005; Booker et al., 2007; Ni, 2009; Hanushek et al., 2007; Zimmer et al., 2012; Jinnai, 2014; Hoxby, 2004; Davis and Raymond, 2012).

pacts on students' later-in-life outcomes is important. With many states opening charter schools by 2003, it is worth focusing on their impacts on students' later-in-life outcomes.

This paper estimates the long-term effects of exposure to charter schools on later-in-life education outcomes and health behaviors. Specifically, we answer the following questions. (1) What are the long-term impacts of charter school exposure on educational outcomes such as completed years of schooling and college completion? (2) What are the long-term effects of charter school exposure on health behaviors, including cigarette smoking and excessive alcohol consumption? We define charter school exposure from two different dimensions. First, we construct charter school exposure as the number of years students were exposed to charter schools in their resident counties before graduating from secondary schools. Second, we define a comprehensive measure of exposure analogous to those used in Aaronson and Mazumder (2011), who estimated Rosenwald school exposure on black achievements and Miller and Wherry (2019) that assessed the long-term effects of early Medicaid coverage, respectively. Our measure calculates the intensity of charter schools coverage before graduating from high schools.

We use an instrumental variable approach as our empirical strategy since the ordinary least squares has two primary limitations. First, there might be endogenous migration, which would bias the estimates if we use the exposure at the county of residence. Charter school opening could induce students to migrate across counties. Second, even if we define charter school exposure at birth counties, we still face attenuation bias concerns since people do not stay in their county of birth forever. To address these issues, we compute the two measures, exposure from the county of residence and at the county of birth. Then we use the birth county exposure as an instrument for that of the county of residence. Therefore, the first-stage model regresses the county of residence's exposure on exposure from the birth county and controls for individual characteristics, county, and birth year fixed effects. The second stage then estimates the impacts of charter school exposure from the county of residence on the outcomes. Using this strategy overcomes the attenuation and endogeneity concerns.

We link potentially exposed individuals from the restricted, geocoded National Longitudinal Survey of Youth to charter schools at the county level using the National Center for Education Statistics Common Core Data, providing the universe of public elementary and secondary schools. A summary of our findings is as follows. We find a local

average treatment effect estimate such that an additional year of charter school exposure increases the chances of completing a four-year college or better by 3%. When we use charter school coverage as the measure, our results are that a 1-point increase in the exposure increases the probability of completing a four-year university education or better by 0.4% among those induced. We also obtain larger estimates (i.e., 4% for every additional year and 0.5% for a 1-point increase in charter school coverage) when we restrict our sample to include only those who completed high schools. We also demonstrate that charter schools favor females more than males and blacks and hispanics than whites on four-year college completion. Our local average treatment effects for alcohol consumption and cigarette smoking are small but not negligible and more pronounced among high-educated individuals and blacks and hispanics.

The study makes four substantial contributions to the literature. First, we are the first to provide national estimates on charter schools' impacts on students' outcomes to the best of our knowledge. Second, because the law started in 1991 and by 2003, more than 40 states had opened charter schools, there is a need to consider their long-term impacts on students' outcomes. Importantly, the inconclusive evidence of their short- and medium-term effects makes it impossible to predict their long-term effects. Third, a few studies have considered the impacts of charter schools on health behaviors. Although the policymakers intend to improve education outcomes, we demonstrate that charter schools also reduce adverse health behaviors. Finally, rather than focusing on only the direct effects (i.e., charter school attendance), our strategy estimates the spillover effects of charter schools, adjusting for their impacts on those who were indirectly affected.

We organize the rest of the paper as follows. In Section 2, we provide brief institutional details of charter school establishment. Section 3 summarizes the previous studies on charter schools, while Section 4 discusses the conceptual framework and mechanisms through which charter school exposure affect students' long-term outcomes. The empirical models and charter school exposure measures and data sources for this study follow in Section 5. We present the results in Section 6 and discuss the results, provide policy recommendations, and conclude the paper in Section 7.

2. Institutional Details

Charter laws are state-level legislation that permits K-12 schools to operate independent of local authorities. A state that legislates a charter school law provides guidelines for writing charter contracts. Charter authorizers are responsible for managing charter schools through the contracts. The authorizers can be the local school board, the state education board, or any independent organization. Charter laws differ by state. For example, several states place caps on the number or percent of charter schools the state (or a school district) can establish at any period. Mississippi passed charter law in 1996 but started with only one pilot charter school until 2013. California restricted authorizations to 250 schools in 1998/99 but allows a successive increase of 100 annually. Table A in the appendix presents states' charter law and regulations on the number of charter schools that the school districts permit. By 2018, about sixteen states and the District of Columbia imposed caps on charter schools.

Within school districts, charter authorizers are responsible for managing charter contracts. A traditional public school (henceforth, TPS) can convert into a charter school, while new schools can also begin as start-up charter schools. Charter schools can also reverse to TPS at any time. Non-profit organizations, people, or communities who wish to start charter schools apply for approval from the charter authorizers. In the application, the prospective school provides a comprehensive description of the school, including the attendance zone, the number of proposed students and teachers, available facilities, food and health service available, and students' background information such as students' age and grade levels. If the authorizer approves the application, they write the charter contract, and the two parties sign it.[5] The contract specifies essential clauses, including the duration of the charter status, the minimum academic performance, and a periodic reporting of the school's progress.[6] The school keeps the conditions in the contract; otherwise, the authorizers revoke the contract. A charter contract is also not renewed automatically. Instead, continuation depends on how well the school performs in upholding the standards specified in the contract.

After gaining the charter school status, it operates independently as a public school. Charter schools are semi-autonomous. Like any TPS, charter schools are publicly funded,

[5] See more from https://www.dekalbschoolsga.org/charter-schools/.

[6] An example of a charter contract is available at https://www.gadoe.org/External-Affairs-and-Policy/Charter-Schools/Documents/Atlanta%20Public%20Schools.pdf.

have more freedom over their budgets, staffing, curricula, and other operations. They cannot charge tuition or demand extra fees from students and must hold the same academic accountability measures as TPSs and private schools (Yilan and Berger, 2011). Meeting the accountability standards outlined in the contracts are some of the ways that charter schools use to maintain their charter status so that they get exempted from a particular state or local rules and regulations accompanied by freedom, flexibility, and autonomy.[7]

Unlike the TPSs, charter schools are open to all students within the school district or attendance zone. Students within the school district are given enrollment opportunities before allowing those outside the school district to sign up if seats are still available.[8] In this regard, parents freely choose to enroll (or disenroll) their children in (or from) any charter school. A charter school cannot discriminate on demographic characteristics or medical conditions. A student who transfers from a TPS to a charter school moves with the previously provided funds. If a charter school receives more application than its capacity, it uses a lottery strategy to allocate students. Also, charter schools have periods when students can apply for consideration. Therefore, charter schools are alternative (i.e., substitutes) to TPS and private schools since parents do not incur additional direct costs to transfer their children. In localities where all public schools are charter are called charter districts or system. A few charter districts exist.[9] The Decatur City Schools in Georgia is an example of a charter school system.

3. Previous Studies

Many studies have estimated the short-term impacts of charter school attendance on several outcomes, predominantly test scores but find mixed evidence with no consensus. Some evidence of no impact exists (Dobbie and Fryer, 2020; Zimmer et al., 2012; Booker et al., 2007; Hanushek et al., 2007; Bettinger, 2005). However, there is both positive and negative evidence as well. One group of studies find positive effects (Abdulkadiroğlu et al., 2017; Angrist et al., 2016; Dobbie and Fryer Jr, 2015; Deming et al., 2014; Jinnai, 2014; Abdulkadiroğlu et al., 2011; Hoxby, 2009; Booker et al., 2008; Holmes et al., 2006;

[7] See more from "The Condition of Education 2018", a report on education from the National Center for Education Statistics.

[8] See more from https://www.gadoe.org/External-Affairs-and-Policy/Charter-Schools/Pages/General-Frequently-Asked-Questions.aspx.

[9] See more from https://www.crpe.org/publications/charter-school-districts.

Hoxby, 2004). On the other hand, some studies also find negative effects (Imberman, 2011; Winters, 2012; Ni, 2009; Sass, 2006; Bifulco and Ladd, 2006).[10] Other nonacademic outcomes of students such as health behaviors are rarely analyzed in the literature. Dobbie and Fryer Jr (2015) find that charter school attendance reduces pregnancy for girls and incarceration for boys using New York data. One caveat is that studies that find positive impacts on short-run outcomes also find that students perform poorly in the few years following enrolling in charter schools (Booker et al., 2007; Hanushek et al., 2007).[11]

A few studies have considered medium-term outcomes, including college enrollment and shifts from 2- to 4-year institutions. Generally, they find positive effects (Sass et al., 2016; Angrist et al., 2016; Dobbie and Fryer Jr, 2015; Booker et al., 2011).[12] Deming et al. (2014) is the only study that has studied four-year college degree completion. They use Charlotte-Mecklenburg schools data to find that girls who attend their first-choice schools are 14 percentage points more likely to complete four-year degree colleges, but no effects on boys. To the best of my knowledge, only two studies have analyzed labor-market outcomes. However, these two papers find contradictory evidence. Sass et al. (2016) find that charter school attendance has positive effects on students earnings in their early 20s, while Dobbie and Fryer (2020) find adverse effects.[13] Therefore, the medium-term effects are also inconclusive. Besides, other studies have considered the effect of charter school competition on students' outcomes. In general, there is mixed evidence in the literature as well. While studies such as Ni (2009) find less competitive effects, Zimmer et al. (2009) finds no impact, as Holmes et al. (2006) demonstrates positive effects on competition among schools.

