The Competitive Effect of Charter Schools on Traditional Public Schools in Oakland Unified School District

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ABSTRACT

The rise of charter schools in the United States has led to questions about their effect on traditional public schools. I use panel data starting in 2003 to examine the impact of charter school openings on traditional public schools in Oakland Unified School District. I find that the entry of charter schools is associated with changes in student composition, most notably an increase in traditional public schools in the percentage of African American students, Hispanic or Latino students, economically disadvantaged students, and students with a disability. I find that charter school entry is associated with a statistically significant decrease in student test scores in traditional public schools. Lastly, I find that charter school entry is associated with a decrease in student-teacher ratio and an increase in full-time equivalent teachers in traditional public schools.

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I. INTRODUCTION

Charter schools have been one of the most popular policy solutions to education struggles in the United States in the past few decades. The first U.S. charter schools were approved in the 1990s, and have since expanded to educate over 3.7 million students, representing about 8% of public school students in the United States (Jacobs & Veney, 2024). Because charter schools are funded publicly, their impact on the education landscape is a significant policy concern. Advocates argue that charter schools can benefit the education system both by providing a more tailored education program than traditional public schools (TPSs), and by increasing the competitive pressure that TPSs feel to attract students. The first claim has been widely studied, and results tend to vary based on the efficacy of the individual charter school (Betts and Tang, 2016). The second claim has received more attention in recent years, with similarly mixed results. A 2022 meta-analysis found a small positive effect of competition on student achievement, though the results varied by the type of competitive policy and student demographics (Jabbar et al. 2022). Ultimately, because the majority of public school students do not attend charter schools, the impact of charter schools on non-charter school students may be the most significant measure of the value of charter schools to public school students (McCabe and Vinzant 1999).

In this paper I examine the competitive effects on TPSs caused by the introduction of charter schools in Oakland Unified School District (OUSD), in Oakland, California. I use school-level data from the California Department of Education, which includes annual information from OUSD, including enrollment figures, standardized test scores, and demographics, as well as school-level data from the National Center for Education Statistics for teacher-related data. I employ a difference-in-differences design that tests the impact of charter entry on traditional public school outcomes such as student composition, test scores, and staffing.

I find charter entry to be associated with a statistically significant increase in the percentage of lower-scoring students in TPSs. The percentages of economically disadvantaged students, African American students, Hispanic or Latino students, and students whose parents did not graduate from high school all increase. The percentages of White, Asian, and gifted and talented students all decrease. I also find that charter entry is associated with a roughly 3.5 percentage point decrease over eight years in the percentage of traditional public school students meeting the standardized test proficiency benchmark. This effect is slightly smaller in female and economically disadvantaged students, and slightly larger in male, non-economically disadvantaged, Hispanic or Latino, and African American students. However, the average cumulative effect over eight years is negative for all subgroups with sufficient data to test. I find charter entry to be associated with an increase in the number of full-time equivalent teachers in traditional public schools, with an average cumulative effect of about 2 over eight years. Charter entry is also associated with a decrease in student-teacher ratio of more than 2.5 over eight years.

The primary way that I hope to add to the existing literature on charter competition is by studying the competitive effects of charter schools in a district in which this topic has not previously been studied. Many of the papers in the field acknowledge that the competitive effects of charter schools depend on factors that vary greatly from district to district, such as the quality of public schools, parent sentiment, socioeconomic factors, and others. Adding to the number of districts that have been studied allows us to create a more complete picture of the characteristics of a district that impact charter school efficacy. Another contribution of this paper is that the methodology is very widely applicable. Much of the literature on the effects of charter school competition on traditional public schools either uses student-level data, or leverages a location-specific instrumental variable, or both. This significantly limits the number of districts that can be studied, because states often do not publicly share student-level data, and relevant instruments are often difficult to identify and unique to each district. The methodology in this paper can be applied to every district in California, using only publicly available data.

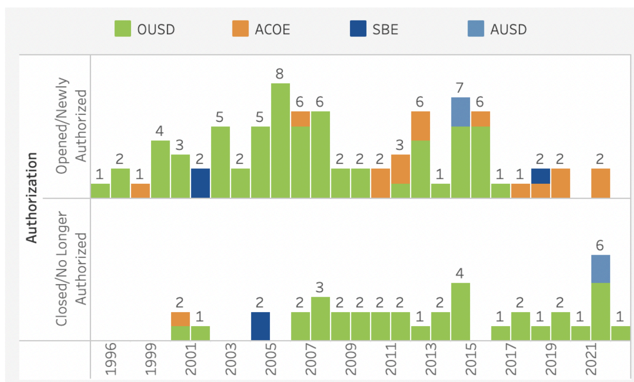
In the following sections, I first examine the competitive landscape for Oakland schools and summarize relevant literature on the impacts of charter schools. I review the data sources used and the methodology employed. Finally, I report the key results from my analysis and discuss the implications and limitations of these findings.

