Annenberg Institute
BROWN UNIVERSITY

EdWorkingPaper No. 26-1439

The Expansion of Alternative Schools: Impact of Schools Targeting Lower Performing Students

Farah Mallah
Stanford University

Nour Abdul-Razzak
University of Chicago

Monica Bhatt
University of Chicago

Despite rising high school graduation rates in the US, a substantial portion of students do not obtain a high school degree. Alternative schools have emerged as a potential solution offering opportunities for credit recovery and flexible scheduling. Using variation in the timing and proximity of alternative school expansions in Chicago, we find that living within a mile of an alternative school increases a students' likelihood of enrolling in the school by 28%. We find evidence that while alternative schools may boost high school enrollment and decrease total arrests, they also decrease students' likelihood of enrolling in college.

VERSION: April 2026

Suggested citation: Mallah, Farah, Nour Abdul-Razzak, and Monica Bhatt. (2026). The Expansion of Alternative Schools: Impact of Schools Targeting Lower Performing Students. (EdWorkingPaper: 26-1439). Retrieved from Annenberg Institute at Brown University: https://doi.org/10.26300/c4kv-v948

The Expansion of Alternative Schools: Impact of Schools Targeting Lower Performing Students

Farah Mallah*[1], Nour J Abdul-Razzak†[2], and Monica Bhatt‡[2]

[1]Stanford University

[2]University of Chicago

March 27, 2026

Abstract

Despite rising high school graduation rates in the US, a substantial portion of students do not obtain a high school degree. Alternative schools have emerged as a potential solution offering opportunities for credit recovery and flexible scheduling. Using variation in the timing and proximity of alternative school expansions in Chicago, we find that living within a mile of an alternative school increases a student's likelihood of enrolling in the school by 28%. We find evidence that while alternative schools may boost high school enrollment and decrease total arrests, they also decrease students' likelihood of enrolling in college.

1 Introduction

High school graduation rates in the United States have increased dramatically over the past two decades. In Chicago, the four-year graduation rate rose from 43% in 2002 to 83% in 2022.[1] Yet millions of students nationwide still do not graduate each year (Brough, Phillips, and Turner, 2024). This is a critical issue given the substantial wage premium associated with obtaining a high school diploma (Angrist and Krueger, 1991; Oreopoulos, 2006; Autor, Goldin, and Katz, 2020). For students facing barriers to high school graduation, alternative schools have emerged as a promising intervention, offering flexible schedules, credit recovery, and

*farahmallah@standord.edu

†abdulrazzak@uchicago.edu

‡mbhatt@uchicago.edu

[1]https://www.cps.edu/

other kinds of individualized supports. Alternative schools designed for students who have been underserved or disconnected from traditional schooling now represent a growing segment of public education in the United States. Nationally, nearly one million students attend alternative schools across 48 states (Lehr, Tan, and Ysseldyke, 2009; Lehr and Lange, 2003).[2] Chicago exemplifies this trend: by 2019, 10% of the city's high school population was enrolled in one of its 38 alternative schools. Despite this expansion, rigorous evidence on the effectiveness of alternative schools in improving graduation rates and other outcomes remains limited.

Alternative schools are often viewed as "last chance" options for students at high risk of dropping out—often students with low academic performance, chronic absenteeism, criminal justice involvement, and/or caregiving responsibilities. Their expected impacts are ambiguous. Alternative schools may improve student-school match through flexible structures and targeted supports, consistent with evidence on tracking and instructional alignment (Duflo, Dupas, and Kremer, 2011). They often operate with smaller classes and more individualized instruction, features linked to improved outcomes (Angrist and Lavy, 1999; Chetty et al., 2011). On the other hand, they may concentrate high-need students, potentially amplifying negative peer effects and straining resources. There is no strong evidence for negative peer effects in the school tracking literature (e.g., Duflo, Dupas, and Kremer, 2011; Collins and Gan, 2013), but with a particularly high-need group of students, some peer effects may play a role in student outcomes. Traditional administrators may also divert students with high rates of disciplinary infractions to alternative schools irrespective of efficacy. These competing mechanisms underscore the need for credible causal evidence.

Chicago has experienced a significant and unprecedented increase in the number of alternative schools, as shown in Figure 1. Within just a few years, the share of high school students enrolled in an alternative school doubled.[3] We estimate the causal effects of alternative schools on graduation, school persistence, college enrollment, and arrests. To address selection, we exploit variation in the timing and geographic proximity of openings, comparing cohorts differentially exposed due to when schools open and where students live. In our main identification strategy, we compare the change in the outcome for students who are within a 1-mile radius of an alternative school during their high school years (grades 9-13) to students who would have been within a 1-mile radius of an alternative school had they been in a different cohort, relative to the change in the outcomes for students who are never within a 1-mile radius of an alternative school, controlling for student baseline characteristics, grade 9 entry school and including cohort fixed effects.

[2]Based on a survey of nationally representative districts, NCES estimates the number of students served by alternative schools to be closer to 646,500. Lehr, Tan, and Ysseldyke (2009) base their estimate on a national survey of state officials.

[3]We focus on a subset of alternative schools for which we have complete data, excluding those operated by Youth Connection Charter Schools (YCCS). YCCS campuses have been part of Chicago's landscape for decades but only appear in CPS administrative data after 2013. Because our design exploits the timing of openings and students' distance to newly opened campuses, we exclude YCCS schools from our analysis. We include the following three types of schools: Contract or Charter (not YCCS) schools (8 schools), Alternative Learning Opportunities Programs (ALOPs) (14 schools), and CPS-run alternative schools that are not juvenile detention centers or prison (4 schools).

Students living within one mile of an alternative school during high school are 1.7 percentage points more likely to enroll in an alternative school, relative to a 6% baseline, with larger effects among lower-performing ninth graders. Exposure increases school persistence by roughly 1 percentage point per year. We find suggestive evidence that students may be more likely to graduate high school; we see a marginally statistically significant 0.79 percentage-point increase in graduation rates among those exposed to alternative schools. This is the equivalent of over 1,500 students graduating, though this effect is not robust to different analytic considerations and should be interpreted with caution. The impact on non-high school outcomes is mixed. Residing close to an alternative school decreases students' college enrollment likelihood by between 0.4 and 1.8 percentage points (a 0.8 to 3.7% decrease relative to the control mean).[4] At the same time, exposure reduces total arrests by about 0.06 over six to eight years (a 7% decline), amounting to approximately 3.5 fewer arrests among those ever arrested. These findings present a nuanced picture of the impact of alternative schools: alternative schools appear to keep students in school and reduce criminal justice involvement, with weaker evidence of gains in graduation and declines in college-going.

The expansion of alternative schools may also pose additional spillover effects on students who remain within the traditional school system. The literature on the expansion of charter schools suggests that the increase in access to other schooling options increases sorting (e.g., Kim, 2018; Monarrez, Kisida, and Chingos, 2022). Mumma (2022) finds that a charter school opening within a 2-mile radius decreased enrollment in traditional public schools, but had no effect on student academic achievement. In this case, given the high levels of out-of-school suspension alternative school students tend to have in grade 9 (Table 1), these students may be particularly disruptive to instruction and could have larger negative peer effects. Carrell, Hoekstra, and Kuka (2018) find exposure to a disruptive peer in elementary school reduces college enrollment by 1.4 percent and earnings by 3 percent. As such, we might expect that alternative school openings may have a positive spillover on students who remain in public schools even with a small sorting margin.

We find that proximity to more alternative schools is associated with a reduction in total arrests among students attending neighboring traditional public schools. The decline in arrests appears to be driven by students who remain enrolled in the traditional public schools, suggesting that the presence of alternative schools may generate positive spillover effects on traditional public schools, potentially by decreasing the share of students who may have had negative peer effects on traditional public school students.

This study contributes new causal evidence to a literature dominated by descriptive and qualitative analyses (Lehr, Tan, and Ysseldyke, 2009; Ruzzi and Kraemer, 2006). Two prior studies are particularly relevant.

[4]The 0.4–1.8 percentage-point range reflects different distance cutoffs. The estimated decline is larger and statistically significant at the 5% level for students living within one mile; estimates are smaller and not significant when including those living at or beyond one mile of an alternative school.

Dynarski and Wood (1997) randomly assigned 912 students to three alternative schools in Stockton, Wichita, and Cincinnati, finding improved graduation and modest test score gains in two schools but no benefits in the third, and no impacts on college, employment, or arrests overall. We extend their work by studying a large-scale, district-wide expansion and by examining spillovers in a setting without formal referrals, which allows us to explore broader spillover effects on traditional public schools. Brough, Phillips, and Turner (2024) evaluate adult-serving reengagement schools for students over age 18 who have already dropped out, While this paper examines alternative schools aimed at preventing dropout among school-aged students. This distinction is critical, as the counterfactual in Brough et al. (2024) is obtaining a GED or having no credential, whereas in this paper, the counterfactual may involve remaining enrolled in a traditional public school. By focusing on younger populations and preventative interventions, this study complements and broadens the existing literature on alternative education.

Our findings also speak to the broader literature on school choice and tracking. Alternative schools introduce horizontal differentiation (flexible schedules, tailored environments) and vertical differentiation (targeting students lower on the academic distribution) in contrast to magnet and private schools that target higher achieving students (Nechyba, 2006). Prior work suggests that expanding choice can increase sorting by test scores and demographics (Kim, 2018; Monarrez, Kisida, and Chingos, 2022) and can alter enrollment in nearby traditional schools without clear achievement effects (Mumma, 2022). By focusing on an expansion targeted to high-need students, we provide new evidence on the impact of increased school heterogeneity on students' academic and behavioral outcomes and the peer environment in traditional schools. Relatedly, we complement the tracking literature—largely concentrated on within-school tracking practices (Figlio and Page, 2001; Duflo, Dupas, and Kremer, 2011; Collins and Gan, 2013; Figlio and Ozek, 2023)—by evaluating the impact of alternative schools that are meant to serve students who experience multiple barriers to graduation in a separate school rather than in the same school, but a different classroom.

This paper is organized as follows. Section 2 provides background on alternative schools, both nationally and in Chicago. Section 3 describes the data sources and sample construction. Section 4 details the identification strategy. Section 5 presents the main results and robustness checks. Section 6 examines the spillover effects of alternative schools on neighboring traditional schools. Finally, Section 7 concludes by summarizing the findings and discussing their implications.

2 Background: Alternative Schools

A. National Patterns

Although alternative schools vary in how they serve students locally and nationally, they are consistently intended to serve students who are at risk of dropping out of the traditional school system. The US Department of Education defines alternative schools as:

> A public elementary/secondary school that: 1) addresses needs of students that typically cannot be met in a regular school, 2) provides nontraditional education, 3) serves as an adjunct to a regular school, or 4) falls outside of the categories of regular, special education, or vocational education. (U.S. Department of Education, 2002, p. 55-56)

Prior research by Raywid (1994) grouped alternative schools into three categories that could be useful for understanding the variation in school types. In type I schools, students enroll in the alternative school by choice. Type I schools are similar to magnet schools in that they employ innovative programs and methods to attract students. Type II schools are schools where students are placed because of disciplinary reasons. Placement in Type II schools tends to be the last step before expulsion.[5] Type III schools provide mainly remedial programs to address academic and/or socio-emotional issues. Enrollment in Type II and III schools tends to be mandatory, while enrollment in Type I schools tends to be by choice. Schools may recommend Type I schools to students who may have disciplinary or attendance issues, but attending these schools instead of a traditional public school tends to be the student's choice. In this paper, we focus on Type I alternative schools in Chicago since we are interested in schools where enrollment is by choice as type I schools may compete for students with traditional public schools. Importantly, for students who enroll in Type I alternative schools, enrollment in traditional public schools is a possible counterfactual.

In 2007-2008, the National Center for Education Statistics (NCES) conducted a nationally representative survey of alternative school programs. They found that 64% of the districts had at least one alternative school or program and 63% of the alternative schools and programs were housed in a separate facility (outside regular schools). Based on their estimate, 646,500 students attended alternative schools and programs for at-risk students nationally in 2007-08.[6] In many districts, the demand to enroll in an alternative school tends to be greater than the supply of alternative schools. One-third of the districts that had a district-run alternative school and/or program reported being unable to enroll new students in alternative schools and programs due to staffing or space limitations.

Districts often vary in the type of alternative schools offered and how students end up enrolled in these schools. Districts that have alternative schools and/or programs said that student placement is often based on

[5] In Chicago, these schools are called Safe schools. We exclude these schools in our analysis because enrollment in them is not by choice, it is mandatory.

[6] Based on another state-level survey of key informants on alternative schools, the number of students estimated to be enrolled in an alternative school summed across 18 states was more than 1 million students in 2001-02. (Lehr et al., 2022)

recommendations from regular school staff (75%), on a committee of teachers, administrators and counselors (71%), on district-level administrators (54%), on parent request (48%) and on student request (41%).

B. Chicago

According to Illinois' legislation on alternative programs, alternative schools and programs' intended target group are defined as follows:[7]

> [Alternative learning opportunities programs] are intended to provide students at risk of academic failure with the education and support services needed to meet Illinois Learning Standards and to complete their education in an orderly, safe, and secure learning environment...An alternative learning opportunities program shall provide a flexible standards-based learning environment, innovative and varied instructional strategies, a student-centered curriculum, social programs, and supplemental social, health, and support services to improve the educational achievement of students at risk of academic failure. (Alternative Learning Opportunities Law, ILCS)

The expansion of alternative schools in Chicago Public Schools (CPS) was initially driven by efforts to reengage students at risk of dropping out by providing them with an alternative path to graduate high school. According to CPS, alternative schools—also called Options schools—are designed to offer flexible schedules for students with work obligations, parenting responsibilities, criminal justice system involvement, or financial constraints. These schools also provide additional supports and services for students who have left traditional public schools or fallen behind on credits but seek to graduate.

There is considerable heterogeneity in the governance structure and setup of CPS Options schools. CPS operates five types of alternative schools: 1) Contract or Charter (not YCCS) schools (8 schools), 2) Alternative Learning Opportunities Programs (ALOPs) (14 schools), 3) Youth Connection Charter Schools (YCCS) (20 schools), 4) CPS-run alternative schools (6 schools), two of which operate within juvenile detention centers or prison, and 5) SAFE schools (5 schools). We describe contract, ALOP, and the two CPS non-facility-based alternative schools below, as they are included in our analytic sample. In our analysis, we exclude YCCS charter schools, the two schools that operate in-facility such as the detention center and prison, and SAFE schools, which are temporary safe harbor schools for students facing acute security risks in their neighborhoods. These schools are excluded because we cannot use our identification strategy to exploit timing of opening and distance to school to generate a causal estimate.