The studies discussed above focused on a few states or cities, usually from one state or city to a maximum of sixteen states or cities. However, as of 2017, all but six states had opened charter schools. Since the school systems in states operate differently based on their educational policies, state-level estimates in the literature may lack external validity. Therefore, a study that uses national data is important. Additionally, charter schools'

[10] Hoxby (2004) surveyed 36 charter schools in 15 states and found that charter school attendance increases math and reading proficiency except that of targeted and at-risk students. The study included these states: Alaska, Arizona, California, Colorado, DC, Florida, Georgia, Hawaii, Illinois, Louisiana, Massachusetts, Michigan, New Jersey, New York, Ohio, Oregon, Pennsylvania, Texas, and Wisconsin.

[11] When students transfer from TPSs to charter schools, they experience lower outcomes than their counterparts who remain in TPSs in the first two years. They equalize in the third and fourth years before experiencing a positive impact afterward.

[12] Dobbie and Fryer (2020) analyzed administrative data from Texas and found that at the mean, charter school attendance has no impact on test scores, and the best case is increasing both test scores and college enrollment.

[13] Sass et al. (2016) study students from Florida and Chicago using administrative data find that charter school students experience higher earnings in their mid-20s. (Dobbie and Fryer Jr, 2015) also analyzed the impact of charter school attendance on students' outcomes six years after enrollment using Texas data.

long-term impacts are relevant since states and the federal government expenditure on welfare programs correlate with the later-in-life outcomes. For example, states spent about $31.7 billion on Temporary Assistant for Needy Families (TANF) in 2015,[14] $592.75 billion on Medicaid in 2017,[15] and $66.54 billion on Supplemental Nutrition Assistance Program (SNAP) in 2016.[16] Understanding how charter school exposure affects these outcomes is useful because policymakers will understand how they possibly change these expenditures through education.

4. Conceptual Framework

Card and Krueger (1996) developments on Becker (1967) model provides a basis for understanding how charter schools affect students' future outcomes. Additionally, Aaronson and Mazumder (2011) framework demonstrates how changing schools' availability and quality affect the socially optimal choice. One precise prediction of the latter study's model was that Rosenwald's school construction, increasing the number and quality of schools in rural counties, increased the time spent in school among treated students. Since charter school opening is an expansion of school choice, we develop a similar framework for understanding the model's prediction as charter schools opening varied over time. By equating the marginal cost to the marginal benefit of schooling, students can choose their optimal education. Since the marginal benefit is a positive function of the school quality, improving the school quality increases the marginal benefit. Hence, the optimal level of schooling among affected school children rises as the school quality improves.

Suppose there are individuals with a suboptimal level of schooling due to some reasons, including low quality of schools and lack of competition. Then any policy that improves the quality of education leads to an increment in human capital investments. If opening charter schools enhances school quality within that neighborhood, students whose marginal benefits rise will invest more in their education. It implies that the theoretical prediction is an increment in schooling years among students with suboptimal levels due to charter school openings. Of course, one can argue with many reasons why charter school openings may improve school quality. States passing charter laws permit

[14] See from https://www.acf.hhs.gov/ofa/resource/tanf-and-moe-spending-and-transfers-by-activity-fy-2015.

[15] See from https://www.medicaid.gov/state-overviews/scorecard/national-context/annual-expenditures/index.html.

[16] See from https://fns-prod.azureedge.net/sites/default/files/snap/FY16-State-Activity-Report.pdf.

the penetration of charter schools, which serve as alternatives to TPSs to drive competition. Therefore, every new charter school potentially improves the average school quality in its neighborhood. Also, the fact that parents and students searched for alternative schools suggests that they were not satisfied with the quality of TPSs and wanted better alternatives.

Contrarily, suppose charter schools opening decreases the quality of schools in its neighborhood. In that case, the marginal benefit of schooling falls, and the optimal choice of years of education decreases as well. The optimal levels of investments in human capital will fall among affected school children. However, the argument that charter schools can reduce the quality of schools in their neighborhoods is debatable. On the one hand, proponents argue that charter schools are under close supervision and close if they cannot satisfy their charter contracts. The authorities' close monitoring ensures that charter schools are accountable to the charter contract. Moreover, parents possibly migrate or move their children to places with relatively good schools if schools' quality falls in their neighborhood. However, critics of charter schools argue that since a few charter schools closed for many reasons, including mismanagement, inadequate enrollment, and non-compliance of contract, they could have detrimental effects on students. Therefore, one cannot undermine the possibility that some charter schools negatively affect quality in their neighborhoods. In summary, the theoretical prediction of a charter school opening on human capital investment is ambiguous.

Aside from educational outcomes, there is no direct connection between charter school attendance or the state's charter law and health behaviors. However, there are so many mechanisms through which charter school exposure can potentially affect health behaviors in the long-term. Earnings and education are two apparent mechanisms. If charter schools impact earning and education, then we expect health outcomes to be affected. Starting from Grossman (1972), a body of literature has established the effects of earnings and education on health behaviors and outcomes. In his model, education increases the productivity of medical services, which predicts that any exogenous shock that shifts the education level increases the demand for health. Therefore, higher education leads to improved health outcomes. Also, in the model, higher earnings increase the opportunity cost of sick days. People cannot afford to stay home when their earnings are high. Therefore, they invest more in their health. The higher earnings translate into

health investment to get more healthy days to work.

Many empirical studies have established these predictions of the Grossman (1972) model. For example, studies including but not limited to Gerdtham et al. (1999); Apouey and Clark (2015); Lindahl (2005) find that income improves health while Lleras-Muney (2005); Silles (2009); Kenkel et al. (2006) find that higher education causes less risky behaviors and improve health outcomes. Using charter school exposure as an exogenous source of variation for education to estimate their impacts on health behaviors and outcomes is a useful exercise.

Because Sass et al. (2016) find that charter school attendance increases students' earnings in their mid-20s, it is exciting to explore how these students' health behaviors and outcomes are affected. Also, since Dobbie and Fryer Jr (2015) demonstrate that Texas' charter school attendance reduces teenage pregnancy and male incarceration, we further explore these outcomes using national data. Only these studies have analyzed the charter school effects on earnings and health behaviors to the best of our knowledge.

Aside from the two mechanisms discussed above, Dobbie and Fryer Jr (2015) identified other channels through which charter school attendance can affect health outcomes and behaviors. They argued that noncognitive skills, social networks, and economic preference parameters are other mechanisms. They also find that charter school attendance negatively impacts noncognitive skills, including self-esteem and persistence. Also, they find that attending charter schools has no effect on the discount rate but increases risk-aversion. The incremental change in risk aversion implies that charter school students are less likely to take risky behaviors, including smoking, drinking, and drug abuse, but are more likely to invest in their health, including purchasing insurance, exercising, and eating healthy food. Finally, they find that charter school attendance does not cause changes in peer quality. Because the evidence suggests that charter schools affect health behaviors, it is interesting to use nationally representative data.

5. Methods

5.1. Empirical Strategy

To address endogeneity in this study, we abstract away from models that compare the outcomes of students who attend charter schools to students who attend TPSs. We

discuss the various method used in the literature in Appendix B. Our method relies on a few assumptions. First, we assume that an opening of charter schools potentially affects all elementary and secondary school students at all grade levels in the county. Because every student at the level of implementation of the law in the county with a charter school is potentially exposed, it is not feasible to use a pre-post approach to identify the effects. Second, we assume that there is no heterogeneous effect across grades. In other words, the impact of exposure in elementary level 6 is not different from that of grade 12. From these assumptions, we estimate a model that compares the outcome of students with varying levels of exposure based on year, cohort, and the county of residence (i.e., cohort-by-year-by-county analysis).

We begin with a basic specification of an intent-to-treat model below:

$$Y_{ibct} = \gamma_0 + \gamma_1 \text{ACSExposure}_{ibc} + \boldsymbol{\beta}\mathbf{X_{ibct}} + \boldsymbol{\lambda_c} + \boldsymbol{\lambda_b} + \boldsymbol{\lambda_t} + \xi_{ibct}. \tag{1}$$

In equation (1), the variable Y_{ibct} represents the outcome of the individual i, born in birth cohort b at county c, whose outcome was observed at year t. The outcomes are years of schooling, college attendance, cigarette smoking, and alcohol consumption. The variable ACSExposure_{ibc} represents a measure of charter school exposure for individuals i, among the birth cohort b, and in county c. The vector X_{ibct} represents a set of individual basic characteristics. To overcome potential endogeneity, we only include age, race, gender, and education. Also, $\boldsymbol{\lambda_c}$, $\boldsymbol{\lambda_b}$, and $\boldsymbol{\lambda_t}$ represent vector of county, birth cohort, and year fixed effects, respectively. Finally, the variable ξ_{ibct} captures the random unobserved component of equation (1).

The argument for specifying the model of this form is that the outcomes of the student within a county with charter schools may vary by birth cohort. In equation (1), the coefficient of interest is γ_1, capturing the effect of charter school exposure at the county level. By including the county, birth cohort and year of outcome fixed effects in the model, we compare individuals born in the same county across periods, while those in counties without charter schools serve as controls for those in areas with charter schools. The model exploits the cross-cohort variation in the timing of the opening of charter schools in counties.

In the next few paragraphs, we provide a detailed account of the strategies used to generate various measures of charter exposure. These measures rely on a few assumptions

as follows. First, each cohort begins their first grade at age 6 and are expected to graduate from high school at age 18, irrespective of the state of residence. Although the school starting age in states in the U.S. are 5, 6 or 7, the mean and median age of starting school is 6. Second, every child in ages 6–18 in counties where charter schools opened was exposed. Third, all children of ages 6–8 enrolled in schools.

As our first measure of the charter school exposure, we aggregate the number of years that each birth cohort were exposed based on their county of residence. This variable is discrete and takes values 0 to 12. Using this definition, we assume that all states use a 12-year system where students begin first grade at age 6 and are expected to graduate at age 18. Therefore, by observing the grade of the student at the time the county of residence first opened a charter school and the years expected to graduate, we calculate the maximum years of exposure.