II. BACKGROUND

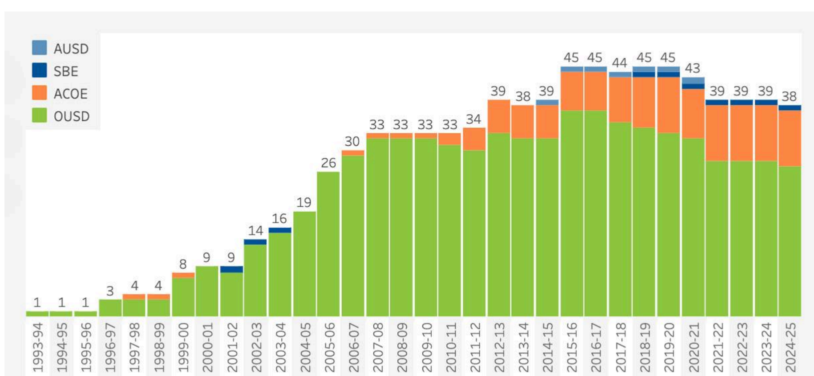
The argument that increased school choice improves the education landscape is often traced back to Milton Friedman's 1955 paper, "The Role of Government in Education" (Friedman 1955). Friedman argues that while the government should be responsible for ensuring access to education, it should not have a monopoly on the provision of education. He proposes that the government issue vouchers that can be used by families to pay for private schools. Friedman reasons that vouchers increase parental choice by allowing lower income families to afford private schools in addition to public schools, which enables families to choose a school that fits their student's needs. He also argues that vouchers increase competition in the education market by expanding the set of competitors, which incentivizes public schools to improve to retain students (Friedman 1955). Friedman's arguments in favor of vouchers apply to a variety of forms of school choice, such as open enrollment policies, charter schools, and magnet schools, all of which are designed to increase school choice and competition in the education system (McCabe and Vinzant 1999).

Ray Budde, an education professor, was the first to propose a model resembling modern charter schools. In his 1988 book, "Education by Charter: Restructuring School Districts," Budde proposed that teachers should be empowered to form agreements with their school boards to operate schools. He argued that this increased autonomy for teachers would allow for more experimentation and innovation without the burdens of excessive bureaucracy. Charter schools would not be subject to the same restrictions as traditional public schools, though they would still be accountable for student achievement (EdSource 2004) (Saulny 2005). His ideas quickly took hold, and the first law permitting the establishment of charter schools in the United States was passed in 1991 in Minnesota (National Charter School Resource Center n.d.). California became the second state to allow charters in 1992, and the first Oakland charter school opened soon after in 1993 (Oakland Unified School District n.d.). Since then, the influence of charter schools in the US has continued to grow. From the 2019-2020 school year to the 2023-2024 school year, nationwide charter school enrollment increased by over 400,000, while district public schools lost over 1.8 million students (Jacobs & Veney 2024).

In California, charter schools are authorized by a public school district, a county board of education, or a state board of education. Through 2016, most charters in Oakland were approved by Oakland Unified School District. Since 2017, most charters have been approved by the Alameda County Office of Education (See Figure 1a). Charter schools are approved for a term (typically 5 years), during which the charter school can operate. The charter must be renewed if a charter school wishes to continue to operate beyond the end of its term (Oakland Unified School District n.d.). Charter schools receive the same per-pupil funding as public schools in California, so if a student leaves a traditional public school to attend a charter school, the public funding is said to "follow the student" (EdSource 2004). However, charter schools do not receive separate funding for start-up costs such as facilities, so charter schools tend to receive less total funding per student compared to traditional public schools (EdSource 2004). As of 2019, Assembly Bill 406 requires California charter schools to be non-profit (California School Boards Association 2018).



(a)
Charter Approvals and Expirations



(b)
Charter Totals

FIGURE 1. CHARTER SCHOOLS BY YEAR IN OAKLAND UNIFIED SCHOOL DISTRICT

Notes: Data from OUSD Dashboards. Colors correspond to the body that approved the charter. OUSD = Oakland Unified School District, ACOE = Alameda County Office of Education, SBE = State Board of Education, AUSD = Alameda Unified School District.