The amenities and structure of alternative schools in CPS vary considerably. Some alternative schools

[7] In 2022 an amendment was added to the Statutes that limits school student transfers to alternative programs for disciplinary reasons, requiring a formal written notice and a school board hearing.

closely resemble traditional public schools. These schools are usually contract alternative schools operated by external providers. Contract alternative schools typically maintain standard school schedules and offer extracurricular activities, including sports (UChicago Education Lab Report, 2021). In terms of the physical school space and type of student supports, contract alternative schools are largely indistinguishable from traditional public schools. The primary distinction from traditional public schools is in their academic model: contract alternative schools provide accelerated pathways that allow students to earn more credits per year and graduate in less time than in a traditional school.

Other alternative schools called Alternative Learning Opportunities Programs (ALOP) differ from traditional alternative schools in their heavy reliance on technology and self-paced, computer-based learning models. These programs offer students greater flexibility, often operating in nontraditional physical spaces (e.g., strip mall buildings) with limited capacity to accommodate all enrolled students simultaneously. ALOP schools provide flexible scheduling, allowing students to attend either morning or afternoon sessions for four hours per day, five days per week. They offer few extracurricular activities and limited social-emotional support, such as counseling services. ALOP schools primarily serve students who require flexible schedules due to employment or caregiving responsibilities.

CPS central office directly runs four alternative schools themselves without contracts or charters. Two—Nancy B. Jefferson and York—are located in-facility in the juvenile detention center and prison and are excluded from our analysis. Two others - Simpson and Peace and Education - are alternative schools opened for specific populations of students in 2007. Simpson High School was developed to serve female students who were pregnant or mothers. Peace and Education was developed as an arts education alternative school.

The number of students enrolled in an alternative school increased by 41% between 2013 and 2019 and that increase was completely driven by the expansion of enrollment in alternative schools through contract and ALOP mechanisms. In 2016—at the peak of alternative school expansion—CPS had 22 charter alternative schools, 6 contract alternative schools, 13 ALOP schools, and two CPS run alternative schools.[8] This study focuses on the expansion of contract and ALOP alternative schools only. We exclude Youth Connection Charter Schools (YCCS), which constitute the majority of charter alternative schools, CPS-run alternative schools that operate in juvenile detention and Cook County Sherriff's office facilities, and SAFE schools, as we cannot use distance as an instrument to identify the effect of attending these schools.[9] In 2016, 23 out of 43 alternative schools met the criteria for inclusion in our study. Our analysis focuses on these 23 schools.

[8]These numbers exclude two CPS-run alternative schools that are run in the juvenile detention center and prison, two SAFE school campuses, and the YCCS charter schools.

[9]In 2016, YCCS operated 20 of the 22 charter alternative schools in Chicago, serving approximately 41% of alternative school students. These campuses existed before the analysis period (2009) and were not part of the alternative school expansion. YCCS schools did not consistently report data to CPS until after 2013, making them unsuitable for inclusion in our dataset.

Of these, 13 are ALOP schools, while the remaining 10 are contract schools that follow a more traditional school structure. Going forward, when referring to alternative schools we are referring to alternative schools that are *not* operated by YCCS, are not SAFE schools, and are not CPS-run in-facility schools.

The number of alternative schools grew rapidly between 2010 and 2015, doubling between 2013 and 2014. After 2015, the expansion plateaued, with four schools closing between 2015 and 2019 (Figure 1, panel a). Similarly, the proportion of high school students enrolled in these alternative schools increased from near zero to approximately 7% (Figure 1, panel b). The new alternative schools were geographically dispersed throughout the city (Figure 2).

Enrollment in alternative schools is driven by various potential factors. Some students may choose alternative schools based on peer recommendations, attracted by the flexible schedules and smaller class sizes. Conversations with both CPS staff and facilitators who work closely with both alternative schools and traditional schools, suggest that students may be informally "pushed out" of their original schools. Schools may advise students—particularly those struggling academically or exhibiting behavioral issues—that an alternative school is a better fit. While formal expulsion policies prevent involuntary transfers, such conversations can strongly influence student decisions.

Patterns in the data support concerns that schools may be informally pushing out students with higher disciplinary and arrest records. Table A1 shows that, conditional on test scores and attendance, students with higher rates of in-school and out-of-school suspensions, as well as prior arrests, are more likely to enroll in alternative schools. That said, as shown in Tables A2 and A3 the primary predictors of both alternative school enrollment and dropout risk are similar: the number of failed courses, GPA, and absenteeism.[10] This suggests that ultimately, students who are at risk of dropping out because of academic performance and attendance are those most likely to attend an alternative school.

3 Data and Sample

A. Data Source

To test these hypotheses, we use administrative panel data at the student level maintained by Chicago Public Schools covering all years from 2009 to 2019. These data include detailed student enrollment, student test scores, GPA, attendance, student home addresses, disciplinary record, graduation, and college enrollment. We also use Chicago's Police Department data (1999-2019) to control for arrest records prior to grade

[10]Predictor rankings are based on a random forest model with 200 iterations using bagging. Variable importance is assessed via the difference in mean squared error (MSE) between models that include and exclude the covariate. These three variables significantly improved MSE in predicting both dropout risk and alternative school enrollment.

9 (baseline), as well as to identify the impact of alternative schools on future arrests.

B. Sample

We analyze six cohorts of students (2009–2014) determined by the academic year in which they are first observed enrolled in grade 9. For example, students entering grade 9 in the 2008–09 school year belong to the 2009 cohort. We observe students in the 2009 through 2012 cohorts for eight years. Students in the 2013 and 2014 cohorts are observed for seven and six years. Our estimation strategy will use both cross-sectional and panel data for these students depending on our strategy and outcome. Our sample excludes students who enter Chicago Public Schools (CPS) at grades 10 or 11 because we use grade 9 data to control for students' baseline characteristics.

Student baseline characteristics are based on their entry year to grade 9 and include grade 9 GPA, number of courses failed, member days, absent days, homelessness status (indicated as Students in Temporary Living Situations, STLS), free/reduced-price lunch (FRPL) eligibility, special education (SPED) status, race and/or ethnicity, age, in-school (IS) and out-of-school (OS) suspensions, and grade 9 or prior arrest records. We also include ACT Explore Test math and reading scores.

A full list of baseline variables is shown in Table 1. Students who enroll in an alternative school have much higher absences (37 days on average compared to 18 days of absences for students who never enroll in an alternative school) and lower test-scores by grade 9 (lower GPA, ACT Explorer test scores, and higher number of failed courses in grade 9). They also tend to have higher average total arrests by grade 9 (0.5) and total victimization rates by grade 9 (0.2). [11] Students in the sample typically first enroll in alternative schools during grades 11 or 12, as shown in Figure 7.[12]

C. Outcomes

The first-stage outcome of interest is enrollment in an alternative school. A student is defined as having enrolled in an alternative school if they are enrolled in an alternative school at any point after their first entry into grade 9. Students who enroll in an alternative school after 2019 are not observed in our data, though this is unlikely for our cohorts, as the latest cohort included first enrolled in grade 9 in the 2012–13 school year.

[11] The rate at which students are missing test-score and demographic information varies by group. 28% of students who never enroll in an alternative school and 21% of students who do enroll in an alternative school are missing grade 9 GPA data. This is primarily because GPA data is not available for students attending charter schools. Similarly, 21% of students who never enroll and 24% of students who eventually enroll in alternative schools lack ACT score data, while 12% and 5% of students, respectively, are missing attendance information. Missing values are imputed with zero, and a missing indicator is included in the regression analyses.

[12] Here and throughout the paper, "expected grade" is used instead of actual grade, as actual grade may be endogenous to alternative school exposure.

Our primary outcome of interest is high school graduation, defined as a student having graduated within the observation period. Similar to alternative school enrollment, students graduating after 2019 are not captured in our data. Given that we have at least six years of data for all cohorts, most students who would graduate are likely to have done so within this time frame.

Secondary outcomes include college enrollment and arrests. We explore these outcomes given that graduation alone may have limited value if it is not accompanied by positive postsecondary outcomes, such as college enrollment or reduced arrest rates. For college enrollment, a student is coded as enrolled if they graduate and subsequently enroll in college at any point during the observation period (up to 2019). It is possible that we undercount college enrollment for students in later cohorts, particularly the 2013 cohort, as they may enroll in college after 2019. Total arrests include both arrests during school years (between grade 9 and expected grade 13) and after expected school years.[13] Ideally, we would also have access to employment data to better understand the impact of alternative schools on postsecondary outcomes. In the absence of this, we focus on college enrollment and arrest outcomes, both of which are likely to influence employment outcomes as well. However, it's possible that alternative schools may divert students away from college and into direct employment. The impact of this shift on long-term employment prospects is unclear.

We also examine potential mechanisms by looking at changes in school outcomes post exposure to an alternative school. We identify the impact of exposure to alternative schools on the likelihood of being enrolled in school during expected school years (G9-13), arrests during expected school years and proportion of days absent during those school years. The attendance outcome is measured as the proportion of days attended out of the total days enrolled, conditional on the student being enrolled in school during the observation period. In other words, students not enrolled in school in a given year have no attendance outcome.

4 Identification Strategy

4.1 Main Identification Strategy

To identify the causal impact of attending an alternative school, we leverage the variation in the timing of alternative school openings and proximity to these schools to capture the impact of the expansion of alternative schools on students' persistence, graduation and college enrollment. One simple methodological approach would be to use the variation in the distance between students' home address and the nearest

[13]The observation period ends in 2019, so the post-school period varies by cohort. For the 2014 cohort, we observe only one post-school year.

alternative school to capture the impact of alternative school expansion. This method assumes that the placement of alternative schools is effectively random. However, alternative schools are often intentionally placed in neighborhoods with higher numbers of eligible students via market research, potentially introducing bias. As shown in Table A6, we see that students within a 1-mile radius of an alternative school tend to have lower GPA and test scores and are absent for more days in grade 9 compared to those not within a one-mile radius. Below we describe two ways we try to address these concerns by using both neighborhood fixed effects and exploiting variation in the timing of exposure to an alternative school.

While the broader neighborhood placement may not be random, the specific location of an alternative school within a neighborhood (i.e., conditional on neighborhood) is likely influenced by factors such as available space, which could be considered effectively random. To capture exposure to an alternative school, we define a student as "assigned to treatment" if an alternative school opens during their expected high school grades (9-13) within a one-mile radius of their grade 9 home address.[14] We define a neighborhood area as either a zip code or a census tract. There are 277 zip codes and 928 census tracts in Chicago. In a given cohort, the median zip code and census tract has 983 and 60 students, respectively.

A one-mile radius is approximately a 20-minute walk, making it conveniently located relative to students' home address. Students who enroll in alternative schools tend to reside near an alternative school. On average, students who enroll in alternative school live within 1.9 miles of an alternative school. The relationship between distance from an alternative school and enrollment in one is downward sloping, with enrollment probabilities flattening around a 2-mile radius, as depicted in Figure 3. Also, as shown in Figure 4, conditional on zip code, the relationship between distance and enrolling in an alternative school flattens around the 2-mile radius. We show the estimates using different distance cutoffs as well. Equation (1) below is used to capture the impact of residing within a 1-mile radius of an alternative school relative to students in the same neighborhood b who reside further away.

$$Y_i = \beta_0 + \beta_1 ASExposedG9to13_i + X_i'\beta_3 + \delta_s + \delta_b + \delta_c + \epsilon_i \tag{1}$$

Where $ASExposedG9to13_i$ is an indicator for a student residing within a one-mile radius of an alternative school during grades 9 to 13. Neighborhood fixed effects (δ_b) and Grade 9 entry school fixed effects (δ_s) capture neighborhood and grade 9 school level differences, while cohort fixed effects, δ_c, account for the trends

[14] We use students' grade 9 address because students usually attend an alternative school only after attending a traditional high school and are unlikely to make a decision about where to live in grade 9 based on expected alternative school enrollment in later grades. CPS strongly discouraged enrollment of 9th graders in alternative schools during this time period.

in outcomes across time (student cohorts). We also include a vector of student baseline covariates, X_i', which includes student grade 9 test-scores, attendance, race/ethnicity, English language status, free/reduced lunch status, in-school and out-of-school suspension, and prior arrests. The main coefficient of interest, β_1, captures the impact of residing within a 1-mile radius of an alternative school during grades 9-13 relative to other students in the same neighborhood who are not within a 1-mile radius.

This analysis exploits the variation in distance to an alternative school and exposure to an alternative school between different cohorts of students from the same neighborhood. A student may not reside within a 1-mile radius of an alternative school either because they are older and the alternative school opened in later cohorts, or because they reside further away from the alternative school.

Here, Y_i corresponds to a point-in-time (long-run) outcome of interest for student i: enrollment in an alternative school (first-stage), graduation from high school, college enrollment and total (or any) arrests after grade 10. If a student ever enrolls in an alternative school, the outcome takes on a value of 1 throughout. Similarly, if a student graduates within the eight years we observe them in, the outcome takes on a value of 1 throughout. We control for two different definitions of neighborhood area: zip code, as shown in Model (1) in Table 2, and census tract as shown in Model (2). The trade-off is between increasing the likelihood that the comparison of two groups of students is truly random – e.g., two students living on the same block, one set of whom happened to have an alternative school nearby during grades 9 to 13 likely because their cohort coincided with the alternative school opening, against limiting the variation so severely that meaningful inferences cannot be drawn.[15]

A key identifying assumption in Equation (1) is that the exact location of an alternative school within a neighborhood is random. To address the concern that, even conditional on neighborhood, the location of an alternative school may not be random, in Equation 2 we instead only use the variation in when an alternative school opens relative to a student's cohort. The timing of when an alternative school opens relative to a student's grade level may plausibly be random. Students exposed to an alternative school during grades 9 to 13 are likely affected by an alternative school opening nearby, while those exposed in hypothetical grades 14 to 16 (older cohorts) are not. Building on this intuition, we employ a difference-in-differences design to compare older students within a 1-mile radius of alternative school that was not an option for them during high school years to younger students who were still in school and an alternative school opened within 1-mile of them. Though this method limits the variation we use to variation due to the timing of an alternative school opening, it is our preferred method because the timing of an alternative school opening

[15]Chicago is 228 square miles (Census, 2023). A census tract size differs depending on the population residing in the area (Census, 2025). Assuming each census tract is equal in size, each census tract would be about 0.23 square miles. As such, when including census tract fixed effects, most of the variation we are capturing is between cohorts rather than within a cohort.

in a neighborhood is less likely to be correlated with student characteristics.[16] Equation (2) captures this identification strategy:

$$Y_i = \beta_0 + \beta_1 ASExposedG9to13_i + \beta_2 ASExposed_i + X_i'\beta_3 + \delta_s + \delta_c + \epsilon_i \tag{2}$$

Where $ASExposedG9to13_i$ takes on a value of 1 if a student is within a 1-mile radius of an alternative school at any point between their grades 9 to 13 school years, and 0 otherwise, while $ASExposed_i$ takes on a value of 1 if a student is within a 1-mile radius of an alternative school, independent of grade level, and 0 otherwise. Here, β_1 measures the effect of exposure during grades 9 to 13, while β_2 captures level differences between students ever exposed and those never exposed to an alternative school. Similar to Equation (1), we also include Grade 9 entry school fixed effects, δ_s to capture level differences between students who enroll in different grade 9 traditional public schools, and cohort fixed effects δ_c to capture differences across cohorts, as well as a vector of student baseline characteristics, X_i'.