Our second measure of charter school exposure is like the definition of Rosenwald school exposure in Aaronson and Mazumder (2011). In their study, they constructed the "Rosenwald Coverage" that each student born in year b in county c experienced over ages 7–13 as the average probability of enrolling in the Rosenwald school.[17] Additionally, Miller and Wherry (2019) used a similar strategy in their long-term effect of early life Medicaid study. In their study, they constructed a cumulative measure of public health insurance eligibility at ages 1-18 for each birth year and the state as the fraction of children eligible for coverage at each age during childhood in each state and summed across ages. We construct our exposure variable as the average charter school coverage that each student born in year b in county c experienced between ages 6–18 based on counties ever resided before graduating from high school. That is, we compute the second measure of exposure ("actual exposure") based on all counties that the student lived before graduating from secondary school. Specifically, for all individuals in my sample who began school after their counties opened charter schools, we define exposure as:

$$\text{ACSExposure}_{ibc} = \frac{1}{T_{ib}} \sum_{\tau=b+6}^{b+12} \frac{\text{CSStud}_{c\tau}}{N_{c\tau,6-18}}, \tag{2}$$

where T_{ib} denotes the number of years student i in birth cohort c was exposed to charter

[17] Aaronson and Mazumder defined their measure as $\text{E}_{bc} = \frac{1}{7} \sum_{\tau=b+6}^{b+13} \frac{\text{T}_{ct} \times 45}{N_{ct}}$, where E_{bc} represents Rosenwald exposure for individuals born in year b in county c, T_{ct} represents the number of Rosenwald teachers and N_{ct} represents the number of black populations in county c.

school, which depends on the counties lived. We also use $CSStu_{c\tau}$ to represent the number of charter school students in county c at year τ and $N_{c\tau,6-18}$ for the number of school-going children of ages 6-18 in county c in year τ.

This measure of charter school exposure calculates the cumulative probability of enrolling in a charter school at the county level and normalizes it by the expected years of exposure. It measures the average probability of enrolling in charter schools for all individuals throughout their elementary and secondary school years. The variation comes from the fact that individuals in counties without charter schools get zero exposure, while those in counties with charter schools get value ranging from 0 to 1, depending on the years of exposure and available seats. Therefore, two individuals born in the same county can have different exposures due to differences in the year of birth and places stayed.

Notice that exposure to charter schools depends on the counties that the person lived during his elementary and high school period. A major drawback of this approach is endogenous migration. Students could move due to charter school opening. This will bias the estimates in equation (1) if estimated by OLS. Even if we calculate the exposure at the county of birth, we would attenuate γ_1 since people do not stay in their county of births forever. To address these issues, we calculate the exposure from the county of birth and follow Miller and Wherry (2019) to use an instrumental variable approach. The first stage model is as follows:

$$\text{ACSExposure}_{ibc} = \alpha_0 + \alpha_1 \text{CSExposure}_{bc} + \mathbf{\Phi X_{ibct}} + \boldsymbol{\lambda_c} + \boldsymbol{\lambda_b} + \boldsymbol{\lambda_t} + \nu_{ibct}. \tag{3}$$

In equation (3), the dependent variable represents the "actual exposure," which depends on the year born and county of residence before graduating from high school, is regressed on charter school exposure in the county of birth, CSExposure_{bc}. We also include a vector of covariates that we use in equation (1). The parameter of interest is α_1, which shows the correlation between charter school exposure at the county of birth and residence.

We discuss the identifying assumptions as follows. First, the instrument needs to be relevant. We test this assumption from the data by looking at the first stage estimates, capturing the correlation between exposure in the county of birth and residence. A high positive α_1 and F-statistic would imply a robust first stage estimate and suggest exposure in the county of birth highly predicts exposure in the county of residence. The second identifying assumption required for a causal interpretation of our estimates is the

exclusion restriction. This assumption requires that the charter school exposure be uncorrelated with omitted and unobserved variables that affect education outcomes and health behaviors. It also implies that the only mechanism through which the instrument affects the outcomes is exposure in the resident counties. The extent to which this assumption is satisfied depends on the randomness of charter school opening in birth counties. If charter school opening correlates with county characteristics, including wealth, educational resources, and educational outcomes, our IV estimates would be biased. We minimize this possibility by including county-level time-varying factors in our models. Finally, we discuss the assumption of monotonicity, which requires that the instrument must affect subjects in the same direction. In other words, the exposure in the birth county must increase exposure in the county of residence and not decrease it. Since this assumption is less likely to be satisfied among some children (i.e., some non-compliers exist), our estimates come from only compliers (local average treatment effects or simply LATE), children whose charter school exposure positively correlates with exposure in their birth counties.

5.2. Data Sources

5.2.1 Student-Level Data

The primary source of individual-level data comes from the publicly available and restricted, geocoded National Longitudinal Survey of Youth (NLSY), organized by the Bureau of Labor Statistics (BLS). The BLS follows different groups of individuals in the NLSY surveys. One cohort is a sample of youth born from 1957 through 1964, which included about 12,700 individuals surveyed in 1979 (i.e., NLSY79). Since charter school openings started in 1992, none of the youth in this sample potentially attended one. Fortunately, the BLS began to follow the children of all females in the NLSY79 cohort as well. This series tracks all children under the age of 14 born to women in the NLSY79 cohort annually beginning from 1986. In the NLSY79, the mothers provide information on their children. Starting in 1994, the BLS followed the young adults aged 14 or more born to all women in the NLSY79 cohort. These two series were combined and name NLSY79 Children Survey and Young Adult Survey.

We include all individuals in the sample potentially exposed to charter schools

born from 1974–1995 in the NLSY79 Child and Young Adult Survey. By choosing this sample, we exclude all children who had completed high school before 1992, when charter school opening started. We supplement these individuals with the NLSY97 cohort, born from 1980-1984, due to sample size concerns. One advantage of using the NLSY data is that it collects information on respondent's state and county of residence. In the children's sample, because we can identify where their mothers lived when they were born or before charter school opening began, their county of birth and residence are available.[18] We obtained the respondents' geographic data and linked it to the publicly available information. The publicly available data has all other data, including demographics, including family income, age, sex, educational attainment, years of schooling, economic status such as employment status, type of employment, and wages, etc., and health behaviors, including smoking, alcohol consumption, and incarceration status.

5.2.2 Charter School Data

One limitation of the NLSY data is that the BLS did not collect charter school information until 2000. Consequently, using the actual charter school attendance to estimate the long-term impacts can bias the estimates due to the missing information. We use the National Center for Education Statistics (NCES) Common Core Data (CCD)—data on the universe of public elementary and secondary schools. Every year, the NCES gathers information on all primary and secondary schools. Relevant information needed is the number of full-time teachers and county of residence of all charter schools. Therefore, the unit of analysis for the variable of interest, charter school exposure, can only be aggregated within the residence county.

A limitation to the NCES-CCD data is that they included charter schools' identifiers starting from 1998, even though charter school opening began in 1992. To overcome missing data concerns, we supplement the NCES-CCD data with two additional sources to identify all charter schools. First, we contacted the twenty-four states that opened charter schools before 1998, but only eight states have made the data available. Unfortunately, some states do not keep information on charter schools at all. Second, we scrape charter school data from the U.S. Department of Education (ED) National Charter School Resource Center (NCSRC) website. The ED-NCSRC keeps names of all charter schools

[18] Unfortunately, the NLSY data does not include census tracks, residential address, or school district of residence. Therefore, the smallest geographic identifier is county-level information.

it has funded in the past. Importantly, for all active charter schools under their umbrella, it keeps their information, including address, year of opening, number of students in the current year, and grades offered in the current year. Unfortunately, it does not include information on closed charter schools. Nevertheless, it publishes a complete list of all closed charter schools every year. Their publication consists of the school name, state and school district of residence, and the year opened or closed. Hence, we identify a complete list of all charter schools that opened and is still in operation or closed. We match all charter schools that existed before 1998 to the NCES data using school names and addresses.

5.3. Descriptive Statistics

Figure 1 shows variations ins states' adoptions of charter school laws over time. Seven states had not adopted the policy by 2015. Among states that passed the law, Figure 2 shows the timing of charter school openings within counties. Our definition for presence is all counties that had at least one charter school. We also calculate charter school coverage in each county and year, shown in Figure 3. Despite the fact that several counties opened charter schools, their students' populations were mostly low and below 25%. Therefore, the average exposure across all periods is expected to be small.

In Table 1, we summarize the variables. We obtained about $13,000$ individuals born in 518 counties. About 56% of them were exposed to charter schools in their county of residence. The average years of charter school exposure were 3.1 years, and their chances of getting admissions in charter schools were about 2.8%. Among those exposed to charter schools, the average length of exposure was about 5.6 years, and they had a 5.1% chance of enrolling in a charter school. To get enough variation in the variable of interest (i.e., exposure to charter schools), we exclude all counties with less than five births throughout the study period. Of the sample, 7,348 lived in counties with charter schools before graduating from secondary schools, while the remaining 5,817 lived in counties that never opened charter schools before graduating from high schools. The sample consists of 60% of individuals drawn from the NLSY97 cohort (the cohort born from 1980 to 1984). The remaining 40 percent comes from the NLSY79 Child and Young Adult Survey, born in 1971 to 1995. The NLSY oversampled the minority (i.e., the blacks and hispanics). Whites, blacks, and hispanics in our sample are 49, 31, and 20%,

respectively. To mitigate the impacts of oversampling in our regressions, we use sampling weights and report the weighted means in the last two columns of Table 1. The weighted means are closer to national estimates. Males constitute about 51% of the sample. The ages of individuals in the sample ranged from 15 to 42, with an average of 30.

Since we do not have information on the county of birth for the individuals in the NLSY97 cohort but only know their counties resided on their 12th birthday, we use them as proxies for their birth counties. With charter schools opening beginning from 1992, we include a dummy variable in the regressions to distinguish between all individuals whose county of births are available or those in states that later adopted charter school law. About 60% of the individuals have information of their county of birth or residence at age 12 available.