The number of charter schools in Oakland grew quickly, from 14 in 2003-04 to a peak of 45 in 2015-16 (Figure 1b). The number of charter schools and the number of students in charter schools has fallen slightly since the pandemic, and new authorizations have slowed (Figure 1). There are currently 38 charter schools and 77 district-run schools in Oakland (Figure 1b; Oakland Unified School District 2024). Overall public school enrollment in Oakland has remained largely unchanged since 2003 (the first year for which I have test score data), hovering around 50,000 students (Figure 2). However, when enrollment figures are separated into charter and district-run schools, the picture changes significantly. Enrollment in charter schools has grown steadily over this period, from under 3,000 in 2003-04 to nearly 17,000 in 2019-20. Meanwhile, district-run schools have lost over 13,000 students since 2003-04 (Figure 2).

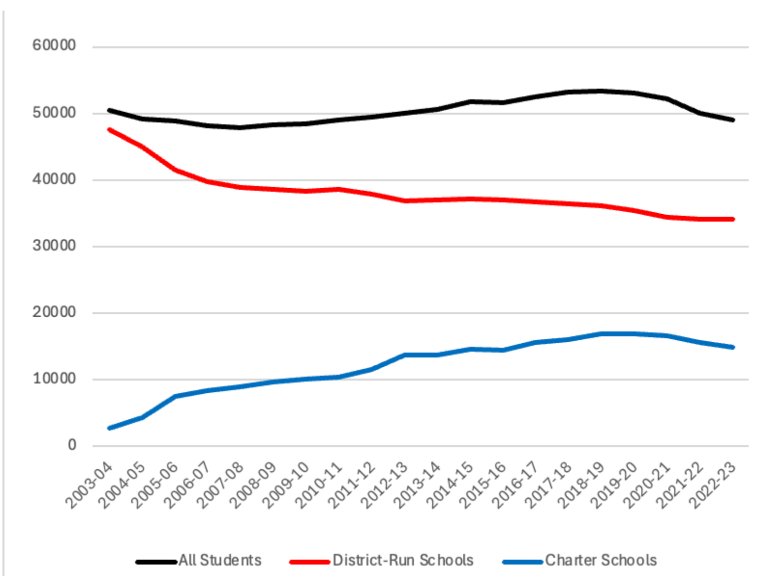


FIGURE 2. OAKLAND PUBLIC SCHOOL ENROLLMENT IN CHARTER AND DISTRICT RUN SCHOOLS

Notes: Data from the California Department of Education (CDE). “All Students” refers to students in Oakland public schools. “District-Run Schools” refers to traditional public schools.

OUSD currently has a limited school-choice policy, which means that students can apply to any public school, but entrance is determined by a variety of factors (OUSD). The most notable factor is that students in the attendance area of a traditional public school are given admissions priority over those who are not in the attendance area. Other factors include siblings already enrolled in the school, parents working for the school, foster status, and Oakland residence (Oakland Unified School District n.d.). Historically, about half of Oakland students attend a school in their attendance area (Oakland Unified School District n.d.).

In April 2021, the OUSD Board of Education passed Board Policy 5115 (BP 5115). The policy was designed to stabilize enrollment in OUSD schools, which had fallen significantly in the preceding years (Oakland Unified School District 2021). The District's incentives were clear; OUSD's revenue is primarily driven by enrollment, and declining enrollment had led to budget cuts, layoffs, and school closures. BP 5115 explicitly cites school competition as a threat to OUSD enrollment (Oakland Unified School District 2021). The "No Resources for Competing Systems" section prevents Oakland charter and private schools from being included in OUSD school maps, family guides, enrollment fairs, and other similar resources. The Board also prohibits OUSD staff from referring students to competing schools (Oakland Unified School District 2021). These restrictions presumably hurt OUSD students and parents; Oakland residents seeking information about Oakland schools will not be able to learn about the full set of public options from OUSD resources or staff. On the other hand, some aspects of BP 5115 likely benefit students; for example, the policy outlines plans to make enrollment more accessible for families, increase racial and ethnic diversity, and improve outreach to families (Oakland Unified School District 2021). Board Policy 5115 indicates that OUSD did in fact feel competitive pressure from charter schools, and provides evidence that the District acted in response to this pressure. What is less clear is whether this competitive pressure benefits OUSD students. Does the positive impact of increased outreach and streamlined enrollment outweigh the negative impact of reduced information about competitors and the potential for money wasted on marketing? Could competition be the solution to an ailing school district, or do students receive a better education when schools' incentives are aligned?