This strategy assumes that the timing of an alternative school opening is independent of students' potential outcomes. This assumption is violated if the change in an outcome (e.g., graduation rates) across cohorts within a neighborhood is correlated with an alternative school opening year. For example, if alternative schools that opened earlier targeted neighborhoods with a slower increase in graduation rates. To test this assumption, we examine pre-trends in alternative school enrollment as well as other outcomes like graduation likelihood and confirm that students first exposed in later grades (14 to 16) are no more or less likely to enroll in an alternative school or graduate high school than those never exposed to an alternative school. Results in Figure 8 show that the impact on alternative school enrollment is driven by exposure during grades 9 to 13. We also present results with neighborhood time trends to account for temporal changes within neighborhoods unrelated to alternative schools. This allows us to estimate the impact of alternative schools on deviations from neighborhood trends. In Model (4) in Table 2 we include zip code trend lines, and in Model (5) we include tract trend lines.

Lastly, using distance as an instrument may not meet the exclusion restriction, as distance to an alternative school may impact student outcomes through other ways besides attending an alternative school. For instance, alternative school placement may alter the composition of nearby traditional schools, indirectly

[16]Since most alternative schools opened between 2012 and 2014, the variation in this estimation is mostly capturing the difference between earlier cohorts (2009 to 2011) who would have resided within 1-mile of an alternative school had the alternative school opened, and later cohorts (2012-2014) whose high-school years overlapped with an alternative school opening within 1-mile.

affecting outcomes. Similarly, increased police presence near alternative schools could influence students' behavioral outcomes independent of if they enroll in an alternative school. Thus, we primarily interpret the reduced-form estimates, which provide meaningful insights into how alternative schools may impact student outcomes. We also later directly try to identify the potential spillover of alternative schools on neighboring schools.

4.2 Within Student Changes

While our primary outcomes of interest, such as ever graduating high school or enrolling in college, are time-invariant, analyzing time-varying outcomes can provide insights into how and why alternative schools may impact student outcomes. For the within-student analysis, our main outcomes include school enrollment, the proportion of days absent, and arrests during school years. To identify the impact of exposure to an alternative school on changes in these outcomes, we employ a difference-in-differences (DiD) design, leveraging variation in the timing of when an alternative school first becomes available within a one-mile radius of a student's home.

In this identification strategy, we observe each student for five years, starting from their initial enrollment in grade 9. Equation (3) represents this approach:

$$Y_{it} = \beta_0 + \beta_1 PostASExposure_{it} + \delta_{s,c,t} + \delta_i + \epsilon_{it} \tag{3}$$

Where $PostASExposure_{it}$ takes on a value of 1 during the years a student is within a 1-mile radius of an alternative school. Grade 9 school-cohort-year fixed effects ($\delta_{s,c,t}$) are included to account for school, cohort, and grade-specific changes that could influence student outcomes. Student fixed effects (δ_i) control for time-invariant differences at the student level.

As shown in Figure 11, there is no evidence of pre-trends, suggesting that the trajectories of treated students (those exposed to an alternative school within a one-mile radius) were similar to those of untreated students (those never within a 1-mile radius) prior to exposure to an alternative school.

Treatment effects may be gradual and heterogeneous; as such, the treated units may not be good comparison units for later treated units. To address this concern, we group treated units by year of exposure to an alternative school and use students who are never within a one-mile radius of an alternative school during grades 9 to 13 (never treated) as the comparison group for each of the treated units. We then calculate a

weighted average of the estimated treatment effect from each treated unit using regression weights (similar to Mallah, 2025).[17] This approach is based on the stacked difference-in-differences estimator proposed by Callaway and Sant'Anna (2021) to correct for biases in the traditional difference-in-difference identification strategy.

4.3 Heterogeneity by Students' Baseline Academic Performance

Alternative schools specifically target and serve students at high risk of dropping out. Therefore, we run our analysis both averaging across all students and by students' academic performance in grade 9. As shown in Table A3, the strongest predictors of enrollment in alternative schools are grade 9 GPA and the number of courses failed in grade 9.[18] As such, we present results for both the full sample and for subgroups defined by baseline GPA and the number of courses failed.

Figure 5 illustrates that students with lower GPAs in grade 9 are significantly more likely to enroll in an alternative school. Among students residing within a one-mile radius of an alternative school during their schooling years, those with a 1.0 GPA are approximately 20 percentage points more likely to enroll in an alternative school compared to peers with a 3.0 GPA. This relationship is more pronounced among students exposed to an alternative school during their schooling years (grades 9–13) than among those exposed after their schooling years (grades 14–16). For students residing within a one-mile radius of an alternative school after their schooling years, the difference in enrollment likelihood between students with a 1.0 GPA and those with a 3.0 GPA is closer to 10 percentage points. This pattern further motivates our preferred identification strategy, which leverages differences in when a student is exposed to an alternative school.

A similar relationship is observed between the number of courses failed in grade 9 and the likelihood of enrolling in an alternative school, as shown in Figure 6. Students who fail more courses in grade 9 are disproportionately more likely to enroll in an alternative school, particularly if they are exposed to one during their schooling years.

[17] The weights are based on the variance and sample size of each treated group, as such, units treated in the middle of the observation period typically receive more weight.

[18] The prediction is based on a Random Forest Model using three covariates at each split. The model was developed with the 2014 cohort and validated using other cohorts to assess its fit.

5 Results

5.1 Main Results

5.1.1 First Stage

We find that residing within a one-mile radius of an alternative school during schooling years increases the likelihood of enrolling in an alternative school by approximately 28 percent, or 1.7 percentage points, from a baseline of 6%—by about 3,349 more students across the six cohorts of students. Table 2 shows the first-stage results, with F-statistics ranging from 118 in Model (1) to 35 in Model (5). The impact of residing within a 1-mile radius of an alternative school on alternative school enrollment is most pronounced among students in the bottom GPA quartile and those with a higher number of courses failed in ninth grade, as illustrated in Figures 9 and 10. Specifically, students in the bottom quartile of the ninth grade GPA distribution and students who failed six or more courses are 3.1 percentage points more likely to enroll in an alternative school, compared to a baseline of 19%.[19]

Defining exposure to an alternative school based on a smaller versus a larger radius involves several trade-offs. The closer the alternative school that opened is to a student's home address the more likely it is to impact their decision to enroll in an alternative school. However, it also increases the likelihood that the comparison group includes students who are marginally beyond the cutoff but still affected by the alternative school's presence, potentially biasing our estimates downward. Additionally, a smaller radius may reduce statistical power, as fewer students are classified as treated. At the same time, the smaller the radius the more likely the alternative school opening nearby is random, improving the validity of the estimates.

Given the trade-offs, we re-estimate our primary outcomes using alternative distance cutoffs ranging from 0.4 to 2 miles to identify if the estimates are consistent. The first-stage estimates of the impact of proximity to an alternative school on enrollment likelihood range from 2.6 (F-Stat = 25), 2.0 (F-Stat = 47) and 1.1 percentage points (F-Stat = 21) at the 0.4-, 0.8-, and 1.2-mile cutoffs, respectively, as shown in Figure 14.[20]

5.1.2 Second Stage (Reduced Form)

We find suggestive evidence of a positive effect on high school graduation. Students residing within a one-mile radius of an alternative school are 0.8 percentage points more likely to graduate from high school—an

[19] Students were grouped by the number of courses failed to ensure comparable group sizes: 53% of students did not fail any courses in grade 9, 17% failed 1–2 courses, 11% failed 3–5 courses, and 18% failed 6 or more courses.

[20] The summarized estimates are based on our preferred specification (model 3), which leverages variation in the timing of alternative school openings within x-miles of a student, as shown in Equation 2.

increase of about 1,550 more students graduating across the six cohorts. This estimate is consistent across model specifications, as shown in Table 2. However, the result is sensitive to the choice of distance cutoff, as illustrated in Figure 14. Specifically, the estimated effects on graduation rates are –0.7, -0.07, and 0.5 percentage points at the 0.4-, 0.8-, and 1.2-mile cutoffs, respectively. None of the estimates are significant at the 5% level. The wide standard errors and lack of alignment between these patterns and the first-stage estimates on alternative school enrollment suggests that the positive effect observed at the 1-mile cutoff may be spurious.[21]

While high school graduation is undoubtedly an important outcome, its value may depend on whether students gain essential skills and achieve positive postsecondary outcomes. Our findings suggest alternative schools help protect students from involvement in the criminal legal system, but simultaneously decrease the likelihood that they attend postsecondary school.

We find exposure to an alternative school decreases college enrollment; this estimate is significant at the 5% level in Model (1), as shown in Table 2. The effect on college enrollment using different distance cutoff points are generally consistent with the first-stage estimates as shown in Figure 14. The estimated negative impact on college enrollment is -1.8, -1.5, and -0.5 percentage points at the 0.4-, 0.8-, and 1.2-mile cutoffs, respectively. Two of these estimates are statistically significant at the 5% level, indicating that exposure to alternative schools reduces students' likelihood of enrolling in college within the eight-year observation window.

However, we also find evidence suggesting that exposure to alternative school decreases total arrests by 0.06 (approximately 3.5 fewer arrests if scaled by the first-stage, assuming the exclusion restriction holds). Similar to college enrollment effects, the estimated effects on the number of arrests after entry to grade 9 using different distance cutoff points are consistent with the first-stage estimates as shown in Figure 14. The estimated decrease in total arrests is -0.04, -0.07, and -0.05, at the 0.4-, 0.8-, and 1.2-mile cutoffs, respectively, with two of the estimates significant at the 10% level. The estimates of the impact on arrests suggest that alternative school exposure likely reduce total arrests after ninth grade.[22]

To address concerns that the comparison group–students residing beyond a one-mile radius of an alternative school–may include individuals at the margin of the cutoff who are still affected by the school's opening, potentially biasing estimates downward, we exclude students who were ever within one to two miles of an alternative school. By excluding students within a 1- to 2-mile radius, we ensure that the comparison group

[21] The estimates for the different cutoffs are based on our preferred specification (model 3), which leverages variation in the timing of alternative school openings within x-miles of a student, as shown in Equation 2.

[22] we find evidence of a decrease in total arrests (intensive margin), but no evidence of a decrease in any arrests (extensive margin).

consists solely of students residing at least two miles away from an alternative school. Table A8 presents the estimates after excluding these marginal cases. The results remain broadly consistent, with larger and more precise estimated decreases on total arrests, supporting the concern that the original control group may have included some treated units, thereby attenuating our baseline estimates.

Figures 9 and 10 split the estimates by students' GPA and number of courses failed in grade 9 to provide additional insights to what group of students may be driving the estimates, but the estimates lack precision. There is some evidence that the increase in graduation likelihood is driven by students who failed six or more courses in grade 9. Students in this group residing within a one-mile radius of an alternative school are 1.8 percentage points more likely to graduate high school, compared to a baseline of 32% ($p = 0.07$). [23]

5.2 Changes in Student Outcomes During School Years

To understand what might explain effects on graduation, postsecondary and arrest outcomes, we examine whether exposure to alternative schools affects students' persistence (remaining enrolled in school), attendance, and arrest likelihood during schooling years. Using within-student variation in exposure to alternative schools (Equation 3), we find that when an alternative school opens within a one-mile radius of a student's home, they are 1.3 percentage points more likely to enroll in an alternative school (F-stat = 88.53) as shown in Table 3. The effect is more pronounced among students in the bottom GPA quartile and those who failed six or more courses in grade 9, with increases of 2.5 and 2.7 percentage points in alternative school enrollment, respectively.

Exposure to an alternative school increases students' likelihood of remaining enrolled in high school (i.e., reduces dropout rates in a given year). Students are 1.0 percentage point more likely to be enrolled in school following exposure to an alternative school. The effect on persistence is large relative to the first-stage impact on alternative school enrollment of 1.3 percentage points. Assuming the exclusion restriction holds, this may suggest that students who are induced to enroll in an alternative school would have otherwise dropped out of high school (i.e., the counterfactual for most students induced to enroll in an alternative school is not enrollment in another traditional public school). Including students who have graduated, we find a similar effect: students are 1.0 percentage points more likely to be enrolled or to have graduated high school after exposure to an alternative school. We find no significant impact on students' attendance (measured as the proportion of days absent, conditional on enrollment) or their likelihood of being arrested during schooling years.

[23] For students who failed six or more courses, there is evidence suggesting that they are 2.4 percentage points more likely to be arrested ($p = 0.03$), though we do not detect any change in their total number of arrests.

The increase in enrollment likelihood is driven primarily by students in the bottom GPA quartile and those who failed six or more courses in grade 9 as shown in Figure 12 and Figure 13. For students who scored in the bottom GPA quartile and those who failed six or more courses in ninth grade, the likelihood of remaining enrolled in high school increases by 1.9 and 2.2 percentage points, respectively. In other words, exposure to an alternative school seems to decrease students' likelihood of dropping out of high school in a given year, and this is mainly driven by students at higher risk of dropping out given their academic performance in ninth grade.

We also analyze the sensitivity of the within-student difference-in-differences estimates to the distance cutoff. Across cutoffs between 0.4 and 2 miles, exposure to an alternative school consistently increases students' likelihood of being enrolled in any school during expected grades 9 to 13, as shown in Figures 15. However, the non-linear relationship between the estimated impact on enrollment in high school and distance suggests potential spillover effects. This implies that the observed impacts may not be exclusively driven by direct enrollment in alternative schools. In the next section, we explore the potential for spillover effects on neighboring schools.

6 Spillovers on Neighboring Schools

Alternative schools may intensify competition among schools for lower-achieving students, who often have higher rates of misconduct and arrests at baseline.[24] The traditional public schools are less likely to try to retain those students given the potential negative peer effects of lower-achieving students with high misconduct records on other students. Consequently, the presence of alternative schools may lead to changes in student composition by reducing the number of high-misconduct, low-achievement students. This reallocation could have spillover effects on students who remain in traditional schools. For example, as previously mentioned, Carrell and Hoekstra (2010) find that exposure to one additional disruptive peer in a classroom of 20 students decreases student achievement by 0.69 percentile points and increases disciplinary infractions by 17%. Thus, even a small degree of sorting may generate positive spillovers for peers in traditional public schools.