We also summarize the outcomes in Table 1. The schooling outcome represents the highest grade as of the date of the last interview. The survey asks respondents to select one for the following categories: 8th grade or less, some high school, high school graduate, some vocational or technical after high school, completed some vocational or technical after high school, some college, completed an associate degree, completed bachelor's degree, some graduate, completed a Master's degree, some graduate beyond a Master's degree, Ph.D., some professional education such as Law, Medical School, Nursing, etc., and completed a professional education. We coded these categories into a continuous education outcome. For individuals with years of schooling above 20, we top-coded to 20.[19] The data shows that, on average, students in the NLSY attended some college (i.e., 13 years of schooling). We do not find any statistically significant difference between the years of education of those exposed to charter schools and those that were not. Since our continuous education measure is more likely to be inaccurate, we also create discrete education outcomes as follows: some high school, high school graduate, some college, and a college degree or better. About 19% had no high school diploma in the sample, while 26% had completed only high school. Also, about 30% of the individuals had between 13 and 15 years of education (some college). The remaining 34% had college degrees.

Two other outcomes that we consider in this study are cigarette smoking and alcohol consumption. Prevalence of current smoking (i.e., past 30-days smoking) was 2% lower among those exposed to charter schools than their counterparts that did not have any

[19]We do not distinguish between the number of years required to complete different post-graduate programs, especially professional programs such as law school, medical school, doctoral programs.

exposure. However, binge drinking (i.e., drinking more than 5 bottles a day) did not differ by charter school exposure status.

6. Results

6.1. First Stage Estimates

Some information not reported in Table 1 is exposure when individuals are treated as if they never moved from their county of birth (i.e., exposure at the county of birth). Among school children born in counties that never opened charter schools, only 10% had charter school exposure with about four months of exposure on average. In contrast, children born in counties with charter schools were 96% more likely to have exposure and an average of 5.6 years. Not everyone whose birth counties opened charter schools had actual exposure since some individuals moved from their birth counties before charter schools opened or before beginning K-12 education.

The description above suggests that the unconditional correlation between charter school exposure in the birth county and exposure in the county of residence is positive. On average, individuals with more years of exposure at their birth counties are more likely to be exposed to charter schools in their counties resided. We formally test the correlation and present the first stage estimates for all outcomes in Table 2. The estimates in Panel A correspond to the first stage results for the education outcomes, Panel B for alcohol consumption, and Panel C for current cigarette smoking. The results in columns (1)–(4) in each panel show the case where we construct charter school exposure in years. Therefore, we regress the years of exposure in the county of residence on those from the birth counties. The estimates in the last four columns are for the case where we define charter school exposure as a coverage rate, which is the likelihood of attending a charter school in the county of birth and residence. We provide alternative specifications to demonstrate the robustness of the estimates to the control variables. Each estimate comes from a separate model specification. All the regressions include gender (male/female), race (black, white, and hispanic), a dummy for birth county availability, and NLSY cohort fixed effect. The specifications in columns (2) and (6) include the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), we add control county time-varying characteristics,

including poverty and unemployment rates.

For all outcomes and specifications, the first stage estimates are large, statistically significant at 1%, and with F-statistics above 100, suggesting the instruments are relevant (Lee et al., 2020). Specifically, the estimates in the first four columns show that an additional year of charter school exposure in the birth county is associated with approximately ten months of actual exposure for all the outcomes. Similarly, those in the last four columns suggest that a 1-point increase in charter school exposure in the birth county is associated with at least a 0.7 percentage points increase in the likelihood of receiving actual exposure during the years in K-12.

6.2. Charter School Effects on Education Outcomes

Table 3 presents the effects of charter school exposure on education. In Panel A, we present the results for the case where we define education in discrete terms. However, the discrete education outcome estimates are small and statistically insignificant, except those in the first two columns. When we measure charter school exposure in years, the estimates are similar across different specifications, but the standard errors increase in less parsimonious models. The estimate in the first column of Table 3 suggests that every additional year of charter school exposure increases schooling years by one week among those induced by the instrument. By including survey year and birth cohort fixed effects (see Column 2), which also account for the student's age, the estimate increases to approximately ten days. After including the county fixed effects, we lose precision due to increases in the standard errors. Our preferred specification in the fourth column, where we include all the controls as we as county-time varying characteristics, the estimate is very imprecise. When we define charter school exposure as coverage rates [see equation (2) and the results in columns (5)–(8)], our estimates are smaller and statistically insignificant across all specifications. We suspect that our findings of imprecise estimates for the discrete education outcome are due to, in part, our inability to precisely calculate the actual years of schooling of the students. For example, we do not know the exact years of education for those with some high school education.

Because the completed years of schooling results are less attractive, we also measure education in discrete terms. Specifically, we focus on completed bachelor's degrees or better against everyone else and report the results in Panel B of Table 3. Focusing on

our preferred specifications in columns (4) and (8), we find that every additional year of charter school exposure increases the probability of completing a four-year university education by 0.8 percentage points (i.e., 3% increase at the mean) among those induced by the instrument and is statistically significant at 10%. Using charter school coverage, we find a LATE estimate of a 1-point rise in exposure increases the probability of completing a four-year degree college by a 0.1 percentage point (or 0.4% at the mean of the outcome) and is statistically significant at 5%. The results become interesting when we focus on the effects of charter school exposure the schooling outcomes among high school graduates. We find that a 1-point increase in charter school coverage increases the likelihood of completing a four-year degree college by 0.15 percentage points (i.e., 0.5% at the mean of the outcome) among those induced by our instrument and is statistically significant at 1% (see Panel C of Table 3). It suggests that charter schools induces students who graduate from high school to attend and complete four-year degree colleges.

In Table 4, we demonstrate that charter schools significantly impact females than males (see Panel A and B). Males experience smaller effects and are statistically insignificant at all conventional levels. On the other hand, we find an enormous impact among females, but only significant at 10%. A 1-point increase in charter school coverage increases the probability of completing a four-year degree college by about 18 percentage points. We also find that charter schools improve four-year college completion among nonwhites than whites (see Panel C and D). No statistically significant effect of charter school exposure increasing four-year college completion was found among whites (see Columns 4 and 8 of Panel C). We find that a 1-point increase in charter school coverage increases the probability of nonwhites graduating from four-year colleges by 10 percentage points and is significant at 10% (see the last column of Panel D).

6.3. Charter School Effects on Later-in-Life Health Behaviors

We also provide results on how charter school exposure possibly affects later-in-life health behaviors. One outcome we consider is excessive alcohol consumption. It is responsible for over 95,000 deaths annually in the United States. It is associated with poor pregnancy outcomes and several chronic health effects, including heart attack, high blood pressure, heart disease, stroke, liver disease, and cancer.[20] We obtained 12,482

[20]See from https://www.cdc.gov/chronicdisease/resources/publications/factsheets/alcohol.htm

individuals who reported their alcohol consumption behavior. As already described in Table 1, about 17% of these individuals are binge drinkers (i.e., consuming five or more bottles per day).

The results of the effects of charter school exposure on binge drinking are shown in Table 5. From the full-sample results in Panel A, we find that charter school exposure has no local average treatment effects on binge drinking. In some specifications, we obtained inconsistent signs (i.e., positive effects instead of negative). Specifications that exclude the county of birth fixed effects reveal that each additional year of charter school exposure reduces binge drinking by about 0.4 percentage points (i.e., 2.4% at the mean of the outcome) and is statistically significant at 1%. Our regression results from the charter school coverage show that a 1-point increase in exposure decreases binge drinking by approximately 0.08 percentage points (i.e., 0.4% at the mean), but only statistically significant at 10%. We do not find any statistically significant estimates from our preferred models (see columns 4 and 8 of Panel A).

Because the minimum drinking age was 21 during the years that the NLSY measured the outcomes, we restrict our sample to demonstrate the effects of charter school exposure on binge drinking among individuals in the legal drinking ages in Panel B of Table 5.[21] However, the results do not differ significantly from those from the unrestricted sample. We found that a 1-point increase in charter school exposure decreases binge drinking by 0.1 percentage points (i.e., 0.6% at the mean) and is statistically significant at 5%. Again, we find only imprecise LATE estimates by including the county of birth fixed effects in the specifications.

We explored to show any evidence of heterogeneity in the effects of charter schools on binge drinking, analyzed by gender, race, and education in Table 6. While we find only noisy estimates for males (see Panel A), the results from some specifications among females are statistically significant (see Panel B). Some estimates among whites are statistically significant (see Panel C), but among nonwhites, the estimates are either noisy or significantly counterintuitive (see Panel D). The estimates are most substantial among those with college degrees (see Panel F) than those without college degrees (see Panel E). A 1-point increase in charter school exposure decreases binge drinking among college graduates by 0.2 percentage points (i.e., 1.0% at the mean) and is statistically significant

[21] See more of the National Minimum Drinking Age Act of 1984 from the following link https://www.cdc.gov/alcohol/fact-sheets/minimum-legal-drinking-age.htm.

at 1%. Our preferred specification, which includes the county of birth fixed effects, shows an estimate of approximately 1.7 percentage points and is statistically significant at 5%. On the other hand, we did not find any statistically significant estimate among individuals without college degrees for our preferred models (see Panel E). At best, we find from the model that excludes birth county fixed effects that one additional year of charter school exposure reduces binge drinking by 0.6 percentage points (i.e., 3.5%) among individuals without college degrees and is statistically significant at 1%.

The last outcome that we consider is current cigarette smoking. Cigarette smoking remains the leading cause of preventable death. It is responsible for more than 480,000 deaths annually.[22] Among adults of ages 18 and above, about 14% of the U.S. population are daily smokers. We analyze past 30-day cigarette smoking behavior. We organized information on about 11,156 individuals who responded to the survey questions on their smoking behaviors. Among the respondents, approximately 34% are current smokers (see Table 1).