III. LITERATURE REVIEW

There is a great deal of existing literature that informs the question of whether charter schools and other forms of competition improve the public school landscape. As noted earlier, arguments in favor of increased competition generally have their theoretical foundation in Friedman's 1955 paper, "The Role of Government in Education," which argues that when students have more options, schools are forced to improve to attract students (Friedman 1955). Since then, several papers have explored the idea that charter schools might increase competition in school districts. Hoxby (2003) argues that charter schools introduce market-like competition into the school choice landscape, as schools that can achieve the greatest results with the least funding can draw students away from less productive schools. She argues that this process would either force less productive schools to shut down or raise their productivity, ultimately leading to an improved set of choices for students and families. Bifulco and Ladd (2006) note that while charter competition might induce traditional public schools to become more productive, it could also hurt traditional public schools by drawing away funding, high-achieving students, and/or qualified teachers. Arsen and Ni (2011) suggest that charter competition might force traditional public schools to reallocate resources from less productive to more productive expenses.

Empirical evidence on the competitive impact of school choice varies greatly. A 2022 meta-analysis, "Empirical evidence on effects of school choice on student achievement," analyzed three popular forms of school choice: charter schools, private school vouchers, and policies that expand the set of public schools in which students can enroll (generally referred to as "school choice policies" or "open enrollment policies") (Jabbar et al. 2022). The study found small positive overall effects of school choice in general on student achievement, with even smaller relative effects for charter schools specifically (Jabbar et al. 2022) (See for example: Bettinger, 2005; Bifulco & Ladd, 2006; Holmes, DeSimone, & Rupp, 2003; Hoxby, 2003; Ni, 2009; Zimmer & Buddin, 2009; Sass, 2006). Evidence for the ability of charter schools to educate students better than traditional public schools is also mixed. Results have varied greatly, and differences are likely attributable to the individual institutions running charter schools across the country (Betts and Tang 2016). Other papers have explored the impact of charter schools on student composition in traditional public schools. Broadly, these papers have found that charter entry often leads to changes in TPS demographics and increased segregation (Kho, Zimmer, and McEachin 2022; Monarrez, Kisida, and Chingos 2022; Bifulco and Ladd 2007; Renzulli and Evans 2005). Other studies have shown that charter schools often strategically open in areas with demographics that suit their goals, though the specific qualities that make a charter-friendly location vary by school (Glomm, Harris, and Lo 2005; Henig and MacDonald 2002; Bifulco and Buerger 2015; Riel 2021). There are fewer papers that study the impact of charter school competition on TPS resource allocation. Arsen and Ni (2011) find that charter school competition does not cause public school districts to reallocate resources to "achievement-oriented activities." Similarly, Machin and Silva (2017) find little effect of charter schools on student-teacher ratio, per-pupil spending, or staffing levels.

Studies that seek to measure the impact of charter schools on traditional public school student outcomes such as test scores typically must overcome two key challenges. First, researchers need to be able to control for changes in student composition to make a causal claim about the effect of charter schools on traditional public school outcomes. For example, if a charter school opens next to a TPS and the highest scoring students leave the TPS to attend the charter, the scores at the TPS will drop in the next year, even if the TPS makes no changes in response to competitive pressure. The most common solution to this problem is to use student-level data (Jabbar et al. 2022). This allows researchers to only study students that attended the TPS before and after the shock, and therefore control for changes in student composition. In the absence of student-level data, researchers can first check to see if student composition does in fact change (Holden 2014; Ni 2009). If charter entry does not affect student composition, then this confounding variable can be ruled out. If charter entry does affect student composition, then researchers can run regressions controlling for student subgroups (Ni 2009).

The other common and related challenge is that charter location is not always exogenous. In other words, charter schools are not randomly choosing where to locate, and therefore schools near a charter and schools not near a charter cannot be assumed to be comparable (Betts 2009). One common strategy to address the endogeneity of charter location is to use an instrumental variable. Instrumental variables should be correlated with charter location decisions but not correlated with unexplained student outcomes. For example, Imberman (2011) uses the supply of possible school buildings, and Mumma (2022) compares schools near proposed charter sites to schools near actual charter sites (See also: Bettinger 2005).

IV. DATA AND METHODOLOGY

Data Sources

I use three main data sets in my analysis: standardized test and enrollment data from the California Department of Education (CDE); school location, years open, and grades served from the CDE; and student-teacher ratio and full-time equivalent teacher statistics from the National Center for Education Statistics (NCES).