To assess competition with traditional public schools, we calculate the number of alternative schools within a 2-mile (or 1-mile) radius. We then examine how changes in the number of alternative schools affect students' long-term outcomes: arrest, graduation, and college enrollment. Using the same student sample described in Section 4, we assign students to the traditional public school they first attended in grade 9 and

[24] "traditional public schools" include charter schools that are not classified as alternative schools.

maintain five observations per student corresponding to their expected grades 9 through 13.[25] The empirical specification is given by Equation 4:

$$Y_{it} = \beta_0 + \beta_1 Number of Neighboring Alternative Schools_{it} + X_i' \beta_3 + \delta_s + \delta_{c,t} + \epsilon_{it} \tag{4}$$

Where β_1 captures the impact of changes in the number of alternative schools within 2-miles (or 1-mile) radius of a traditional public school on student outcomes. School fixed effects δ_s control for level differences in outcomes, while cohort-year fixed effects $\delta_{c,t}$ account for time-varying cohort-specific factors. The identifying assumption is that changes in the number of alternative schools around a traditional public school are independent of changes in cohort characteristics, conditional on students' ninth grade baseline characteristics X_i', including students' neighborhood fixed effects (census block). This assumption is violated if schools experiencing changes in the number of alternative schools around them during this period also happen to be experiencing changes in their students' number of arrests or graduation outcomes.

Between 2009 and 2019, 67% of schools within a 2-mile radius experienced a change in the number of neighboring alternative schools, while 27% were never within a 2-mile radius of an alternative school (Figure 16). Within a 1-mile radius, 30% of schools experience a change in the number of neighboring alternative schools, while 66% were never within a 1-mile radius of an alternative school.

Our analysis indicates that alternative schools may generate spillovers on neighboring schools. Specifically, the presence of one additional alternative school within a 2-mile radius is associated with 0.03 fewer average total arrests after grade 9 among students assigned to a neighboring school and a negative effect on college enrollment (Table 4).

To try to isolate the source of these effects, we control for whether a student ever enrolls in an alternative school to identify if the impact is driven by enrollment in an alternative school. Note that here we are controlling for an outcome and therefore the estimates are only suggestive, but not conclusive. The estimate on total arrests remains consistent (slightly larger and more precise) when controlling for alternative school enrollment, suggesting that the reduction in arrests is primarily driven by spillovers on students who remain in traditional public schools. In comparison, the estimated negative effect on college enrollment is slightly smaller and less precise, when controlling for alternative school enrollment.[26]

[25] 0.37% of students in the original sample first enroll in a school for which we do not have address information in the CPS data; those students are dropped in this analysis. We only include schools that are open and serving high school students that school year which is based on enrolling at least ten students in high school grades in a given year.

[26] Estimates are consistent but insignificant when using the 1-mile radius, likely due to the smaller number of schools experi-

7 Conclusion

This study examines the expansion of alternative schools in Chicago and their impact on key student outcomes, including high school persistence, graduation, college enrollment, and arrests. Leveraging variation in the timing and geographic proximity of alternative school openings, we find evidence that alternative schools increase high school persistence with some evidence that they may reduce arrests and reduce college enrollment. Our findings contribute to the literature on school choice, student tracking, and the broader implications of differentiated schooling options for more vulnerable populations.

Alternative schools appear to fulfill their primary objective of increasing high school persistence. However, their impact on postsecondary outcomes is mixed. The positive impact on students' high school graduation rates is less robust, varying by model specification and distance cutoff. This suggests that while alternative schools may successfully retain students, the transition from persistence to graduation remains a challenge. We also find some evidence that alternative schools may reduce students' college enrollment. This finding raises important questions about the preparation and guidance provided by these schools for students' postsecondary trajectories. That said, we find evidence suggesting that alternative schools may improve student behavioral outcomes, namely arrests. Though we find no impact on the extensive margin (any arrests), we do find evidence suggesting that residing within a 1-mile radius of an alternative school reduces students' total arrests (intensive margin).

Finally, we document evidence suggesting that alternative schools may have a spillover effect on neighboring traditional public schools. An increase in the number of neighboring alternative schools is associated with a decrease in total arrests for students in neighboring traditional public schools. The decrease in number of arrests does not seem to be driven by enrollment in an alternative school (the estimate is consistent and significant conditional on a student enrolling in an alternative school). This finding aligns with prior literature on peer effects, suggesting that even small changes in the share of students with negative behavioral outcomes can have significant implications for classroom and school dynamics.

Our findings suggest that policymakers should consider strategies to ensure that alternative schools not only improve high school persistence but also support graduating high school and long-term outcomes such as college enrollment and employment. They also suggest that efforts to scale alternative schools should account for their broader impacts on traditional public schools.

This study is among the first to rigorously evaluate the large-scale implementation of alternative schools in a major urban district. By bridging gaps in the literature on school choice and student tracking, our

encing changes in the number of alternative schools within this narrower radius.

findings provide valuable insights into the role of alternative schools in addressing educational inequities. However, several limitations remain. The generalizability of these results beyond Chicago (specifically ALOP and contract alternative schools) is uncertain, as alternative school structures and student populations may vary across districts. The estimates are also based on students who are induced to enroll in an alternative school because of distance. Students induced to enroll may be different from students who enroll in an alternative school independent of distance.[27] Additionally, future research should investigate the long-term impacts of alternative schools on employment, earnings, and other life outcomes to fully capture their efficacy. Importantly, our identification strategy itself is also prone to bias; it could be that the estimates are driven by neighborhood changes that coincide with an alternative school opening rather than exposure and enrollment in alternative schools. We try to address this limitation in the paper by including neighborhood fixed effects and trend controls as well as extensive student baseline controls, but there may still be factors that we do not account for in our setting.

References

Ang, Desmond (2020). "The Effects of Police Violence on Inner-City Students*". en. In: *The Quarterly Journal of Economics* 136.1, pp. 115–168.

Angrist, Joshua D. and Alan B. Krueger (1991). "Does Compulsory School Attendance Affect Schooling and Earnings?" In: *The Quarterly Journal of Economics* 106.4. Publisher: Oxford University Press, pp. 979–1014.

Autor, David, Claudia Goldin, and Lawrence F. Katz (May 2020). "Extending the Race between Education and Technology". en. In: *AEA Papers and Proceedings* 110, pp. 347–351.

Brough, Rebecca, David C. Phillips, and Patrick S. Turner (Nov. 2024). "High Schools Tailored to Adults Can Help Them Complete a Traditional Diploma and Excel in the Labor Market". en. In: *American Economic Journal: Economic Policy* 16.4, pp. 34–67.

Census (2021). *Seizing the Opportunity to Advance Education Equity: Data Insights from Chicago's Options School Students.*

— (2023). *QuickFacts: Chicago city, Illinois.*

— (2025). *Glossary.*

Collins, Courtney and Li Gan (Feb. 2013). *Does Sorting Students Improve Scores? An Analysis of Class Composition.* en. Tech. rep. w18848. Cambridge, MA: National Bureau of Economic Research, w18848.

COX, STEPHEN M. (Aug. 1999). "An Assessment of an Alternative Education Program for At-Risk Delinquent Youth". EN. In: *Journal of Research in Crime and Delinquency* 36.3. Publisher: SAGE Publications Inc, pp. 323–336.

Duflo, Esther, Pascaline Dupas, and Michael Kremer (Aug. 2011). "Peer Effects, Teacher Incentives, and the Impact of Tracking: Evidence from a Randomized Evaluation in Kenya". en. In: *American Economic Review* 101.5, pp. 1739–1774.

Dugger, Janice M. and Chester W. Dugger (1998). "An Evaluation of a Successful Alternative High School". In: *The High School Journal* 81.4. Publisher: University of North Carolina Press, pp. 218–228.

Dynarski, Mark and Robert Wood (1997). "Helping High-Risk Youths: Results from the Alternative Schools Demonstration Program". In: *Princeton, NJ: Mathematica Policy Research.*

Figlio, David N. and Umut Özek (Jan. 2023). *The Unintended Consequences of Test-Based Remediation.* Working Paper.

[27] That said, students who enroll in an alternative school who reside within 1-mile radius of an alternative school look very similar to students who are not within 1-mile radius of an alternative school but enroll in an alternative school, as shown in Table A6. The limited difference between the two suggests that students induced to enroll because of distance are likely similar to students further away who choose to enroll in an alternative school.

Figlio, David N. and Marianne E. Page (May 2002). "School Choice and the Distributional Effects of Ability Tracking: Does Separation Increase Inequality?" en. In: *Journal of Urban Economics* 51.3, pp. 497–514.

Kim, Youngran (May 2018). "The effects of school choice on achievement gaps between private and public high schools: Evidence from the Seoul high school choice program". en. In: *International Journal of Educational Development* 60, pp. 25–32.

Law, Alternative Learning Opportunities (n.d.). *P.A. 92-42, eff. 1-1-02. Illinois Compiled Statutes (ILCS)*.

Lehr, Camilla A. and Cheryl M. Lange (Jan. 2003). "Alternative Schools Serving Students With and Without Disabilities: What Are the Current Issues and Challenges?" In: *Preventing School Failure: Alternative Education for Children and Youth* 47.2. Publisher: Routledge _eprint: https://doi.org/10.1080/10459880309604431, pp. 59–65.

Lehr, Camilla A., Chee Soon Tan, and Jim Ysseldyke (Jan. 2009). "Alternative Schools: A Synthesis of State-Level Policy and Research". In: *Remedial and Special Education* 30.1. Publisher: SAGE Publications Inc, pp. 19–32.

Mallah, Farah (2025). "Tracking to Retain Higher-Income Students: Evidence from the Addition of Advanced Courses. Working Paper". In.

Monarrez, TomÃ¡s, Brian Kisida, and Matthew Chingos (Feb. 2022). "The Effect of Charter Schools on School Segregation". en. In: *American Economic Journal: Economic Policy* 14.1, pp. 301–340.

Mumma, Kirsten Slungaard (May 2022). "The Effect of Charter School Openings on Traditional Public Schools in Massachusetts and North Carolina". en. In: *American Economic Journal: Economic Policy* 14.2, pp. 445–474.

Nechyba, Thomas J. (Jan. 2006). "Chapter 22 Income and Peer Quality Sorting in Public and Private Schools". en. In: *Handbook of the Economics of Education*. Ed. by E. Hanushek and F. Welch. Vol. 2. Elsevier, pp. 1327–1368.

Oreopoulos, Philip (2006). "Estimating Average and Local Average Treatment Effects of Education When Compulsory Schooling Laws Really Matter". In: *American Economic Review* 96.1, pp. 152–75.

Özek, Umut (Nov. 2021). "The effects of middle school remediation on postsecondary success: Regression discontinuity evidence from Florida". In: *Journal of Public Economics* 203, p. 104518.

Raywid, Mary Anne (1995). "Alternative Schools: The State of the Art". en. In: *Educational Leadership* 52.1. ERIC Number: EJ509876, pp. 26–31.

Ruzzi, Betsy Brown (n.d.). "Academic Programs in Alternative Education An Overview". en. In: ().

U.S. Department of Education, National Center for Educational Statistics (2002). "Characteristics of the 100 Largest Public Elementary and Secondary School Districts in the United States: 2000–01 (NCES 2002-351)". en. In.

8 Main Plots and Figures

Table 1: Grade 9 Baseline Summary Statistics by Students' Enrollment in an Alternative School

Variable	Never Enrolled in an AS	Ever Enrolled in an AS
Number of Days Absent in G9	17.543	36.942
Number of Member Days in G9	159.442	158.259
Total Arrests Prior to and Including G9	.218	.48
Total Victimization Prior to and Including G9	.098	.238
Temporary Living Situation (STLS) G9	.041	.102
English as Second Language (ESL) G9	.074	.059
Special Education (SPED) G9	.147	.178
GPA G9	2.333	1.337
Standardized Math ACT Explore Score G9	.094	-.455
Standardized Reading ACT Explore Score G9	.081	-.462
Courses Failed G9	2.582	6.978
Black	.45	.62
Hispanic	.411	.339
Male	.508	.546
Free/Reduced Lunch G9	.787	.911
Age at Start of G9 Entry Year	15.76	15.843
Total In-School Suspensions in G9	.467	1.388
Total Out-of-School Suspensions in G9	1.024	3.603
Missing Baseline GPA G9	.277	.208
Missing ACT Explore Test Data	.207	.237
Missing Attendance G9	.12	.054
Number of Students	182,032	14,731

The means are based on the non-missing observations for each variable. Missing observations are imputed with 0 in the regression which includes missing indicators. These summary statistics are based on the sample of students defined in Section 4 for the impact analysis.

Table 2: Impact of Residing within a 1-Mile Radius of an Alternative School: Full Sample

	Model 1	Model 2	Model 3	Model 4	Model 5
Enrolled in an Alternative School	0.0169***	0.0133***	0.0173***	0.0185***	0.0168***
	(0.00160)	(0.00205)	(0.00257)	(0.00267)	(0.00282)
F-Stat	111.7	42.29	45.29	47.68	35.70
Control Mean	0.0600	0.0600	0.0600	0.0600	0.0600
N	196520	196537	196643	196520	196537
Graduated High School	0.00533*	0.00591+	0.00793+	0.00928*	0.00996*
	(0.00232)	(0.00306)	(0.00420)	(0.00430)	(0.00443)
Control Mean	0.650	0.650	0.650	0.650	0.650
N	196520	196537	196643	196520	196537
Enrolled in College	-0.00646**	-0.00338	-0.00413	-0.00439	-0.00479
	(0.00250)	(0.00310)	(0.00436)	(0.00449)	(0.00464)
Control Mean	0.480	0.480	0.480	0.480	0.480
N	196520	196537	196643	196520	196537
Total Arrests	-0.00171	-0.0333+	-0.0627*	-0.0743*	-0.0711*
	(0.0150)	(0.0194)	(0.0318)	(0.0325)	(0.0327)
Control Mean	0.850	0.850	0.850	0.850	0.850
N	196520	196537	196643	196520	196537
Any Arrests	0.00497*	0.00263	-0.000129	-0.00207	-0.00345
	(0.00218)	(0.00272)	(0.00426)	(0.00437)	(0.00444)
Control Mean	0.210	0.210	0.210	0.210	0.210
N	196520	196537	196643	196520	196537
Zip-code Fixed Effect	X				
Tract Fixed Effect		X			
Zip-code Trend Lines				X	
Tract Trend Lines					X
Ever within 1-Mile Radius			X	X	X

Standard errors in parentheses. Standard errors are clustered at the block-cohort level. Models (1) and (2) are based on Equation (1). Model (3) is based on Equation (2), and Models (4) and (5) additionally include neighborhood time trends. The outcomes are listed in each column and the coefficients are based on the β_1 estimate from each of the Equations. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3: Impact of Exposure to AS: Full Sample [Within Student Analysis]

	Enrolled in AS	Enrolled	Grad/Enrolled	Total Arrests	Any Arrests	Absent
Post Exposure to AS	0.0126***	0.0102***	0.0106***	-0.00360	-0.000144	0.000932
	(0.00134)	(0.00168)	(0.00190)	(0.00387)	(0.00148)	(0.00109)
F-Stat	88.53					
Control Mean	.02	.86	.86	.21	.11	.15
N	4176270	4176270	4176270	4176270	4176270	2702930