The results of the effects of charter school exposure on current smoking are shown in Table 7. We find that charter school exposure has small local average treatment effects on current smoking. From our preferred specification, every additional year of charter school exposure decreases current cigarette smoking by 0.3%. However, no meaningful results were found from the case where we define charter school exposure as coverage. It implies that a student with twelve years of charter school exposure has approximately 3.7% lower probability of becoming a current smoker.

Our final results table demonstrates possible evidence of heterogeneity in the effects of charter school exposure on current smoking in Table 8. The specification that does not include the county of birth fixed effects shows statistically significant effects of a one-year increase in charter school exposure reducing current smoking by 0.7 percentage points among males (see Panel A). On the other hand, the estimates are all noisy among females (see Panel B). We find twice and statistically significant evidence of charter school exposure reducing current cigarette smoking among nonwhites than whites (see Panel C and D). Specifically, a one-year charter school exposure reduces current smoking by 1.2 percentage points among nonwhites and is statistically significant at 10%. Even though all the estimates among whites have correct signs, they are imprecise. Considering het-

[22] See more from https://www.cdc.gov/tobacco/campaign/tips/resources/data/cigarette-smoking-in-united-states.html.

erogeneity by education, we find that charter school exposure effectively reduces cigarette smoking among the most educated individuals. From our preferred specification, we find that one additional year of exposure decreases cigarette smoking by 1.4 percentage points (i.e., approximately 4.1% at the mean) among individuals with some college graduates or better and is statistical significance at 5% (see Panel E). We find half of this estimate among those with high school graduates or lower educated individuals and only statistically significant in the specification that does not include the county of birth fixed effects and time-varying area-level characteristics (see Panel F).

7. Discussion and Conclusion

States adopted charter school laws starting from 1991, and by 2003, more than 40 states had opened charter schools. Charter schools are public educational institutions that operate without state and local school boards' interference over specified years. Since states opened charter schools, several studies have analyzed their impacts on students, predominantly contemporaneous schooling outcomes such as test scores and high school completion. However, there are gaps in the literature, and other important questions remain unanswered. First, there is no consensus on the direction of their impacts since studies find negative, null, and positive results from different states. Second, no study has used national data to estimate charter school effects on students' outcomes. Finally, a few of the available articles have assessed their impacts on students' long-term outcomes. This study fills the gap by using national data to estimate the effects of charter schools on students' later-in-life education and health behaviors.

We use student-level data from the National Longitudinal Survey of Youth (NLSY) by combining the children of the 1979 cohort with the youth from the 1997 cohort. Some of the children of the NLSY79 cohort in the Child and Young Adult Survey were born when states passed the laws and opened charter schools. Also, the NLSY97 cohorts born from 1980 - 1984 were in elementary and high schools when several states opened charter schools. We obtained county geocoded, restricted students' information from the Bureau of Labor Statistics. Because the NLSY excluded students' information on the type of school they attended until 2003, we organized charter school information from the National Center for Education Statistics Common Core Data (NCES-CDD), which

provides information on all K-12 schools. We supplement NCES-CDD data with charter school information from two other sources. First, we contacted and obtained data from the state's department of education. Second, we use charter school opening and closure information from the U.S. Department of Education National Charter School Resource Center since some states did not respond to our request or have the data available. We link students from the NLSY samples to the universe of elementary and high schools at the county level, which is the lowest geographic identifier in the NLSY.

Because we cannot identify students who attended charter schools in the NLSY, we calculate two charter school exposure measures at the county-level. First, we calculate the number of years that students were potentially exposed to charter schools. Since the years of exposure do not have information on their chances of getting seat assignments, we also calculate a second measure, the intensity and coverage of charter schools (i.e., number of charter school students in the relevant population). We calculated the weighted probability of charter school coverage for every student in the sample.

Our study uses instrumental variable (IV) strategy, which addresses two key methodological concerns. First, regressing the outcomes on the charter school exposure, calculated at the county of residents may bias the estimates due to endogenous migration. Because school choice is an individual decision, rather than random assignment, students could move around counties in response to charter school openings. Second, attenuation bias becomes another challenge when we calculate the charter school exposure at the county of birth rather than the county of residence. We address these concerns by using the county of birth exposure as an instrument for the exposure at the counties resided throughout elementary and high school years in the IV framework.

The findings of this study are as follows. First, we show that charter school exposure affects four-year college completion. Our local average treatment effect (LATE) estimate is that an additional year of charter school exposure increases college graduation by 3%. Given that students in the sample were exposed to charter schools for three years on average, we think the charter schools have substantial and desirable effects on students' long-term education outcomes. It also suggests that students with more years of exposure have significant chances of completing four-year degree programs than those born in the same county without any exposure. In terms of the charter school coverage, we find that a 1-point increase in exposure increases the probability of completing a four-year

college by 0.4% among those induced by the instrument. Again, our estimate implies that students in counties with more charter schools have higher chances of completing a four-year college than those born in the same county but with fewer charter schools. We also demonstrate that the effects are large for blacks and hispanics than whites and females than males.

Aside from education outcomes, we also consider two health behaviors. Excessive alcohol consumption (or binge drinking) and cigarette smoking are the outcomes we analyze. Our results suggest that charter school exposure reduces binge drinking. We find that a 1-point increase in exposure decreases binge drinking by 0.4% and 0.6% among those in the legal drinking age of 21 and above. We also find some evidence of heterogeneity by the level of education. A 1-point increase in exposure decreases binge drinking by 1% among individuals with four-year degrees or better. We do not find any statistically significant evidence among those without college degrees. While we do not find robust evidence from the full sample on cigarette smoking, we show some evidence from our sub-sample analysis. We find that every additional year of charter school exposure decreases cigarette smoking by about 3.6% among blacks and hispanics and 4.1% among individuals with more than twelve years of education.

This study has several strengths. We highlight a few as follows. First, we are the first to estimate both the direct and indirect effects of charter schools using exposure. With several studies focusing on actual attendance, they ignore the spillover effects of charter schools. By following individuals in the NLSY over time, our charter school exposure credibly estimates the spillover effects. Second, no study has used national data to characterize the impacts of charter schools on students' outcomes to the best of our knowledge. Our analysis uses data that includes students born in over 500 counties and currently live in over 44 states. Third, we demonstrate the long-term effects of charter schools by including individuals in the mid-careers. About 70% of our sample is 30 years or above, enabling us to estimate charter schools' long-term impacts. Finally, we show evidence of the positive effects of charter schools on adverse health behaviors, which the literature has ignored.

Two limitations to our analysis are as follows. First, we do not know the actual county of birth for individuals born from 1980-84. We only know the counties they resided at their 12^{th} birthday. Although we can identify their locations before states passed the

laws, they could move earlier in anticipation of the policy and opening of charter schools. Second, our study covers about 25% of all counties and has a small sample size, making it computationally difficult to find statistically significant effects.

Overall, our results demonstrate that charter schools positively affect long-term education and health behaviors. Therefore, we recommend that states and local school boards should allow more charter schools to operate.

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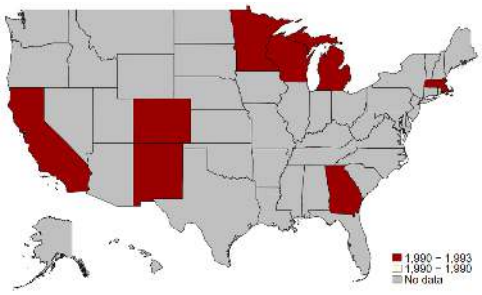
Figures



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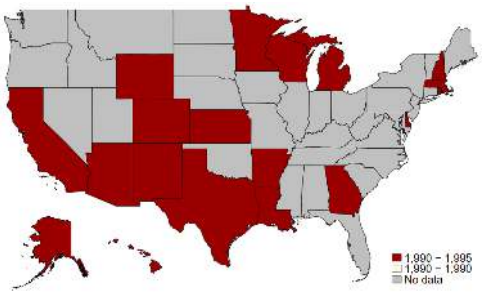

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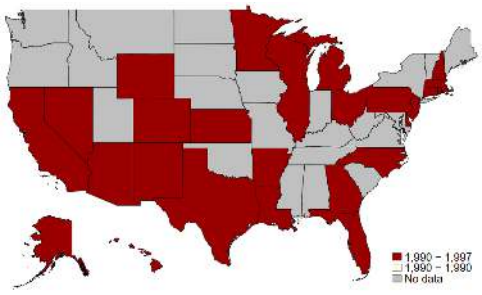

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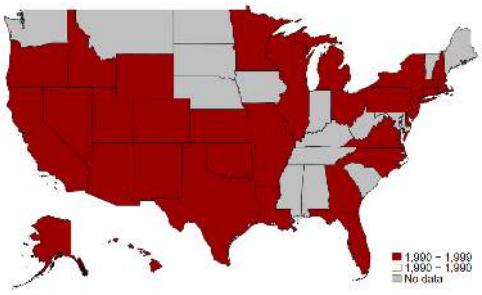

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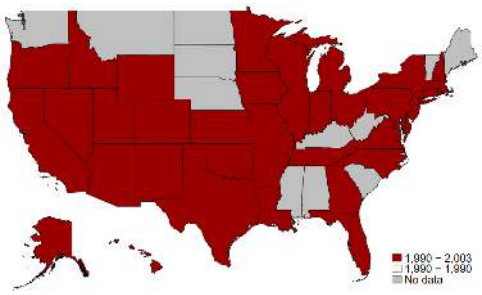


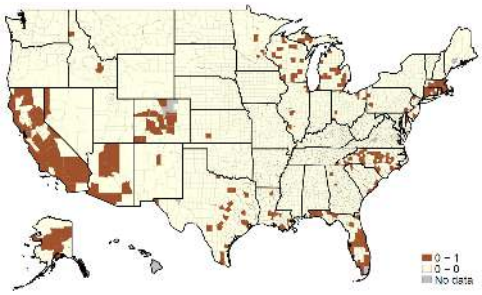
(e) 1992 - 2003

(f) 1992 - 2015

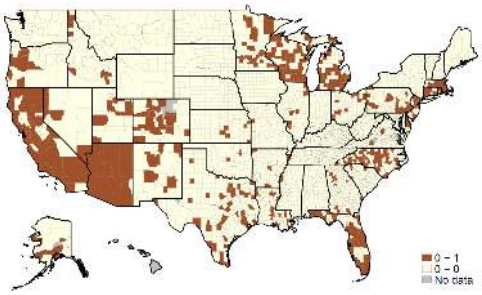
Figure 1. Timing of States Charter School Laws (1992 - 2015).