The primary data set is standardized test and enrollment data provided by the CDE. The data is available at the subgroup level, which means that for each school, and each grade level, and each test type, I can observe the test scores for each subgroup of students. Key subgroup categories include economic status, ethnicity, parent education, and disability/special program participation. The test score figures are the percentage of students in each group that meet a certain standard of proficiency. In addition to test scores, the data set also contains enrollment figures, which allows me to calculate student composition changes. While not as detailed as student-level data, subgroup-level data should help to control for changes in student composition at a given school. While I limit my analysis to schools in OUSD, this data exists for all of California, so this methodology could be expanded to include many more districts. The data is available starting in 2003, which gives me a large sample of charter school openings in OUSD. Data is missing for 2014 as California was shifting to a new standardized test, and is missing for 2020 and 2021 due to the pandemic. To protect student anonymity, any cell with a test score value that is drawn from fewer than 10 students is not available. Therefore, we are more likely to see missing data and noisier estimates for subgroups that include fewer students such as White students, Asian students, students whose parents did not graduate from high school, students whose parents graduated from college, students in the Gifted and Talented program, and students with disabilities.

I also use a dataset from the CDE School Data Directory Export with school longitude and latitude, years open, and grade levels served to generate the treatment data. This data allows me to calculate the number of competing charter schools within a given radius of each public school in each year. Competing charter schools are those that serve grade levels that overlap with the public school.

Finally, I use data from the National Center for Education Statistics for student-teacher ratio (STR) and full-time equivalent teachers (FTE) statistics. This data is available at the school level.

Summary Statistics

Student Composition

In the 2023-24 school year, about 47% of OUSD students were Hispanic or Latino, roughly 20% were African American, 11% were White, and 10% were Asian (Oakland Unified School District n.d.). These demographics have shifted significantly in the past 20 years. In the 2003-04 school year, African American students made up over 42% of the study body, while Hispanic or Latino students comprised less than 34%. About 15% of students were Asian, while fewer than 6% were White (Oakland Unified School District n.d.). The percentage of economically disadvantaged students at the median OUSD school has increased from under 70% in 2003 to over 90% in 2024-2025. The percentage of students whose parents did not graduate from high school and the percentage whose parents graduated from college at the median OUSD school have both decreased since 2003, from roughly 11% and 10% respectively to less than 4%.

The percentage of students with a disability at the median OUSD school has increased slightly over this period, from about 9% to 14%. The Gifted and Talented program expanded from serving about 5% of students in 2003 to about a third in its final year in 2013.

Test Scores

The percentage of students meeting the standard of proficiency each year in OUSD ranges from about 20% to 30%. Male and female scores are similar, though female students consistently score a few percentage points higher. Non-economically disadvantaged students score significantly higher than economically disadvantaged students, and the gap between the two groups has grown over time to over 40 percentage points in 2023-24. White students are the highest scoring ethnicity, averaging over 60% of students meeting the standard, while Asian students average roughly 50% of students meeting the standard. African American and Hispanic or Latino students score similarly with 10-20% of students meeting the standard in most years, though Hispanic/Latino students have scored a few percentage points higher since 2015. Students whose parents graduated from college score consistently higher than students whose parents did not graduate from high school, averaging about 50% compared to less than 20%. Gifted and talented students averaged about 60% proficiency before the program was discontinued between standardized tests in 2014. Students with a disability average just above 10% proficiency. See Figure 3 below for more detail.