Standard errors in parentheses. Standard errors are clustered at the block-cohort level. The absent outcome captures the proportion of students' member days on which they were absent. The arrest outcomes are based on arrests in a given school year. Estimates are based on Equation (3). The control mean is based on students at $t = -1$ who are at some point within a 1-mile radius of an alternative school. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Impact of Changes in the Number of Alternative Schools Neighboring Students' Grade 9 Traditional Public School on Student Outcomes

	Within 2-Miles	Within 1-Mile
Enrolled in an Alternative School	0.00495**	0.00581
	(0.00179)	(0.00467)
	0.0600	0.0700
Ever Graduated	-0.00373	-0.00165
	(0.00231)	(0.00408)
	0.650	0.650
Enrolled in College	-0.00498*	-0.00502
	(0.00222)	(0.00398)
	0.490	0.480
Total Arrests	-0.0257*	-0.0323
	(0.0124)	(0.0259)
	0.860	0.900
Any Arrest	-0.00179	0.00123
	(0.00135)	(0.00334)
	0.220	0.230
School Fixed Effect	X	X
Cohort-Year Fixed Effect	X	X
N	966449	966449

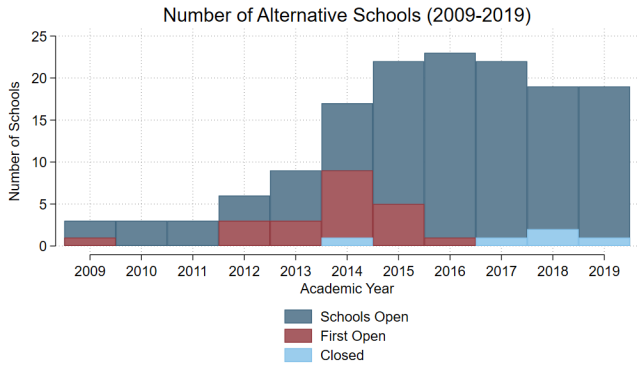
Standard errors in parentheses. Standard errors are clustered at the school level. Both models based on Equation (4). The outcomes are listed in the each column and the coefficients are based on the β_1 estimate from each of the Equations. Model 1 captures the impact of changes in the number of alternative schools within a 2-mile radius of a traditional public school. Model 2 is based on the number of alternative schools within a 1-mile radius instead. Below the standard error in parentheses is the control mean based on the average for students in schools not within a 2-mile radius in model 1, and not within a 1-mile radius in model 2. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Impact of Changes in the Number of Alternative Schools Neighboring Students' Grade 9 Traditional Public School on Students' Outcomes, Conditional on Alternative School Enrollment

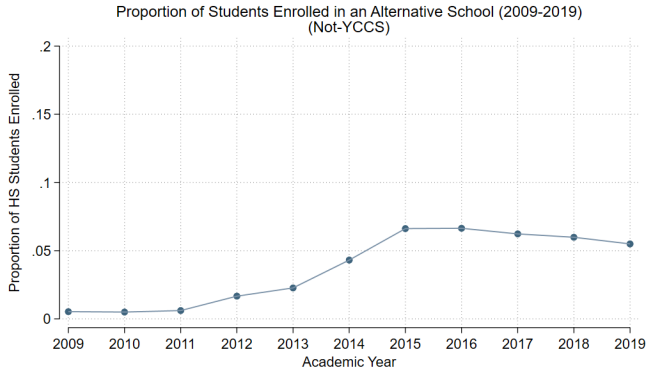
	Within 2-Miles	Within 1-Mile
Ever Graduated	-0.00304	-0.000842
	(0.00230)	(0.00388)
	0.650	0.650
Enrolled in College	-0.00405^{+}	-0.00392
	(0.00212)	(0.00353)
	0.490	0.480
Total Arrests	-0.0306*	-0.0381
	(0.0133)	(0.0282)
	0.860	0.900
Any Arrest	-0.00263^{+}	0.000240
	(0.00137)	(0.00363)
	0.220	0.230
School Fixed Effect	X	X
Cohort-Year Fixed Effect	X	X
N	966449	966449

Similar to table 4 but the model includes a control for if a student has ever attended an alternative school.

Figure 1: Number and Enrollment in Alternative Schools Over Time



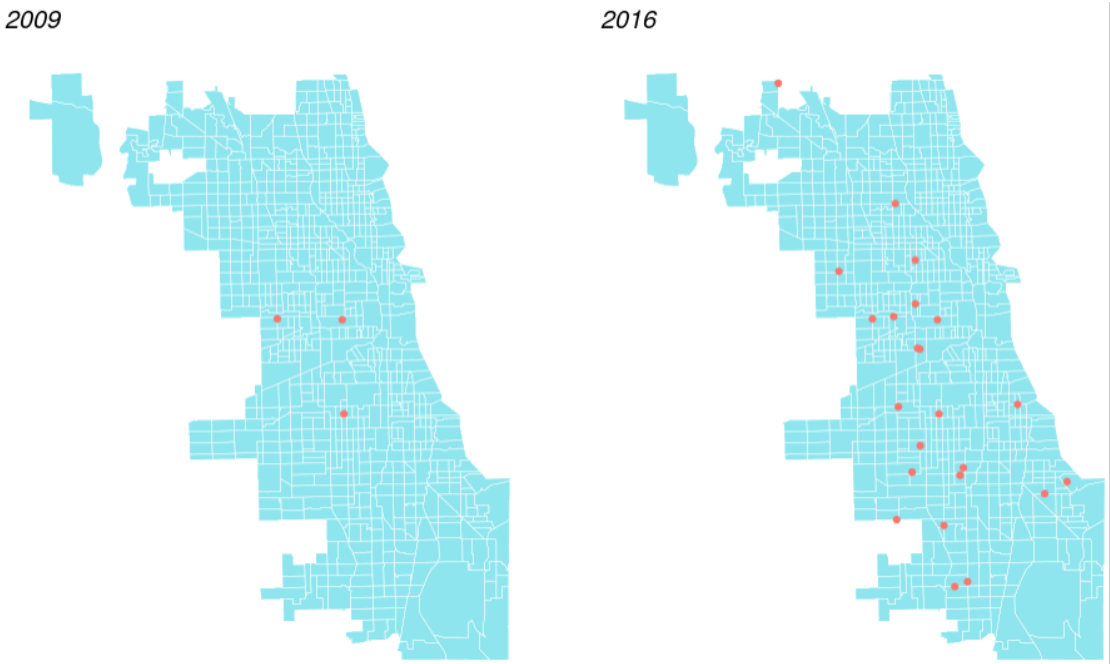
(a) Change in the Number of Alternative Schools



(b) Proportion of Students Enrolled in an Alternative School

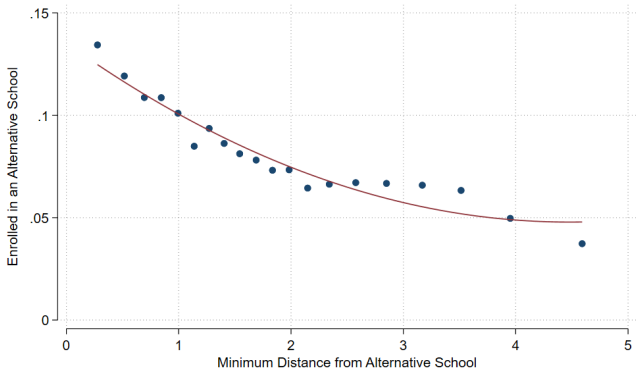
Notes: Plots capture change in number of schools and enrollment in alternative schools (excluding YCCS) between 2009 and 2019. Panel (a) shows the number of alternative schools. Panel (b) shows the change in the proportion of the total number students enrolled in CPS public schools who are enrolled in an alternative (not YCCS) schools. Figures A1 include all alternative schools, including YCCS, from 2013 onward.

Figure 2: Map of Alternative School Locations in 2009 and 2016



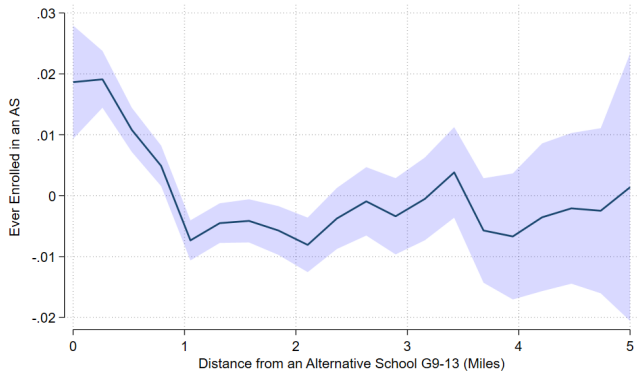
Notes: Plot is based on each alternative school address in the given year based on CPS data. It excludes YCCS alternative schools.

Figure 3: Distance from an Alternative School and Alternative School Enrollment



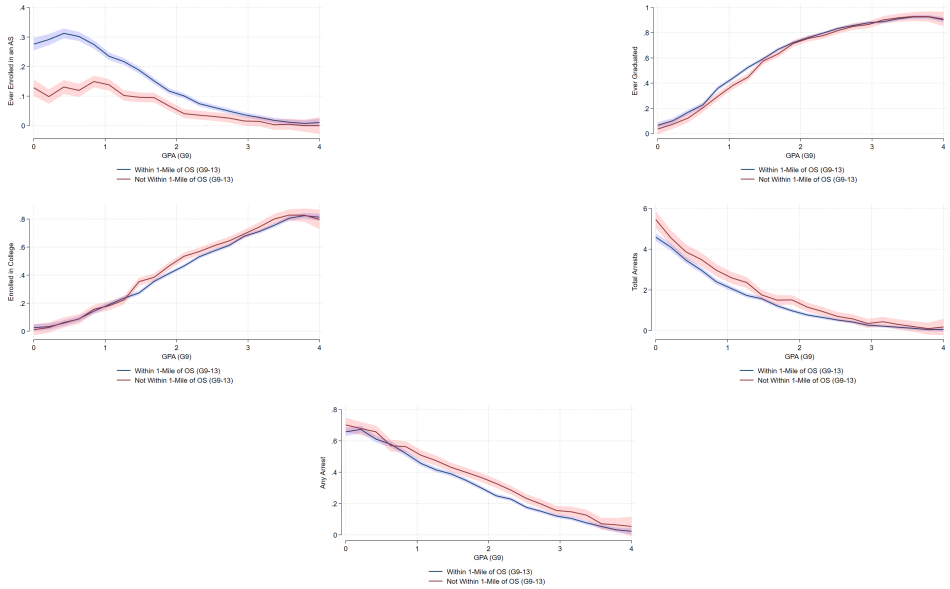
Notes: The y-variable takes on a value of one if a student is ever enrolled in an alternative school. The sample includes students in the 2009-2014 cohorts as defined in Section 4. The distance is based on a student's minimum distance from an alternative school in a given year and the years are limited to those where we expected students to be enrolled in grades 9 to 13. The bins are based on grouping the x-values into 20 equal-sized bins. Then it computes the average y-variable value for each bin. We only include observations within a 5-mile radius.

Figure 4: Likelihood of Enrolling in Alternative School by Distance from Alternative School



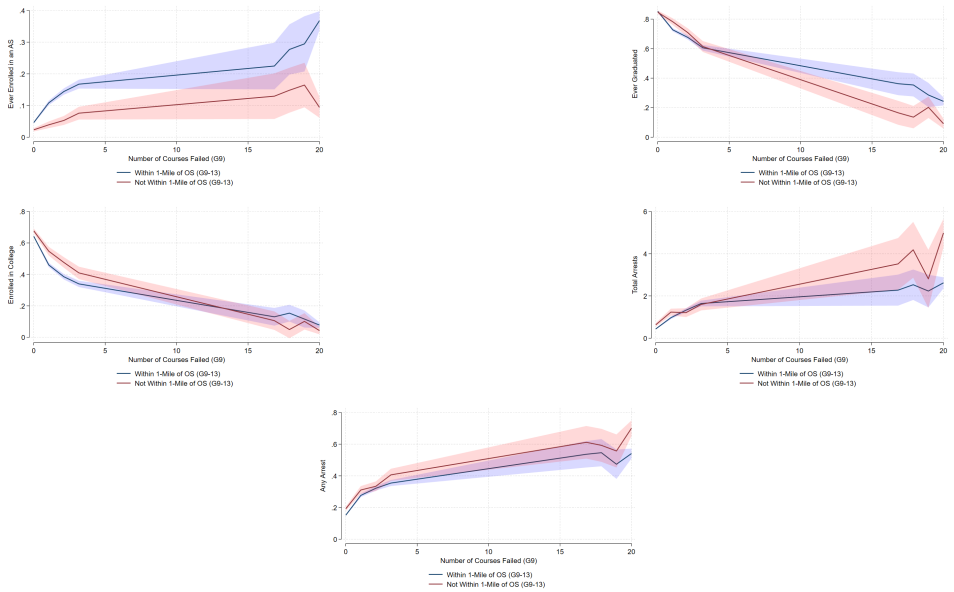
Notes: The outcome is alternative school enrollment and the regression includes cohort, G9 school and zipcode fixed effects (based on Equation (1)). The plot shows the relationship between a students' distance from an alternative school using a local linear regression and the residual likelihood of alternative school enrollment (after including the fixed effects) (bandwidth = 0.075 miles). Similar to Ang (2020), standard errors are calculated using pilot bandwidths equal to 1.5 times the kernel bandwidths. Shaded areas represent 95% confidence intervals. We only include observations within a 5-mile radius.

Figure 5: Exposure to Alternative School and Student Outcomes by G9 GPA



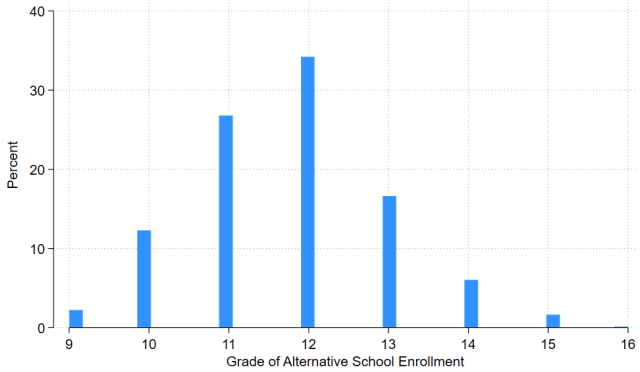
Notes: The plots are based on a local linear regression with GPA as the independent variable. The blue line is for students within 1-mile of an alternative school during their schooling years (G9-13). The red line is for students not within a 1-mile of an alternative school during their schooling years (G9-13), but would have been within a 1-mile had they been in a different cohort (bandwidth = 0.075). Per Fan and Gijbels (1996), standard errors are calculated using pilot bandwidths equal to 1.5 times the kernel bandwidths. Shaded areas represent 95% confidence intervals.