(a) 1992

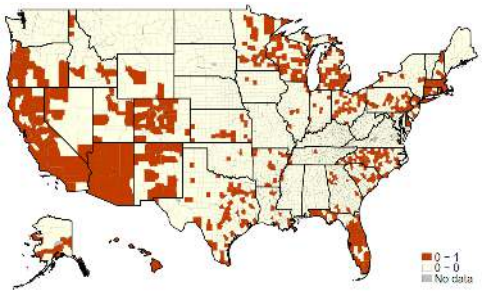
(b) 1995



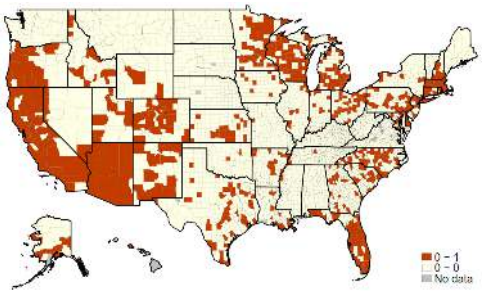
(c) 1998



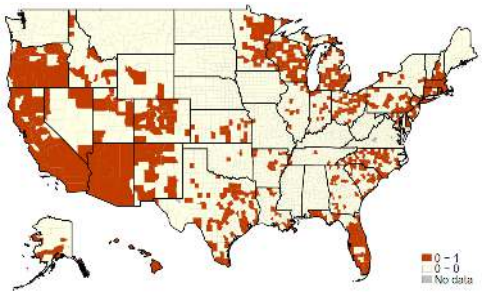
(d) 2001



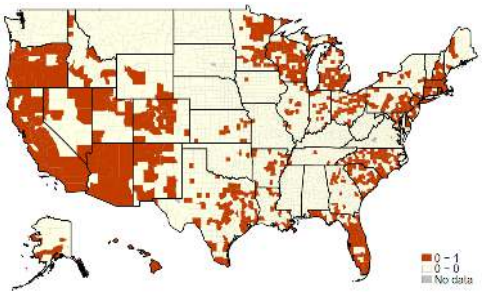
(e) 2004



(f) 2007



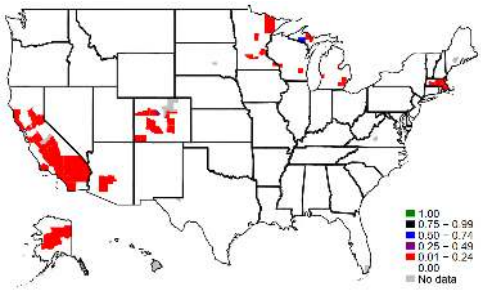
(g) 2010



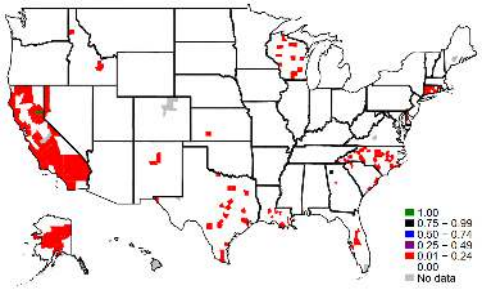
(h) 2013

Figure 2. Charter School Presence in Counties for Some Selected Years (1992 - 2013).

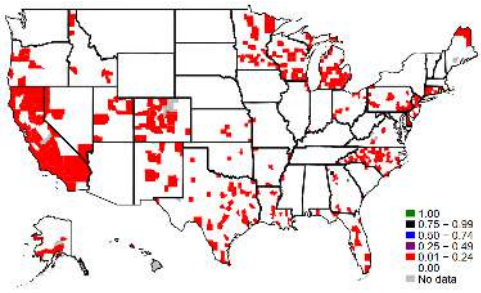
(a) 1992



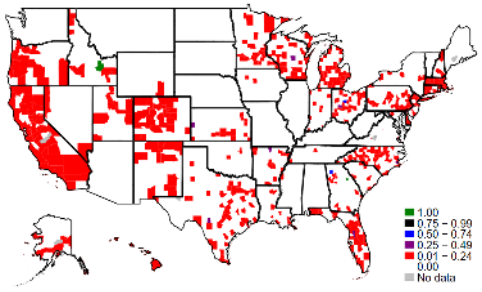
(b) 1995



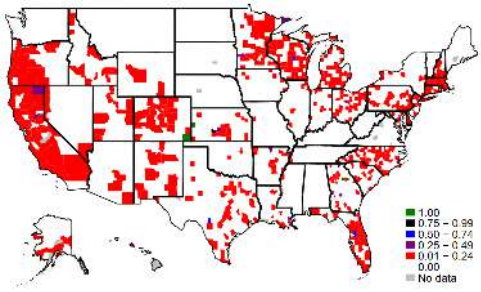
(c) 1998



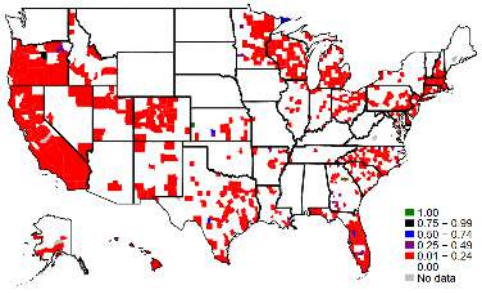
(d) 2001



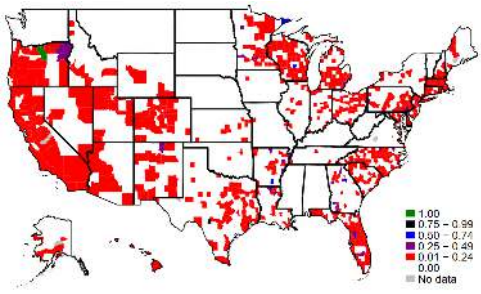
(e) 2004



(f) 2007



(g) 2010



(h) 2013

Figure 3. Charter School Coverage in Counties for Some Selected Years (1992 - 2013).

Tables

Table 1. Means and Standard Deviations (in Parenthesis) of Outcomes and Characteristics of Potentially Exposed Individuals

	Unweighted Means			Weighted Means	
	Full Sample	Not Exposed	Exposed	Not Exposed	Exposed
Outcomes					
Years of Schooling	13.25	13.27	13.24	13.37	13.45
	(2.589)	(2.651)	(2.539)	(2.524)	(2.352)
No High School Diploma	0.188	0.192	0.186	0.165	0.138
	(0.391)	(0.394)	(0.389)	(0.371)	(0.345)
High School Diploma	0.260	0.276	0.247	0.267	0.217
	(0.438)	(0.447)	(0.431)	(0.443)	(0.412)
Some College	0.295	0.278	0.309	0.299	0.331
	(0.456)	(0.448)	(0.462)	(0.458)	(0.471)
Four-Year College Completion	0.256	0.254	0.259	0.268	0.313
	(0.437)	(0.435)	(0.438)	(0.443)	(0.464)
Current smoking	0.342	0.332	0.307	0.350	0.335
	(0.475)	(0.471)	(0.461)	(0.477)	(0.472)
Binge Drinking	0.170	0.151	0.153	0.165	0.174
	(0.375)	(0.358)	(0.360)	(0.371)	(0.379)
Charter School Exposure					
Binary (0/1)	0.558				
	(0.497)				
Years [0,12]	3.116		5.583		6.022
	(3.754)		(3.388)		(3.613)
Coverage [0,1]	0.028		0.051		0.065
	(0.125)		(0.164)		(0.185)
Controls					
Black	0.308	0.329	0.292	0.169	0.164
	(0.462)	(0.470)	(0.455)	(0.374)	(0.370)
White	0.490	0.579	0.419	0.784	0.701
	(0.500)	(0.494)	(0.493)	(0.412)	(0.458)
Hispanic	0.202	0.092	0.289	0.048	0.135
	(0.401)	(0.289)	(0.453)	(0.213)	(0.342)
Male	0.509	0.510	0.507	0.527	0.525
	(0.500)	(0.500)	(0.500)	(0.499)	(0.499)
Birth County Known	0.608	0.569	0.638	0.670	0.752
	(0.488)	(0.495)	(0.481)	(0.470)	(0.432)
NLSY97 Cohort	0.601	0.627	0.580	0.475	0.398
	(0.490)	(0.484)	(0.494)	(0.499)	(0.490)
Observations	13,006	7,348	5,817	7,348	5,817

Table 2. First Stage Estimates - Effects of Charter School Exposure in the County of Birth on Exposure in the County of Residence

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Education Outcomes								
Years [0,12]	0.908***	0.888***	0.819***	0.818***				
	(0.010)	(0.013)	(0.026)	(0.026)				
Coverage [0,1]					0.755***	0.730***	0.707***	0.700***
					(0.037)	(0.043)	(0.046)	(0.041)
F-Statistic	7,704	4,490	995	990	422	287	238	290
Panel B: Binge Drinking Outcomes								
Years [0,12]	0.908***	0.890***	0.816***	0.816***				
	(0.010)	(0.013)	(0.028)	(0.029)				
Coverage [0,1]					0.734***	0.706***	0.687***	0.682***
					(0.041)	(0.047)	(0.051)	(0.047)
F-Statistic	7,724	4,568	822	809	317	221	182	203
Panel C: Current Cigarette Smoking Outcomes								
Years [0,12]	0.919***	0.907***	0.819***	0.819***				
	(0.010)	(0.011)	(0.030)	(0.030)				
Coverage [0,1]					0.758***	0.715***	0.703***	0.696***
					(0.037)	(0.044)	(0.047)	(0.044)
F-Statistic	9,347	6,440	768	753	413	262	220	251