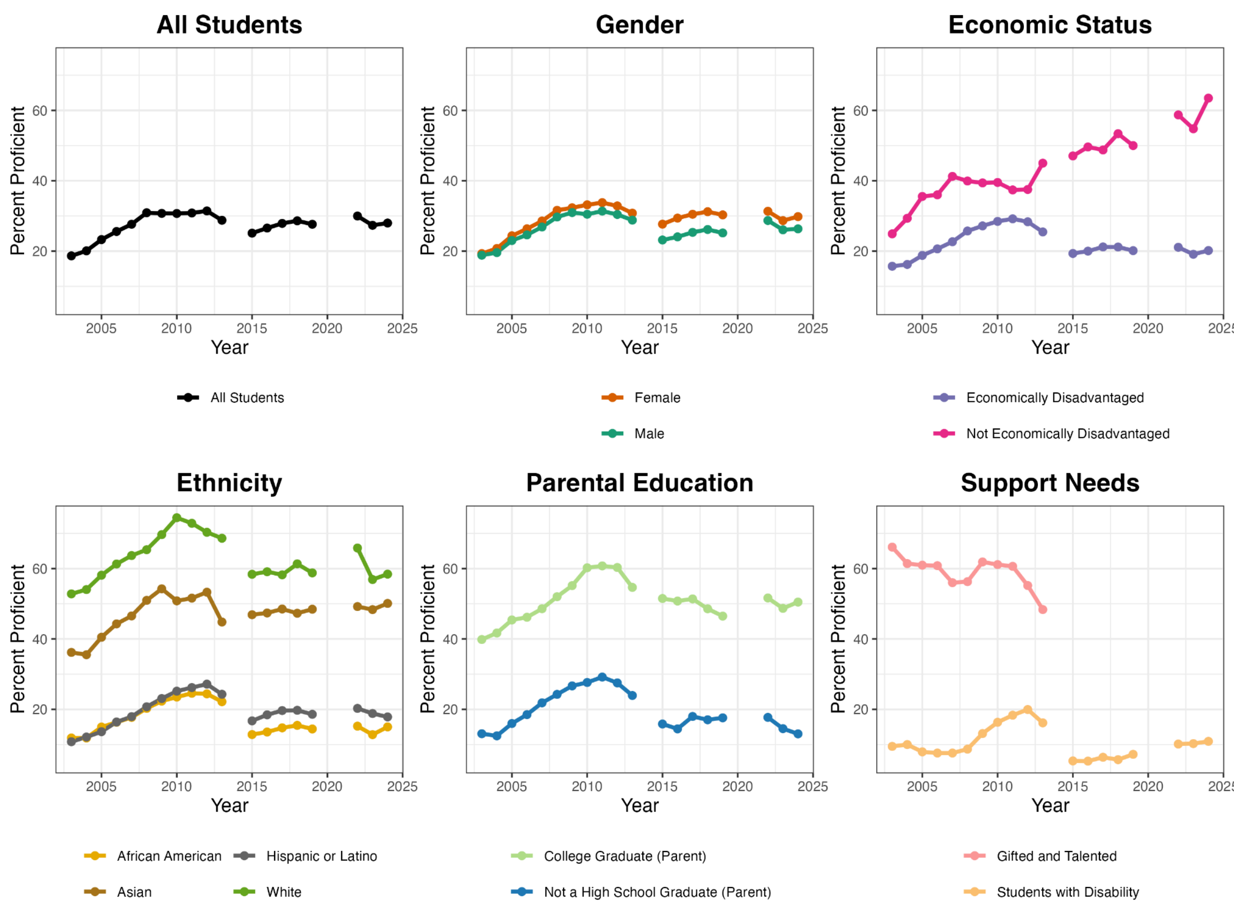


FIGURE 3. ANNUAL AVERAGE TEST SCORES BY SUBGROUP

Notes: Outcome is the percentage of OUSD students meeting the standard of proficiency. Data is missing for 2014, 2020, and 2021.

Teachers

The student-teacher ratio in OUSD has tended to range between 18 and 22. The number of full-time equivalent teachers employed by OUSD has fallen somewhat steadily since 2003 from over 2500 to fewer than 2000. The median student-teacher ratio in my sample is 19.9, and the median school has 16.8 full-time equivalent teachers.

Creating Datasets

I used the CDE dataset with school location (longitude and latitude), grades served, and years open to generate two main types of treatments. The first is a discrete treatment, which measures the total number of charter schools within a certain radius of each public school in each year. The second is a binary treatment, which measures whether there is at least one charter school within a certain radius of each public school in each year. I chose one mile as the primary radius after analyzing maps of where students who attend each OUSD school live, and examining a map of OUSD public and charter school locations (see Appendix Figure 1 and Oakland Unified School District, n.d.)[1]. Past studies have used a similar strategy to quantify the competitive pressure that traditional public schools face from charter schools (Bettinger 2005; Bifulco and Ladd 2006; Sass 2006). Outcomes are robust to small changes in the radius, and test score results are more significant with a 1.25-mile radius (see Appendix Figure 4).

The panel is not balanced because not all of the schools in the data set are open for all of the years in the study. The treatment data goes back to 1995, but the public schools that opened and closed between 2003 and 2024 do not have entries for years in which the school was not open.

I match public schools with competing charter schools when there is a grade overlap. For example, if a charter high school opens right next to a public elementary school, it would not be expected to exert significant competitive pressure. Schools that include more than one of an elementary, middle, or high school were split into separate groups and could receive different treatments. For example, a traditional public school that serves grades 6-12 would be split into a

[1] The maps that show where students from each OUSD public school live exists as "LG:MAP 1" at this link: https://dashboards.ousd.org/views/LiveGo2015-16ForwardPUBLIC/LGMAP1

middle school that only competes with charters that serve grades 6, 7, or 8, and a high school that only competes with charters that serve grades 9, 10, 11, or 12.

The test score data from the CDE includes the number of students tested at the subgroup and grade level. Data was available at the grade and subgroup level for each school and year combination. To determine the percentage of students in each subgroup, I divide the number of students tested in each subgroup by the number of students in the "all students" subgroup. I do this separately for each grade in each school and then average the results for grades K-5, 6-8, and 9-12 to generate subgroup percentages for each school in each year. So, a K-12 school would have separate percentages for K-5, 6-8, and 9-12, just as it would have separate treatments for these three groups.