Figure 6: Exposure to Alternative School and Student Outcomes by Number of Courses Failed in G9



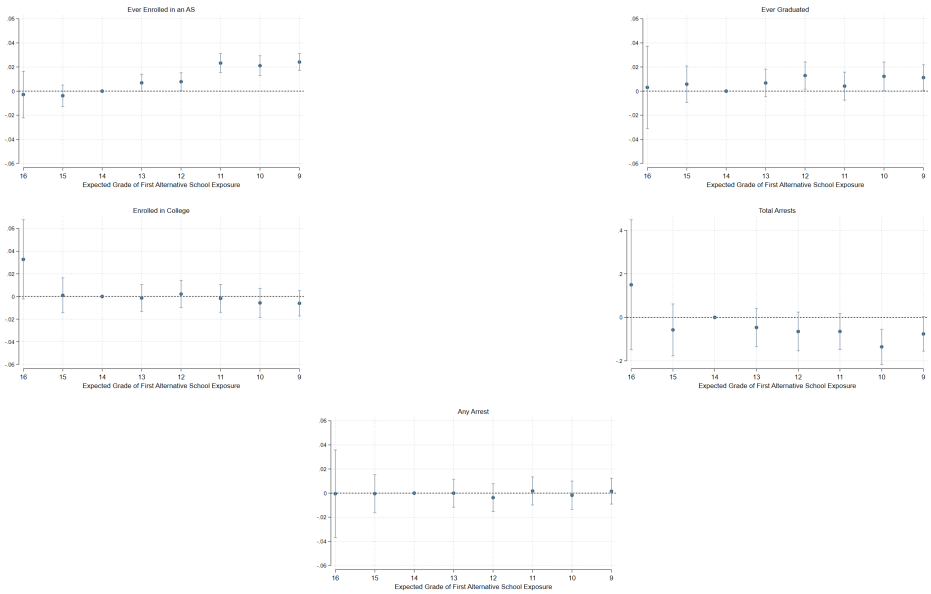
Notes: The plots are based on a local linear regression with number of courses failed as the independent variable. I capped the number of courses failed at 20. The blue line is for students within 1-mile of an alternative school during their schooling years (G9-13). The red line is for students not within a 1-mile of an alternative school during their schooling years (G9-13), but would have have been within a 1-mile had they been in a different cohort (bandwidth = 0.075). Per Fan and Gijbels (1996), standard errors are calculated using pilot bandwidths equal to 1.5 times the kernel bandwidths. Shaded areas represent 95% confidence intervals.

Figure 7: Likelihood of Enrolling in an Alternative School and Expected Grade



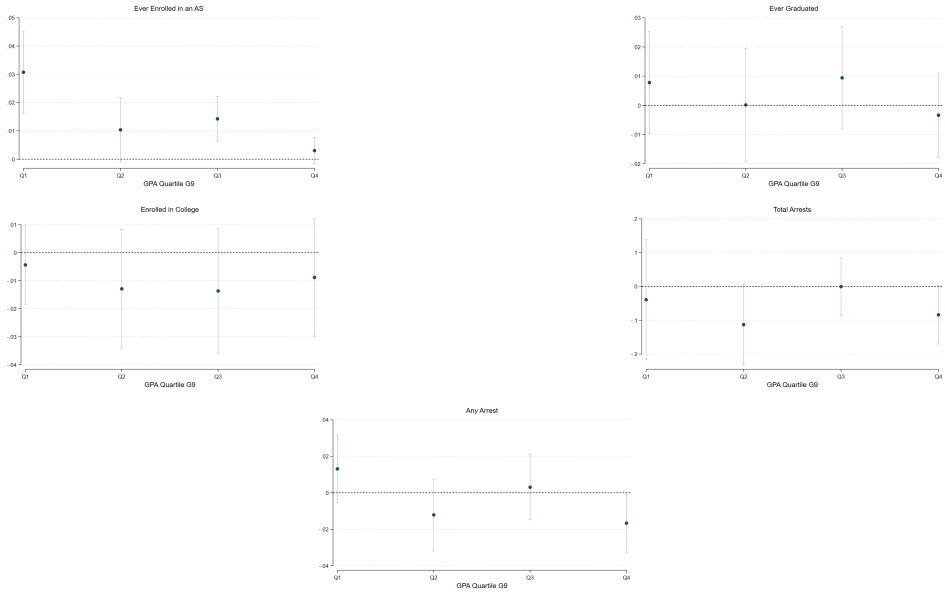
Notes: x-variable captures students' expected grade when they first enrolled in an alternative school. The expected grade is based on years from when a student is first observed in grade 9. The sample includes students in the 2009-2014 cohorts as defined in Section 4. This plot only includes students who ever enroll in an alternative school.

Figure 8: Impact of Residing with a 1 Mile Radius of an Alternative School: Full Sample



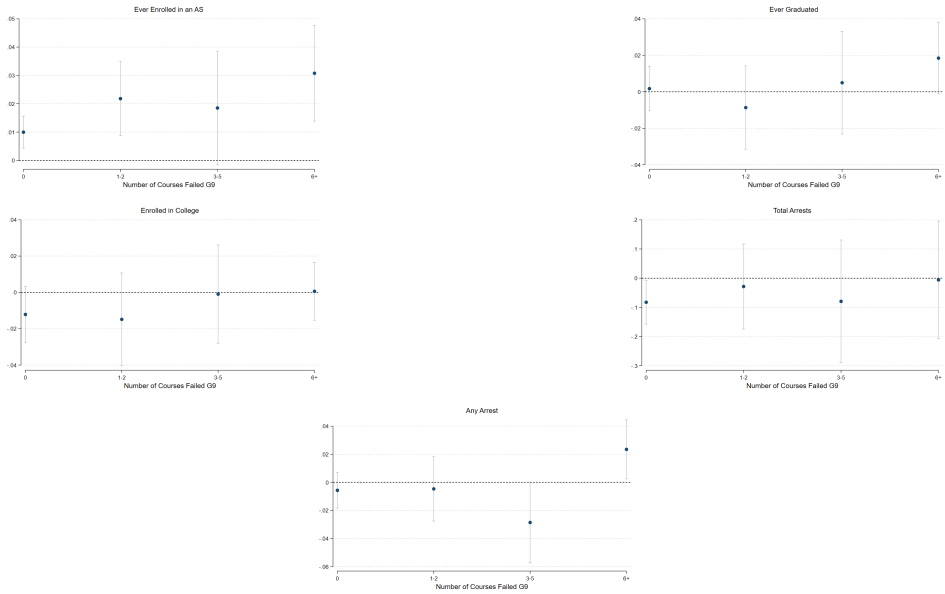
Notes: The estimates are based on equation 2. The coefficients of interest are from an indicator for years of exposure to an alternative school. The outcome takes a value of 1 if a student ever enrolls in an alternative school. For ease of interpretation we used expected grade of exposure as the label instead of years from treatment as in the typical event plot setup. The +5 point in the event plot corresponds to students first exposed to an alternative school in expected grade 9. The -2 point in the event plot corresponds to students first exposed to an alternative school in expected grade 16. Students never exposed to an alternative school are included in the reference group. The 95% confidence intervals are shown in the plot. The standard errors are clustered at the cohort-block level.

Figure 9: Impact of Residing with a 1 Mile Radius of an Alternative School on Alternative School Enrollment: By GPA



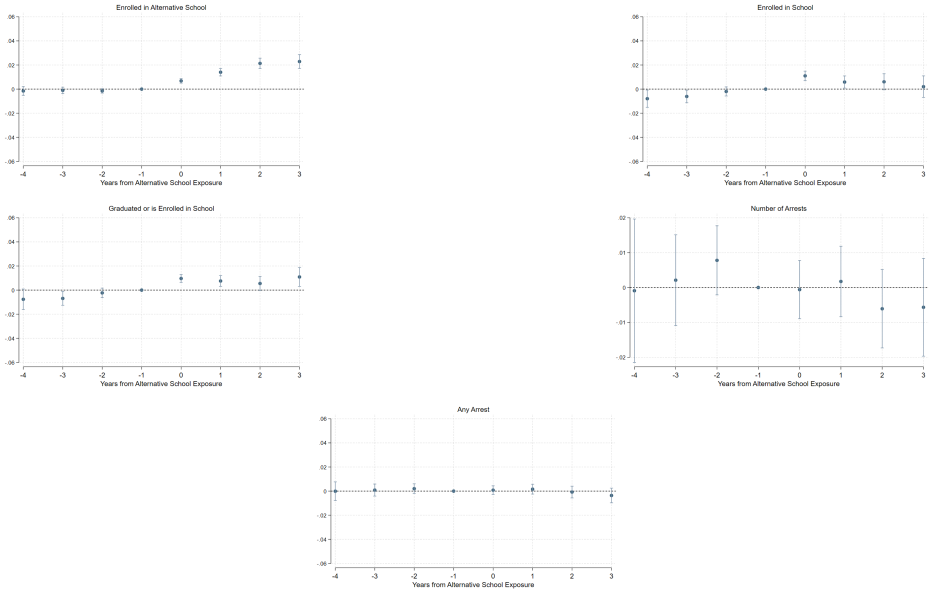
Notes: The estimates are based on equation 2. The outcome takes a value of 1 if a student ever enrolls in an alternative school during expected grades 9 to 13. The 95% confidence intervals are shown in the plot. The standard errors are clustered at the cohort-block level.

Figure 10: Impact of Residing with a 1 Mile Radius of an Alternative School on Alternative School Enrollment: By Number of Courses Failed in G9



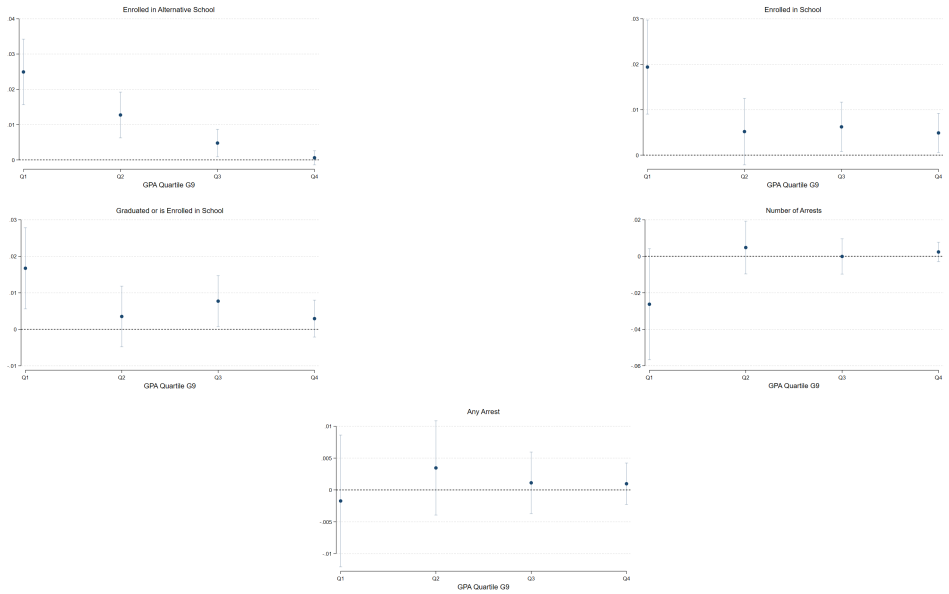
Notes: The estimates are based on equation 2. The outcome takes a value of 1 if a student ever enrolls in an alternative school during expected grades 9 to 13. The 95% confidence intervals are shown in the plot. The standard errors are clustered at the cohort-block level.

Figure 11: Impact of Alternative During G9-13 School Years: Full Sample



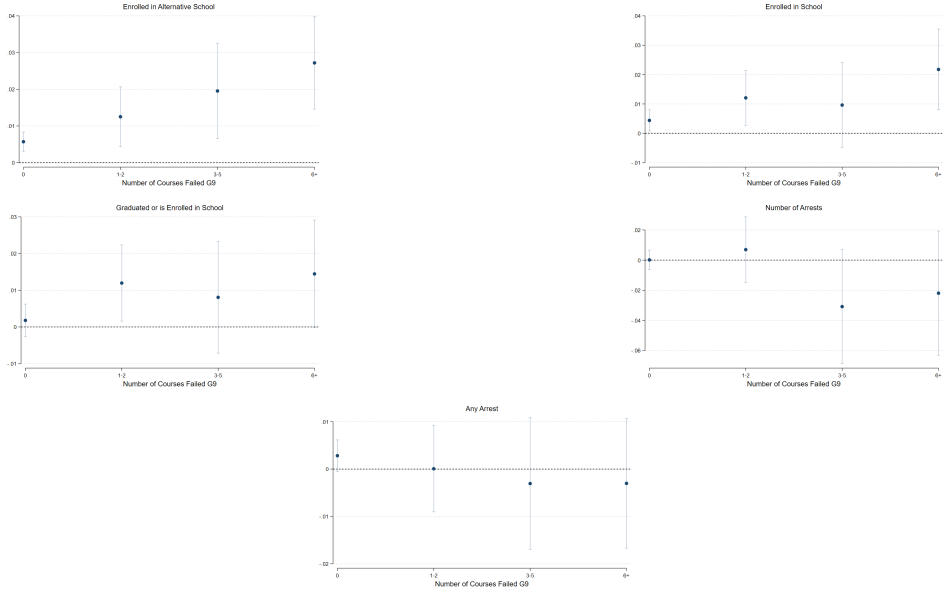
Notes: The sample includes all students who are exposed to an alternative school sometime between expected grade 9 and 16. The estimates are based on including year-cohort-school (school attended in grade 9) and student fixed effects We exclude students who are always treated, i.e., are always within 1-mile of an alternative school. X-axis captures years from when a student is first within 1-mile of an alternative school. Outcomes capture student status in a given year; alternative school enrollment, any school enrollment and arrests takes a value of 1 if a student is enrolled in an alternative school, any school or arrested in a given year. Graduation takes a value of 1 the year a student graduates, and every year after that.

Figure 12: Impact of Alternative During G9-13 School Years: By G9 GPA



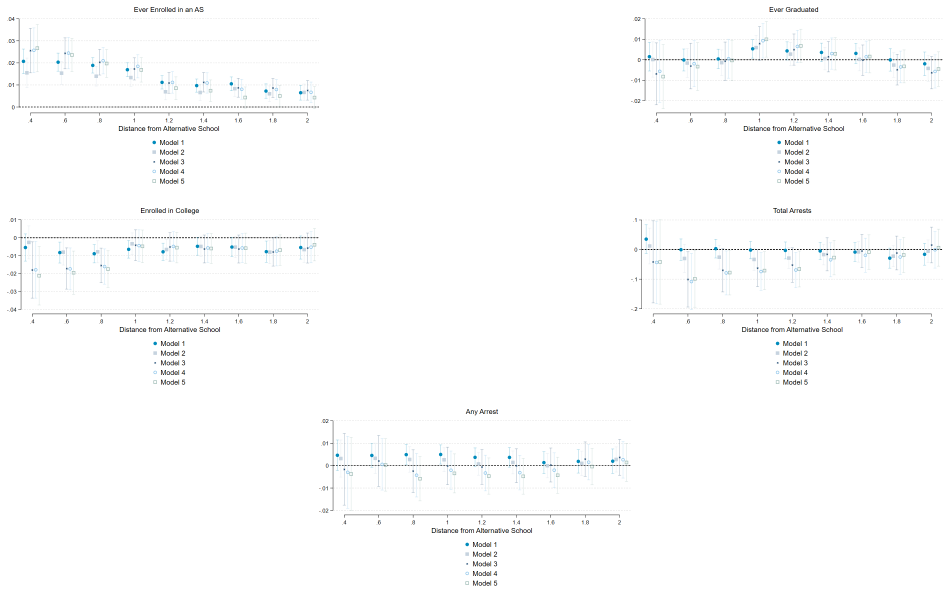
Notes: The estimates are based on Equation(3) for each GPA quartile based on students' grade 9 GPA.