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), race (Black, White, and Hispanic), a dummy for birth county availability, and NLSY cohort fixed effect. The specifications in columns (2) and (6), includes year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), we add control county time-varying characteristics by including poverty rate and unemployment rate. Sample sizes are as follows: education - 13,006, binge drinking - 12,482, and current cigarette smoking - 11,156. *p<.1, **p<.05, ***p<.01

Table 3. Second Stage Estimates for Education - Effects of Charter School Exposure on Years of Schooling, Four-Year College Graduation, and College Graduation Among High School Graduates using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Years of Schooling								
Years [0,12]	0.021**	0.028**	0.019	0.026				
	(0.011)	(0.013)	(0.025)	(0.025)				
Coverage [0,1]					-0.031	0.113	0.093	0.135
					(0.154)	(0.180)	(0.295)	(0.294)
Panel B: Four-Year College Graduation								
Years [0,12]	0.002	0.002	0.007*	0.008*				
	(0.002)	(0.002)	(0.004)	(0.004)				
Coverage [0,1]					0.053	0.069	0.101**	0.101**
					(0.042)	(0.046)	(0.048)	(0.051)
Panel C: Four-Year College Graduation among High School Graduates								
Years [0,12]	0.001	0.001	0.008*	0.010**				
	(0.002)	(0.002)	(0.005)	(0.005)				
Coverage [0,1]					0.045	0.100*	0.152***	0.148***
					(0.053)	(0.054)	(0.052)	(0.055)

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), race (Black, White, and Hispanic), a dummy for birth county availability, and NLSY cohort fixed effect. Additionally, for the specifications in columns (2) and (6), we add the year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. Sample sizes are as follows: years of schooling and four-year college graduation- 13,006 and four-year college graduation among high school graduates - 10,674. *p<.1, **p<.05, ***p<.01

Table 4. Heterogeneous Effects of Charter School Exposure on Four-Year College Graduation

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Four-Year College Graduation Among Males								
Years [0,12]	0.003	0.002	0.008*	0.008				
	(0.002)	(0.002)	(0.005)	(0.005)				
Coverage [0,1]					0.051	0.042	0.067	0.072
					(0.058)	(0.066)	(0.093)	(0.089)
Panel B: Four-Year College Graduation Among Females								
Years [0,12]	0.002	0.001	0.008	0.009				
	(0.003)	(0.003)	(0.006)	(0.007)				
Coverage [0,1]					0.063	0.108	0.186*	0.178*
					(0.083)	(0.085)	(0.099)	(0.101)
Panel C: Four-Year College Graduation Among Whites								
Years [0,12]	0.009***	0.009***	0.003	0.000				
	(0.003)	(0.003)	(0.006)	(0.006)				
Coverage [0,1]					0.219*	0.172	0.191	0.182
					(0.127)	(0.123)	(0.143)	(0.151)
Panel D: Four-Year College Graduation Among Nonwhites								
Years [0,12]	0.000	-0.001	0.006	0.009*				
	(0.002)	(0.002)	(0.004)	(0.005)				
Coverage [0,1]					0.035	0.059	0.096*	0.101*
					(0.046)	(0.050)	(0.054)	(0.057)

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female in Panels C and D), race (Black, White, and Hispanic in Panels A and B), a dummy for birth county availability, and NLSY cohort fixed effect. The specifications in columns (2) and (6) include year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. Sample sizes are as follows: males - 6,696, females - 6,469, Whites - 6,450, and Non-Whites - 6,715. *p<.1, **p<.05, ***p<.01

Table 5. Second Stage Estimates for Binge Drinking - Effects of Charter School Exposure on Alcohol Consumption ($\geq$ 5 bottles daily) using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Binge Drinking Among All Individuals								
Years [0,12]	-0.003***	-0.004***	0.002	0.002				
	(0.001)	(0.001)	(0.004)	(0.004)				
Coverage [0,1]					-0.075*	-0.075*	-0.05	-0.046
					(0.044)	(0.044)	(0.051)	(0.054)
Panel B: Binge Drinking Among Individuals of Ages 21+								
Years [0,12]	-0.003**	-0.004***	0.001	0.001				
	(0.001)	(0.001)	(0.004)	(0.004)				
Coverage [0,1]					-0.101**	-0.101**	-0.078	-0.073
					(0.043)	(0.043)	(0.051)	(0.054)

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), race (Black, White, and Hispanic), education (high school graduates, some college, and college graduates or better) a dummy for birth county availability, and NLSY cohort fixed effect. The specifications in columns (2) and (6) include year of survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. Sample sizes are as follows: all individuals - 12,482 and aged 21+ - 11,852. *p<.1, **p<.05, ***p<.01

Table 6. Heterogeneous Effects of Charter School Exposure on Binge Drinking

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Binge Drinking Among Males								
Years [0,12]	-0.001	-0.002	0.004	0.004				
	(0.001)	(0.001)	(0.004)	(0.004)				
Coverage [0,1]					-0.035	-0.096	-0.049	-0.046
					(0.049)	(0.072)	(0.081)	(0.083)
Panel B: Binge Drinking Among Females								
Years [0,12]	-0.004***	-0.006***	0.002	0.002				
	(0.002)	(0.002)	(0.007)	(0.007)				
Coverage [0,1]					-0.001	-0.043	-0.037	-0.035
					(0.046)	(0.045)	(0.051)	(0.054)
Panel C: Binge Drinking Among Whites								
Years [0,12]	-0.004***	-0.005***	0.001	0.001				
	(0.001)	(0.002)	(0.005)	(0.005)				
Coverage [0,1]					0.066	-0.034	0.042	0.037
					(0.066)	(0.076)	(0.082)	(0.082)
Panel D: Binge Drinking Among Nonwhites								
Years [0,12]	-0.000	-0.002	0.009*	0.008				
	(0.002)	(0.002)	(0.005)	(0.005)				
Coverage [0,1]					-0.039	-0.075	-0.04	-0.034
					(0.038)	(0.050)	(0.059)	(0.063)
Panel E: Binge Drinking Among Individuals Without College Degrees								
Years [0,12]	-0.004***	-0.006***	-0.000	-0.000				
	(0.001)	(0.001)	(0.005)	(0.005)				
Coverage [0,1]					-0.014	-0.022	-0.000	-0.001
					(0.044)	(0.051)	(0.059)	(0.062)

Continued on the next page

Table 6 − Continued

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel F: Binge Drinking Among Individuals With College Degrees								
Years [0,12]	0.002	-0.002	0.009*	0.007				
	(0.002)	(0.002)	(0.005)	(0.005)				
Coverage [0,1]					-0.043	-0.213***	-0.220***	-0.174**
					(0.038)	(0.067)	(0.077)	(0.089)

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), race (White and Non-Whites), a dummy for birth county availability, and NLSY cohort fixed effect. The specifications in columns (2) and (6) gender (male/female in Panels C-F), race (Black, White, and Hispanic in Panels A, B, E, and F), education (high school graduates, some college, and college graduates or better in Panels A- D) a dummy for birth county availability and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county time-varying characteristics by including poverty rate and unemployment rate. Sample sizes are as follows: males - 6,391, females - 6,085, Whites - 6,218, and Non-Whites - 6,264, four-year college graduates - 3,377, and non-college graduates - 9,102. *p<.1, **p<.05, ***p<.01

Table 7. Second Stage Estimates for Current Cigarette Smoking - Effects of Charter School Exposure on 30-Day Smoking using Exposure at the County of Birth as an Instrument

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years [0,12]	-0.003*	-0.006***	-0.009*	-0.009*				
	(0.002)	(0.002)	(0.005)	(0.005)				
Coverage [0,1]					0.029	-0.045	0.037	0.057
					(0.067)	(0.088)	(0.103)	(0.111)

Notes: Each estimate comes from a separate regression. The regressions include gender (male/female), dummies for Whites and Blacks, education (high school graduates, some college, and college graduates or better), a dummy for birth county availability, and NLSY cohort fixed effect. The specifications in columns (2) and (6) include survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county and state time-varying characteristics by including poverty rate, unemployment rate, and Cigarette Prices. The sample size is 11,156. *p<.1, **p<.05, ***p<.01

Table 8. Heterogeneous Effects of Charter School Exposure on Current Cigarette Smoking

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Current Cigarette Smoking Among Males								
Years [0,12]	-0.003	-0.007***	-0.006	-0.005				
	(0.002)	(0.003)	(0.008)	(0.008)				
Coverage [0,1]					0.046	-0.022	0.141	0.161
					(0.096)	(0.139)	(0.122)	(0.130)
Panel B: Current Cigarette Smoking Among Females								
Years [0,12]	-0.003	-0.004	-0.009	-0.009				
	(0.003)	(0.003)	(0.007)	(0.007)				
Coverage [0,1]					0.031	-0.045	-0.063	-0.045
					(0.071)	(0.091)	(0.119)	(0.127)
Panel C: Current Cigarette Smoking Among Whites								
Years [0,12]	-0.003	-0.003	-0.006	-0.005				
	(0.002)	(0.003)	(0.007)	(0.008)				
Coverage [0,1]					0.060	-0.005	-0.049	-0.032
					(0.150)	(0.161)	(0.217)	(0.217)
Panel D: Current Cigarette Smoking Among Nonwhites								
Years [0,12]	-0.002	-0.006**	-0.012*	-0.012*				
	(0.002)	(0.003)	(0.007)	(0.007)				
Coverage [0,1]					0.025	-0.065	0.042	0.065
					(0.066)	(0.094)	(0.106)	(0.115)
Panel E: Current Smoking Among High-Educated Individuals								
Years [0,12]	-0.003	-0.005*	-0.014**	-0.014**				
	(0.002)	(0.002)	(0.007)	(0.007)				
Coverage [0,1]					-0.076	-0.144**	-0.098	-0.082
					(0.053)	(0.068)	(0.089)	(0.095)