The subgroups examined are male students, female students, economically disadvantaged students, non-economically disadvantaged students, White (not Hispanic) students, African American students, Asian students, Hispanic or Latino students, students whose parents graduated from college (a combination of the "College Graduate" and "Graduate School/Post Graduate" subgroups), students whose parents did not graduate from high school ("Not a High School Graduate"), students in the Gifted and Talented program, and students with disabilities.

For test score data, I use the percentage of students above a set level of proficiency as the outcome.[2] This data is also available at the grade and subgroup level, so I generate an overall score for each school (split into K-5, 6-8, and 9-12 as with the composition data), as well as a score for each school within each subgroup. For example, for School A in year X, I can observe

[2] This outcome changed slightly between 2013 and 2015 when the California standardized test changed, and the name switched from "Percentage Standard Met and Above" to "Percentage At or Above Proficient." However, the interpretation remains similar, and the year- fixed effects should account for any systemic changes between 2013 and 2015. Average scores changed by less than four percentage points between 2013 and 2015, from roughly 28.8% to 25.1% (see Figure 3).

the percentage of economically disadvantaged K-5 students that scored above the standard for proficiency.

The teacher staffing data was provided at the school level, not the grade level, so I do not split up the data into K-5, 6-8, and 9-12 groups. I use a version of the treatment that was not divided into separate groups for grades K-5, 6-8, and 9-12, and run the regressions at the school level. The only processing required was the removal of some outliers in the student-teacher ratio data. There were some extremely high values (>10,000) likely caused by data entry errors, so I removed all values greater than 75 (<1% of entries).

Difference-in-Differences Model

To run the regressions of charter entry on traditional public school outcomes, I use the model from "Difference-in-Differences Estimators of Intertemporal Treatment Effects" by Clément de Chaisemartin & Xavier D'Haultfoeuille (Chaisemartin & D'Haultfoeuille 2024). Several key elements of this model are useful for the empirical environment I am testing. First, the model can account for non-binary treatment. Traditional difference-in-difference models use binary treatments, but this model allows for discrete treatments. While I ultimately use a binary variable as my primary treatment, this model allows me to compare the binary and discrete versions of the treatment variable. Second, the model allows for non-absorbing treatment. In a traditional difference-in-difference model, once a unit is treated, it remains treated for the remainder of the years. However, Chaisemartin & D'Haultfoeuille's model allows units to move in and out of treatment. I focus primarily on schools moving into treatment and excluded schools moving out of treatment in the sample, but it is useful to be able to compare the two effects. Third, the model can account for dynamic effects. Dynamic effects means that a treatment in time *t* can impact the outcome in time *t+1*, *t+2*, etc. This is important because when a charter school opens in the radius of a public school, we expect the impacts to manifest over the course of more than one year. It might take a few years for a public school to respond to this new pressure, as responses such as staffing or budget allocation changes often cannot be implemented immediately. Furthermore, charter schools often open one grade per year (e.g., a middle school might open just a 6th grade in the first year) and therefore it may take several years before the full competitive effect is realized. In addition, this model can accommodate an unbalanced panel, group treatment can change at different dates, and groups can experience more than one change of their treatment.

Model Specification

I use the following model specification:

$$Y_{st} = \alpha_s + \gamma_t + \sum_{k \neq 0} \beta_k \cdot 1(EventTime_{st} = k) + \epsilon_{st}$$

Y_{st} is the outcome variable for traditional public school *s* in year *t*. $1(EventTime_{st} = k)$ is an indicator for being *k* years relative to the treatment year (defined as the first year a charter school is present within 1 mile of school *s*). β_k captures the dynamic effect of treatment *k* years before or after charter entry, relative to the baseline year. α_s are school fixed effects controlling for time-invariant characteristics of each school. γ_t are year fixed effects controlling for common shocks across all schools in a given year. ϵ_{st} is the error term. The treatment is a binary indicator equal to 1 if a charter school is operating within 1 mile of school *s* in year *t*, and 0 otherwise. Standard errors are clustered at the school level.

Identifying Assumptions

The model proposed by Chaisemartin & D'Haultfoeuille in “Difference-in-Differences Estimators of Intertemporal Treatment Effects” relies on assumptions about the experimental environment. Below I review the two most important assumptions and explain why I believe that these assumptions are plausible in this setting.