Figure 13: Impact of Alternative During G9-13 School Years: By Number of Courses Failed in G9



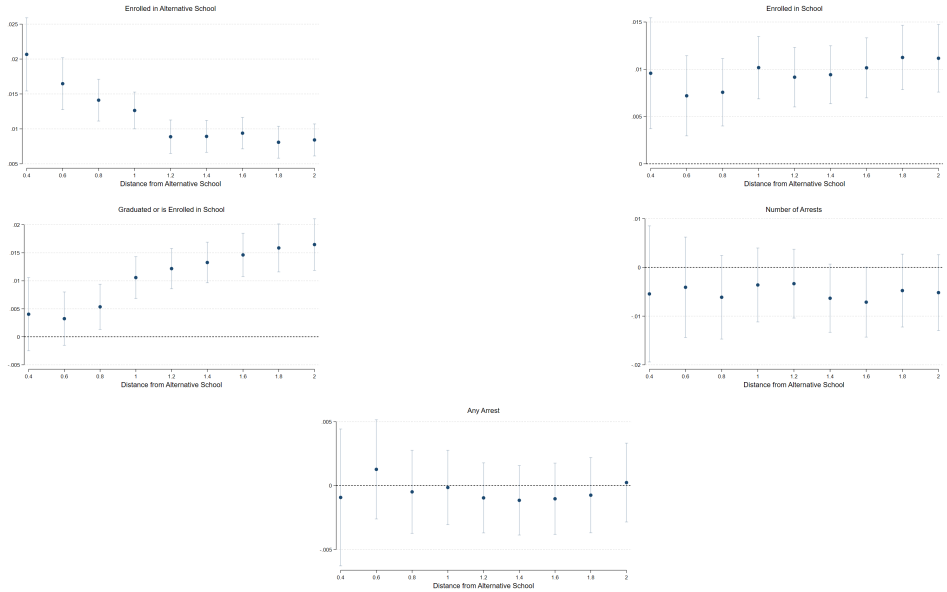
Notes: The estimates are based on Equation(3) by number of courses student failed in grade 9.

Figure 14: Sensitivity to Distance Cutoff Choice: Main Outcomes



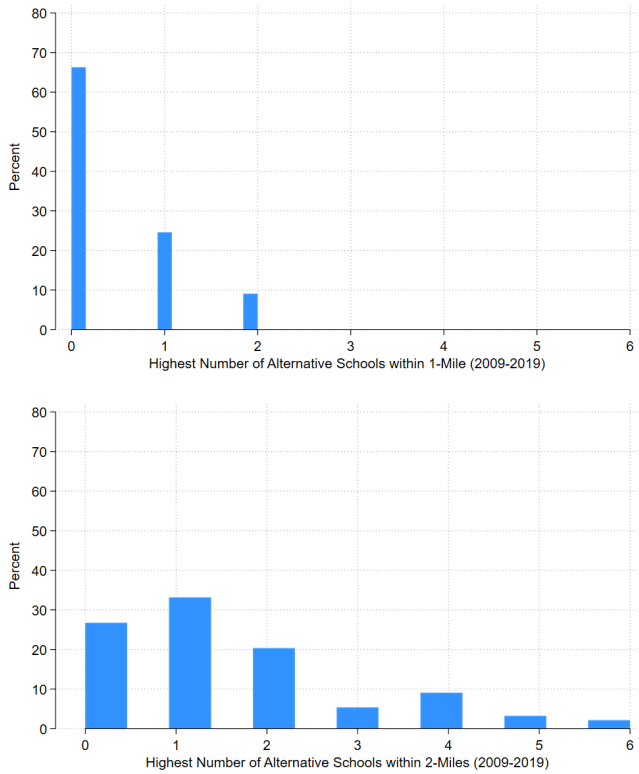
Notes: The estimates are based on models (1) to (5) described in Table 2 using different distance cutoffs. The 95% confidence intervals are shown in the plot. The standard errors are clustered at the cohort-block level.

Figure 15: Sensitivity to Distance Cutoff: Within School Analysis



Notes: The estimates are based on Equation(3) by different distance cutoffs for exposure to alternative schools.

Figure 16: Number of Alternative Schools Ever Within 1- and 2-Miles of a Traditional Public School



Notes: Histograms capture the percentage of public schools that have no more than x number of Alternative Schools within a 1- or 2-mile radius of them between 2009 and 2018.

9 Supplementary Appendix

Table A1: Predictors of AS Enrollment

	Model 1
Number of Days Absent in G9	0.00108***
	(0.0000398)
Number of Member Days in G9	-0.000181***
	(0.0000204)
Total Arrests Prior to and including G9	0.00422***
	(0.000467)
Total Victimization Prior to and including G9	0.0272***
	(0.00147)
Temporary Living Situation (STLS) G9	0.0250***
	(0.00281)
English as Second Language (ESL) G9	-0.0112***
	(0.00236)
Special Education (SPED) G9	-0.0140***
	(0.00175)
GPA G9	-0.0308***
	(0.00105)
Standardized Math ACT Explore Score G9	-0.00449***
	(0.000917)
Standardized Reading ACT Explore Score G9	-0.00507***
	(0.000894)
Courses Failed G9	0.00285***
	(0.000177)
Black Students	0.0107***
	(0.00234)
Hispanic Students	0.00273
	(0.00205)
Male Students	-0.00487***
	(0.00119)
Free/Reduced Lunch G9	0.0116***
	(0.00167)
Age at Start of G9 Entry Year	0.00166
	(0.00105)
Total In-School Suspensions in G9	0.00306***
	(0.000284)
Total Out-of-School Suspensions in G9	0.00348***
	(0.000170)
Missing baseline GPA G9	-0.0832***
	(0.00346)
Missing ACT Explore Test Data	-0.000541
	(0.00195)
Missing Distance	-0.0188**
	(0.00688)
Missing Attendance G9	-0.0152***
	(0.00323)
N	196,643
R2	0.104

Standard errors in parentheses. Includes cohort and G9 school fixed effects. Independent variables are based on G9 values. Alternative school enrollment is based on any enrollment in AS pafter first enrolling in grade 9 in a traditional public school.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A2: Top Predictors of Dropping Out

	MSE	Node Purity
Number of days Absent	0.0294	220.42
GPA	0.0266	173.19
Courses Failed	0.0205	148.84
Number of Member Days	0.0172	113.43
Age at Start of G9	0.0157	183.34
Missing GPA	0.0110	6.37
Free/Reduced Lunch	0.0073	16.93
Standardized Reading ACT Explore Score	0.0056	72.77
Total Arrests Prior to and Including	0.0050	50.38
Missing ACT Test Score	0.0045	14.82
Standardized Math ACT Explore score	0.0042	81.51
Total In-School Suspension	0.0034	62.41
Total Out-of-School Suspension	0.0029	77.94
Male	0.0018	24.25
Black	0.0012	19.01
Hispanic	0.0009	14.60
Special Education (SPED)	0.0005	18.49
Student in Temporary Living Situation (STLS)	0.0004	13.24
English as Second Language (ESL)	0.0001	8.78
Missing Distance from G9 Address	0.0001	1.85
Total Victimization Prior to and Including G9	0.0000	0.00

Based on dropout as the predicted variable using a Random Forest Model with 3 covariates at each split. Even though the outcome is binary, we use a regression to get a continuous probability measure. MSE is based on out-of-bag mean squared error (observations not included when sampling with replacement). The difference between trees using and not using the covariate is used to calculate the MSE. The Node Purity is based on the total decrease in node impurities from splitting on that variable based on residual sum of squares.

Table A3: Top Predictors of Alternative School Enrollment

	MSE	Node Purity
Courses Failed	0.0166	106.85
GPA	0.0165	103.97
Number of days Absent	0.0120	162.92
Missing GPA	0.0092	7.29
Number of Member Days	0.0090	69.14
Standardized Reading ACT Explore Score	0.0049	54.91
Standardized Math ACT Explore Score	0.0041	56.46
Total Out-of-School Suspension	0.0038	61.46
Missing ACT Test Score	0.0032	12.66
Total In-School Suspension	0.0026	39.75
Total Arrests Prior to and Including G9	0.0020	34.34
Age at Start of G9	0.0017	110.00
Black	0.0017	13.09
Free/Reduced Lunch	0.0011	8.09
Male	0.0010	16.27
Hispanic	0.0009	11.45
English as Second Language (ESL)	0.0008	8.83
Special Education (SPED)	0.0008	12.05
Student in Temporary Living Situation (STLS)	0.0004	10.67
Total Victimization Prior to and Including G9	0.0003	25.51
Missing Distance from G9 Address	0.0000	0.89

Similar to Table A2 but with enrollment in alternative schools as the outcome.

Table A4: Differences Between Students within 1-Mile Radius of an Alternative School During Schooling Years and Other Students

	Model 1
Number of Days Absent in G9	.289**
	(.102)
Number of Member Days in G9	-.044
	(.151)
Total Arrests Prior to and Including G9	.062***
	(.007)
Total Victimization Prior to and Including G9	.014***
	(.002)
Temporary Living Situation (STLS) G9	.009***
	(.001)
English as Second Language (ESL) G9	-.006***
	(.001)
Special Education (SPED) G9	.003
	(.002)
GPA G9	-.046***
	(.004)
Standardized Math ACT Explore Score G9	-.053***
	(.004)
Standardized Reading ACT Explore Score G9	-.045***
	(.004)
Courses Failed G9	.033
	(.024)
Black	.094***
	(.002)
Hispanic	-.026***
	(.002)
Male	-.002
	(.003)
Free/Reduced Lunch G9	.033***
	(.002)
Age at Start of G9 Entry Year	.011***
	(.003)
Total In-School Suspensions in G9	-.014
	(.011)
Total Out-of-School Suspensions in G9	.089***
	(.02)
N	196643

Standard errors in parentheses. Standard errors are clustered at the block-cohort level. The coefficients capture the average difference between students within a 1-mile radius of an alternative school during expected grades 9 to 13 and students who are not within a 1-mile radius of an alternative school during those school years. The regression includes grade 9 entry school and cohort fixed effects. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A5: Characteristics of Students Within 1-Mile of an Alternative School Relative to Students Never and Later Within 1-Mile

	Never	After G9-13	During G9-13
Ever Enrolled in an Alternative School G9	.06	.059	.106
Number of Days Absent G9	17.894	26.724	19.764
Number of Member Days G9	159.857	152.416	159.799
Total Arrests Prior to and Including G9	.217	.341	.255
Total Victimization Prior to and Including G9	.097	.095	.133
Temporary Living Status (STLS) G9	.037	.045	.061
English as Second Language (ESL) G9	.08	.046	.064
Special Education (SPED) G9	.145	.154	.156
GPA G9	2.317	1.947	2.196
Standardized Math ACT Explore score G9	.143	-.035	-.087
Standardized Reading ACT Explore score G9	.126	-.056	-.09
Courses Failed G9	2.869	4.314	2.759
Black Students	.378	.619	.588
Hispanic Students	.432	.342	.368
Male	.513	.505	.508
Free/Reduced Lunch G9	.762	.830	.852
Age at start of G9 Entry Year	15.76	15.853	15.76
Total In-School Suspensions in G9	.511	.812	.524
Total Out-of-School Suspensions in G9	1.059	1.653	1.418
Number of Students	119058	13525	64180

Each column summarizes the average student characteristics in each group. Column (1) captures students who are never within a 1-mile radius of an alternative school during or after schooling years (G9-13). Column (2) captures student who would have been within 1-mile radius of an alternative school had it opened earlier. Column (3) captures students who are within 1-mile radius of an alternative school at some point during their schooling years G9-13.

Table A6: Difference in G9 Baseline Student Characteristics Between Students Within 1-mile Who Enroll in an AS and Students Who are not Within 1-mile of an alternative school and Enroll in an Alternative School

	Model 1
Number of Days Absent G9	-0.0000415
	(0.000189)
Number of Member Days G9	0.000298*
	(0.000126)
Total Arrests Prior to and Including G9	0.00190
	(0.00277)
Total Victimization Prior to and Including G9	0.00690
	(0.00631)
Temporary Living Situation (STLS) G9	-0.0229
	(0.0128)
English as Second Language (ESL) G9	0.0389*
	(0.0170)
Special Education (SPED) G9	-0.0156
	(0.0105)
GPA G9	0.0110
	(0.00753)
Standardized Math ACT Explore score G9	-0.00119
	(0.00602)
Standardized Reading ACT Explore Score G9	-0.0126
	(0.00679)
Courses Failed G9	0.000196
	(0.000772)
Black students	0.195***
	(0.0225)
Hispanic students	0.0374
	(0.0210)
Male students	0.00942
	(0.00795)
Free/Reduced Lunch G9	0.0215
	(0.0141)
Age at Start of G9 Entry Year	0.00808
	(0.00701)
Total In-School Suspensions in G9	-0.00272*
	(0.00118)
Total Out-of-School Suspensions in G9	-0.00133*
	(0.000638)
N	14650

Similar to Table A4 but limited to students who enroll in an alternative school. The coefficients capture the average difference between students who enroll in an alternative school and are within a 1-mile radius of an alternative school during expected grades 9 to 13 relative to students who enroll in an alternative school, but are not within a 1-mile radius of an alternative school during those school years. The regression includes grade 9 entry school and cohort fixed effects. Standard errors in parentheses. Standard errors are clustered at the block-cohort level. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A7: Who Enrolls in an AS Among Students who are Never, After and During School years within 1-Mile of an AS?

	Never	After G9-13	During G9-13
Number of Days Absent G9	36.723	42.763	36.518
Number of Member Days G9	159.148	153.89	157.816
Total Arrests Prior to and Including G9	.439	.42	.531
Total Victimization Prior to and Including G9	.229	.128	.26
Temporary Living Status (STLS) G9	.098	.077	.11
English as Second Language (ESL) G9	.062	.03	.06
Special Education (SPED) G9	.178	.159	.179
GPA G9	1.313	1.257	1.373
Standardized Math ACT Explore Score G9	-.422	-.329	-.505
Standardized Reading ACT Explore Score G9	-.427	-.388	-.508
Courses Failed G9	7.423	7.288	6.44
Black Students	.549	.702	.685
Hispanic Students	.382	.293	.299
Male	.545	.504	.552
Free/Reduced Lunch G9	.901	.896	.924
Age at Start of G9 Entry Year	15.82	15.839	15.868
Total In-School Suspensions in G9	1.505	1.532	1.249
Total Out-of-School Suspensions in G9	3.537	3.9	3.637
Number of Students	7153	792	6786

Similar to Table A5 but limited to students who ever enroll in an alternative school.