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Table 8 – Continued

	Years of Charter School Exposure in County of Residence [0,12]				Proportion of Students in Charter School in County of Residence [0,1]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel F: Current Cigarette Smoking Among Low-Educated Individuals								
Years [0,12]	-0.003	-0.007***	-0.000	0.000				
	(0.003)	(0.003)	(0.008)	(0.008)				
Coverage [0,1]					0.212	0.121	0.229	0.255
					(0.142)	(0.203)	(0.201)	(0.212)

Notes: High education is defined as individuals with associate degrees or better, while low-education refers to individual without at least associate degrees. Each estimate comes from a separate regression. The regressions include gender (male/female in Panel C - F), dummies for Whites and Blacks (in Panel A, B, E, and F), education (high school graduates, some college, and college graduates or better in Panels A - D), a dummy for birth county availability, and NLSY cohort fixed effect. The specifications in columns (2) and (6) include survey and birth cohort fixed effects. In columns (3) and (7), we also add county fixed effects. Finally, for the results in columns (4) and (8), the specifications control county and state time-varying characteristics by including poverty rate, unemployment rate, and Cigarette Prices. Sample sizes are as follows: males - 5,846, females - 5,304, Whites - 5,650, and Non-Whites - 5,500, four-year college graduates - 5,843, and non-college graduates - 5,307. *p<.1, **p<.05, ***p<.01

Appendix A

Additional Tables

Table A1. Dates of State Charter Law and Regulations on the Number of Charter Schools Permitted

State	Law Year	Cap	Description of Cap
Alabama	2015	Yes	10 until 2022
Alaska	1995	No	
Arizona	1994	No	
Arkansas	1995	Yes	Approve up to 24 per year
California	1992	Yes	250 in 1998/1999 and increased by 100 annually
Colorado	1993	No	
Connecticut	1996	No	
Delaware	1995	No	
DC	1996	Yes	10 per year per authorizer. If an authorizer has not reached 10, another authorizer can grant up to 20
Florida	1996	No	
Georgia	1993	No	
Hawaii	1994	No	
Idaho	1998	No	
Illinois	1996	Yes	120 but 70 in Chicago with at least 5 for students from low-performing or overcrowded schools. No more than 45 will operate in the rest of the state
Indiana	2001	No	
Iowa	2002	No	But not more than 10 innovation zone applications
Kansas	1994	No	
Kentucky	NA	No	
Louisiana	1995	No	
Maine	2011	Yes	10 until 2022
Maryland	2003	No	
Massachusetts	1993	Yes	120. Up to 48 reserved for Horace Mann charter & schools up to 72 reserved for commonwealth charter school not including charter schools in low-performing school districts. At least 2 charter schools must be in the lowest 10%.
Michigan	1993	Yes	15
Minnesota	1991	No	
Mississippi	2010	Yes	15 per year but expires in 5 years
Missouri	1998	No	But limited to certain areas
Nevada	1997	No	
New Hampshire	1995	No	
New Jersey	1996	No	
New Mexico	1993	Yes	75 schools in any 5-year period. Not more than 15 opened per year

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Table 1 – Continued from the previous page

State	Law Year	Cap	Description of Cap
New York	1998	Yes	460 and not more than 50 charters issued after July 1, 2015 can be granted for schools located in a city with a population of 1 million or more.
Ohio	1997	Yes	100 schools per authorizer and its DOE approving up to 20 schools per year
Oklahoma	1999	Yes	Up to 5 if county pop $<$ 500K and no more than 1 per year in single school district.
Oregon	1999	No	
Pennsylvania	1997	No	
Rhode Island	1995	Yes	35
South Carolina	1996	No	
Tennessee	2002	No	
Texas	1996	Yes	305 new per year starting 2019
Utah	1998	No	
Virginia	1998	No	
Washington	2012	Yes	A maximum of 40 charter schools may be established over a 5-year period, starting in 2016. Not more than 8 per year
Wisconsin	1993	Yes	Cap on the number that an authorizer may oversee
Montana	NA		
North Dakota	NA		
South Dakota	NA		
Nebraska	NA		
Kentucky	NA		
West Virginia	NA		
Vermont	NA		

Appendix B

Methodological Challenges

The positive effects of charter schools on school competition may imply they are "skimming the cream," taking only the best students from the TPSs. They could also signal parents looking for alternatives. On the other hand, a negative effect could suggest that poor-performing students transfer to charter schools, searching for better schools. Suppose charter schools increase competition, which eventually increases the outcomes of the students in their jurisdiction. In that case, comparing charter schools and TPSs students will not capture the charter schools' full effects if the initial distribution of performance is unknown. The presence of school competition could shift the distribution of students' performance. Therefore, understanding students' performance distribution for charter schools and TPSs students is essential for a valid comparison.[23] However, a common concern in most studies in the literature is that they do not have data on pre-treatment outcomes. Without such data, it is challenging to disentangle the charter schools impacts on students' achievements. Therefore, the mixed evidence of the impact of attending charter schools on students' outcomes can mislead policymakers because the results across different studies may be incomparable due to data limitations. Our study uses empirical strategy that does not require students' pre-treatment outcomes, as discussed in Section 5.

Identifying the impact of charter schools on the students' outcomes is not an easy task due to possible endogeneity. First, charter school attendance is not by chance. Parents seek a better alternative for their children, especially those who are already struggling in the TPSs. The fact that students and parents choose charter schools over TPSs suggests that there are inherent reasons which make them seek for alternatives. Also, students who choose charter schools may be having difficulties regarding their performances in the TPSs. Without data on students' initial performance before transferring into charter

[23]The reason is that charter schools may be attracting students from one side of the ability distribution. If only students struggling in the TPSs transfer to the charter schools, we would expect students' average quality in charter schools to be lower than those who remain in the TPSs. Making a comparison based on average performance leads to an underestimation of charter school effects. The converse is also true.

schools, it is challenging to identify charter schools' effects. Hence, unobserved characteristics, including students' ability and preferences, can lead to self-selection into charter schools. Second, the decision by a state to pass a charter school law may be endogenous since states with good and highly competitive school systems are less likely to implement the charter law, whereas states whose school system is less competitive would be willing to pass the law. Even if states do not endogenously pass the laws,[24] school districts and local school boards may endogenously open charter schools. Third, different charter schools may face different rules depending on the state, county, or school district's preferences. Fourth, charter schools may differ from each other based on curricular, teaching styles, teachers, and resources available. Finally, different charter schools may serve different communities or students, which can lead to different outcomes.

Studies that focus on charter school attendance and students' outcomes use different methods to address endogeneity issues. They use both experimental and quasi-experimental methods. The ideal scenario is to conduct an experiment where one-half of the student is randomly assigned to charter schools. The random assignment can identify the average treatment effect of charter school attendance. Unfortunately, conducting such an experiment is not feasible because of ethical concerns. (School choice is voluntary, and it is illegal to force students to attend schools unwillingly). One natural experiment similar to random assignment is using a lottery to assign students in oversubscribed charter schools. Students who lose in the lottery and remain in TPSs or private schools become the control group, and those who win the lottery form the treatment group. Focusing on students who subscribe to charter schools only becomes analogous to the experiment described earlier. These are known in the literature as "lottery-based" studies.

Dobbie and Fryer Jr (2015) is a lottery-based study that followed applicants to a charter middle school in the Harlem Children's Zone (New York). Similarly, Angrist et al. (2016) lottery-based study considered only oversubscribed Boston (Massachusetts) charters. Despite the experimental nature of these studies, they have limitations. First,

[24] This reason is that charter schools were established to provide alternatives to the TPSs and create competition. For example, Hoxby (2004) examines the impact of charter schools by examining the changes in mean test scores before and after introducing charter schools. The findings are that schools exposed to charter school competition have more enormous improvements in an average performance in terms of test scores than schools not exposed to a significant charter school competition.

conditions that lead to oversubscription is neither universal nor random since only older schools and academically better schools get oversubscription (Davis and Raymond, 2012). Second, the rate of oversubscription differs across charter schools. Whereas one school may have a larger subscription rate, others may have a lower subscription rate. Third, students may enroll in a non-TPS because of losing the lottery. If the data for these studies do not reflect students who are home-schooled or move to private schools after losing the lottery, the results can be biased. Fourth, these studies could include only oversubscribed schools. But there are many charter schools without oversubscription. Hence, a generalization of findings from lottery-based studies is problematic, and their findings' external validity is concerning.

An alternative to experimentally designed (or "lottery-based") studies is the quasi-experimental studies. These studies choose methods that can solve the endogeneity issues associated with survey data. Among these studies, the predominantly used method is the student-level fixed effects model. They use panel data to account for the students' pre-treatment outcomes and remove time-invariant observed and unobserved characteristics. The net effect is the "value-added" from attending charter schools. Studies such as Imberman (2011); Booker et al. (2007, 2008); Ni (2009); Zimmer et al. (2012); Jinnai (2014); Winters (2012); Bifulco and Ladd (2006); Zimmer et al. (2009), have implemented this approach. A few of their limitations are as follows. First, there may be demographically identifiable differences between students who always remain in charter schools and TPSs as noted in Davis and Raymond (2012). Second, the research design requires students to move from TPSs to charter schools and eliminates a sample of students who never switched schools. Third, these studies may have smaller sample sizes due to the nature of the design.

Other methods that are predominantly found in the literature are the matching method (Sass et al., 2016; Dobbie and Fryer, 2020; Hoxby, 2004), instrumental variable (IV) approach (Imberman, 2011; Bettinger, 2005), and difference-in-difference (Bettinger, 2005). However, all these methods have issues as well.