Table A8: Impact of Residing within a 1-Mile Radius of an Alternative School [Excluding Students Around 1-2 Miles of an Alternative School]: Full Sample

	Model 1	Model 2	Model 3	Model 4	Model 5
Enrolled in an Alternative School	0.0161***	0.0151***	0.0150***	0.0152***	0.0140***
	(0.00260)	(0.00268)	(0.00279)	(0.00304)	(0.00312)
F-Stat	38.17	31.67	28.99	25.11	20.19
Control Mean	0.0500	0.0500	0.0500	0.0500	0.0500
N	118676	118688	118796	118676	118688
Graduated High School	0.00614	0.00628	0.00982*	0.0109*	0.0128**
	(0.00405)	(0.00424)	(0.00457)	(0.00480)	(0.00488)
Control Mean	0.630	0.630	0.630	0.630	0.630
N	118676	118688	118796	118676	118688
Enrolled in College	-0.0120**	-0.00737+	-0.00318	-0.00384	-0.00345
	(0.00434)	(0.00431)	(0.00475)	(0.00505)	(0.00513)
Control Mean	0.490	0.490	0.490	0.490	0.490
N	118676	118688	118796	118676	118688
Total Arrests	-0.0672*	-0.0569*	-0.0889**	-0.100**	-0.102**
	(0.0271)	(0.0264)	(0.0337)	(0.0351)	(0.0349)
Control Mean	0.780	0.780	0.780	0.780	0.780
N	118676	118688	118796	118676	118688
Any Arrests	-0.000633	-0.000514	-0.00440	-0.00619	-0.00599
	(0.00395)	(0.00372)	(0.00458)	(0.00482)	(0.00486)
Control Mean	0.200	0.200	0.200	0.200	0.200
N	118676	118688	118796	118676	118688
Zip-code Fixed Effect	X				
Tract Fixed Effect		X			
Zip-code Trend Lines				X	
Tract Trend Lines					X
Ever within 1-Mile Radius			X	X	X

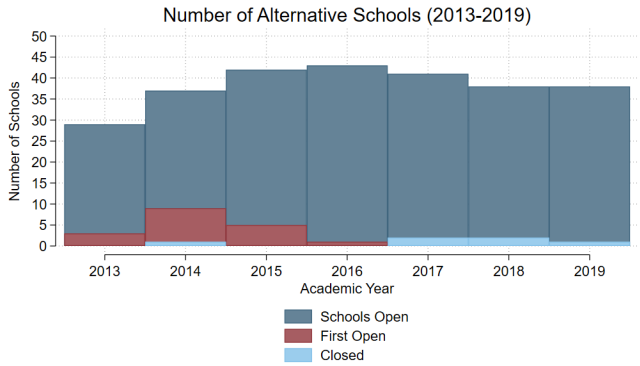
Standard errors in parentheses. Standard errors are clustered at the block-cohort level. Models (1) and (2) are based on Equation (1). Model (3) is based on Equation (2), and Models (4) and (5) additionally include neighborhood time trends. The outcomes are listed in each column and the coefficients are based on the β_1 estimate from each of the Equations. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A9: Impact of Exposure to AS [Excluding Students Around 1-2 Miles of an Alternative School]: Full Sample [Within Student Analysis]

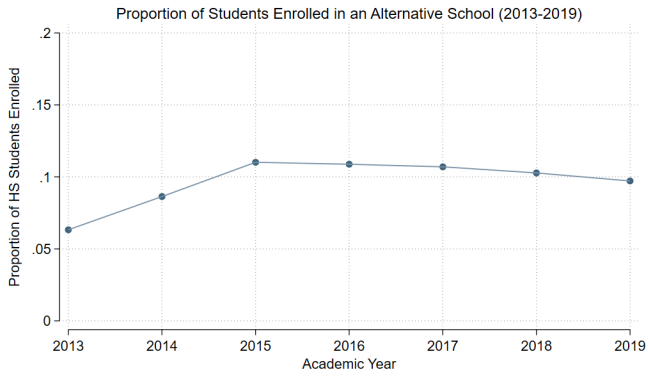
	Enrolled in AS	Enrolled	Grad/Enrolled	Total Arrests	Any Arrests	Absent
Post Exposure to AS	0.0130***	0.0160***	0.0183***	-0.00664	0.000320	-0.00230
	(0.00179)	(0.00274)	(0.00315)	(0.00566)	(0.00233)	(0.00170)
F-Stat	52.70					
Control Mean	.01	.85	.85	.20	.11	.16
N	1792085	1792085	1792085	1792085	1792085	1119399

Standard errors in parentheses. Standard errors are clustered at the block-cohort level. The Absent outcomes captures the proportion of students' member days where they were absent. The arrest outcomes are based on arrests in a given school year. Estimates are based on Equation (3). The control mean is based on students at $t = -1$ who are at some point within a 1-mile radius of an alternative school. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure A1: Number and Enrollment in Alternative Schools Over Time (Including YCCS Alternative Schools)



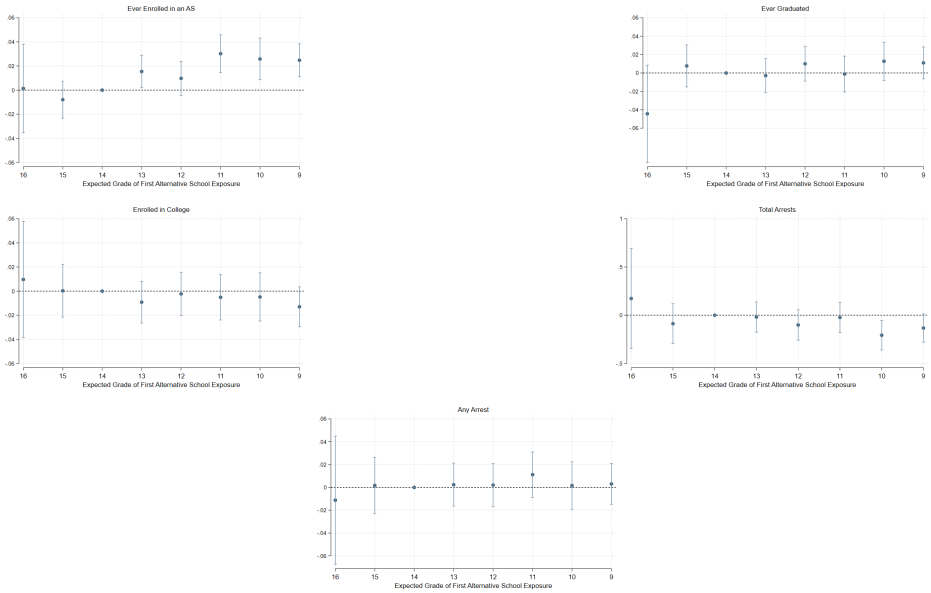
(a) Change in the Number of Alternative Schools



(b) Proportion of Students Enrolled in an Alternative School

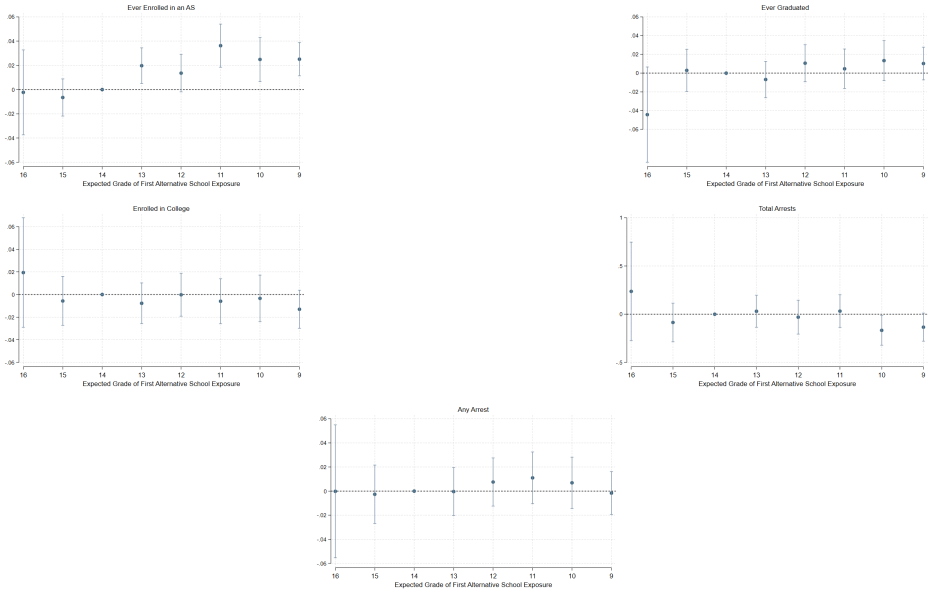
Notes: Similar to Figures 1 but including YCCS schools that we have data for only starting in 2013. Plots capture change in number of schools and enrollment in alternative schools including YCCS between 2013 and 2019. Panel (a) shows the number of alternative schools. Panel (b) shows the change in the proportion of the total number of students enrolled in CPS public schools who are enrolled in an alternative school.

Figure A2: Impact of Residing with a 1 Mile Radius of an Alternative School on Alternative School Enrollment: Students Who Performed in the Bottom Half of Grade 9 GPA



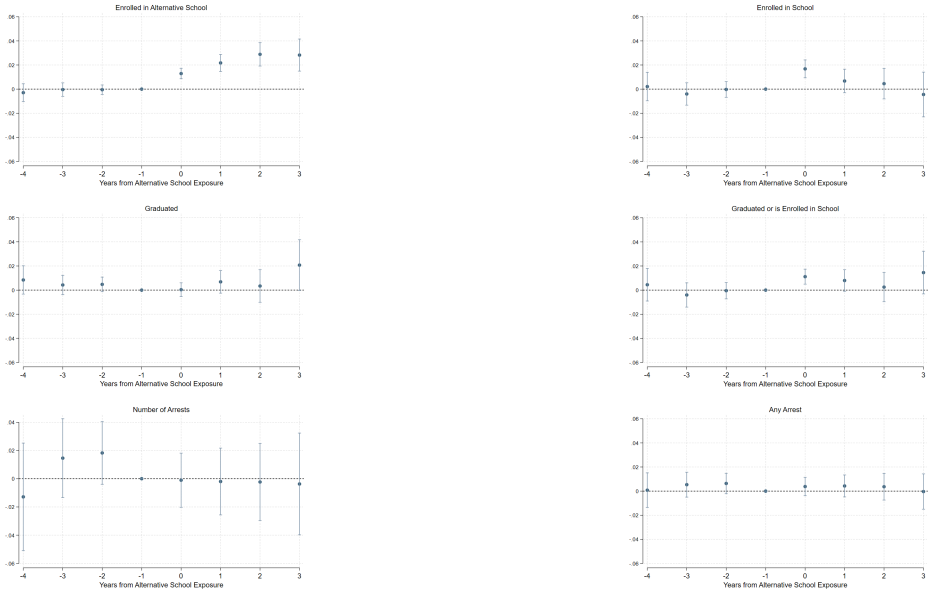
Notes: Similar to Figure (8), but the sample is limited to students who scored in the bottom half of G9 GPA.

Figure A3: Impact of Residing with a 1 Mile Radius of an Alternative School on Alternative School Enrollment: Students Who Failed At Least One Course in G9



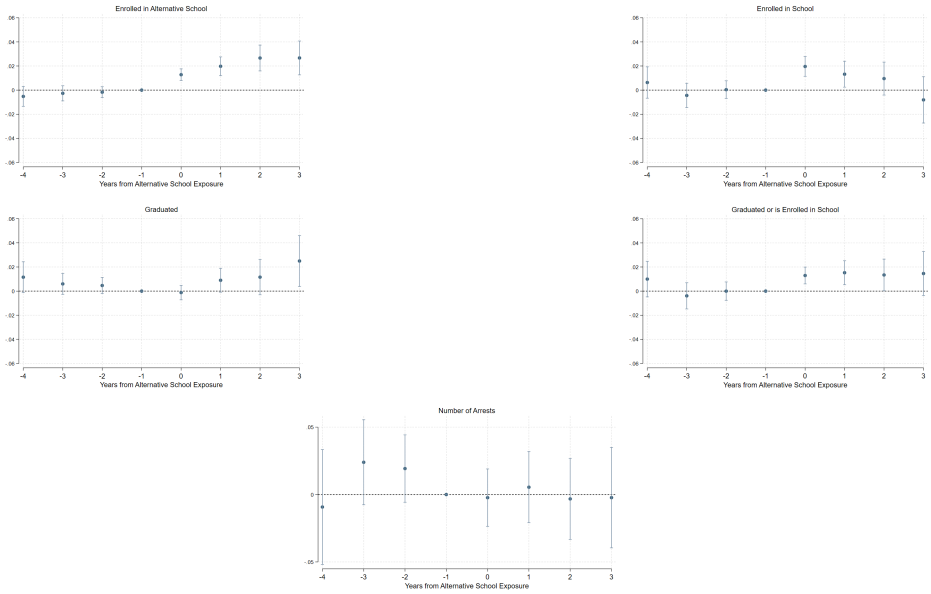
Notes: Similar to Figure (8), but the sample is limited to students who failed at least one course in G9.

Figure A4: Impact of Alternative During G9-13 School Years: Bottom Half of their G9 GPA distribution



Notes: The sample includes all students who are exposed to an alternative school sometime between expected grade 9 and 16. The estimates are based on including year-cohort-school (school attended in grade 9) and student fixed effects. We exclude students who are always treated, i.e., are always within 1-mile of an alternative school. X-axis captures years from when a student is first within 1-mile of an alternative school. Outcomes capture student status in a given year; alternative school enrollment, any school enrollment and arrests take a value of 1 if a student is enrolled in an alternative school, enrolled in any school, or arrested in a given year, respectively. Graduation takes a value of 1 the year a student graduates, and every year after that.

Figure A5: Impact of Alternative During G9-13 School Years: Students who Failed At Least One Course in G9



Notes: The sample includes all students who are exposed to an alternative school sometime between expected grade 9 and 16. The estimates are based on including year-cohort-school (school attended in grade 9) and student fixed effects We exclude students who are always treated, i.e., are always within 1-mile of an alternative school. X-axis captures years from when a student is first within 1-mile of an alternative school. Outcomes capture student status in a given year; alternative school enrollment, any school enrollment and arrests takes a value of 1 if a student is enrolled in an alternative school, any school or arrested in a given year. Graduation takes a value of 1 the year a student graduates, and every year after that.