First, the model relies on a no-anticipation assumption. To be able to argue that the impact of the treatment truly occurred in the period in which the treatment changes, we need to be able to argue that the treated unit could not have anticipated the treatment. In this case, if a traditional public school were to know that a charter school was going to open nearby multiple years in advance of its opening, they would be able to respond to this threat of competition before the treatment began. This would mean that the change at t = 0 would not represent the true impact of the treatment. However, I argue that the no-anticipation condition is plausible for two reasons. First, California Education Code Section 47652(c) requires that a charter school in its first year of operation commence instruction between July 1 and September 30 of that year, so most charters will open within a year of approval (California Legislature n.d.). Second, we don’t see a consistent pattern in the data that suggests that traditional public schools are acting in response to the competitive pressure that charters will cause before the charters open. If this were the case, we would expect the pattern that we see after t = 0 to begin in t = -1 or t = -2, but we do not see this pattern.

The other key assumption that Chaisemartin & D'Haultfoeuille rely on is the parallel-trends assumption. As noted in the literature review, a primary challenge in estimating the impact of charter schools on traditional public schools is the endogeneity of charter school location. The parallel-trends assumption asserts that treated and untreated units would have had the same outcome trajectory in the absence of treatment. In this case, the assumption is that outcomes for schools with charters within a one-mile radius and outcomes for schools without charters in a one-mile radius would have continued to move in parallel if not for charter entry. It is impossible to observe this counterfactual scenario, but we can test the plausibility of this assumption by examining the years prior to treatment. If we find that the units that will become treated and the units that will become controls have similar trajectories before they are treated, we have support for the claim that we expect parallel trends to hold. Outcomes are normalized to 0 at t = 0 which means that schools do not have to have the same outcomes, but they must have the same trends. For example, if charters are more likely to open near higher performing schools, this does not necessarily violate the parallel-trends assumption. However, if charters are more likely to open near schools in which scores are increasing year by year, this would violate the parallel-trends assumption.

Overall, most graphs seem to support the parallel-trends assumption. On a graph, this looks like estimated coefficients close to 0 before t = 0 and confidence intervals that include 0 in years t = -1, t = -2, and t = -3 (also called the placebo years, because they occur before treatment). The parallel-trends assumption can also be supported if there doesn't seem to be a clear trend before year 0, if values are bouncing from positive to negative.

V. RESULTS AND ANALYSIS

Event Study Plots

The did_multiplegt_dyn function associated with Chaisemartin & D'Haultfoeuille (2024) creates an event study plot that shows the effect of the treatment relative to t = 0, which is the last period before treatment. By default, the graph shows t = 0 and t = 1. Because I expect the effect of charter entry to occur over the course of more than 1 year, I use a wider range of years for my event study plots. I also use the years before t = 0 (called placebo years) to test the parallel-trends assumption. I chose to use 4 placebo years because there was a large drop off in the number of units used to calculate the values after 4 years.

The model also generates an "average cumulative effect per treatment unit," which measures the effect of the treatment across all of the event years included. I often refer to this measure as the "average cumulative effect" when discussing a binary treatment, because the treatment unit is always one.

The number of event years was more arbitrary to determine than the number of placebo years because the number of treated units dropped much more gradually and there was no obvious place to cut the graph. I present cumulative estimates for years 6, 8, and 10 years in tables, though the results are robust to changes in the range of years included for almost every regression. I use the eight-year average cumulative effect as the primary figure. For the primary test score regression, I include a graph with the maximum possible range and a table that shows how changes in the range affect the average cumulative effect. See Appendix Figure 2 and Table 1A and 1B.

Results

Student Composition

Broadly, I find evidence that charter entry leads to a higher percentage of students in traditionally lower-scoring subgroups in traditional public schools. Specifically, the introduction of a charter school coincides with an average increase in traditional public schools in the percentage of economically disadvantaged students, African American students, Hispanic or Latino students, students whose parents did not graduate from high school, and students with a disability. In contrast, the percentage of White students, Asian students, and gifted and talented students decreased in public schools after the introduction of a charter school. These patterns are consistent with the idea that some degree of cream-skimming or pushout is occurring. Charter schools may be enrolling a higher percentage of higher-scoring students than traditional public schools, or they may be pushing out lower-scoring students at a higher rate than traditional public schools. However, this data is not causal evidence for either of these claims, as we only observe the changes in TPS composition, not charter composition. This data does tell us that charter entry concentrates lower-scoring students in traditional public schools.

The largest effect is the change in the percentage of economically disadvantaged students, which had an average cumulative effect of over 10 percentage points over eight years, significant at the 99% level. The percentage of African American students saw an average cumulative effect of nearly 3, and Hispanic or Latino students saw an average cumulative effect of roughly 3.5 (Table 1). For White students, the average cumulative effect was roughly -1.5 percentage points, and for Asian students it was about -2. Students whose parents did not graduate from high school experienced an average cumulative effect of about 5, while students in the Gifted and Talented program had an average cumulative effect of nearly -5. There was not a significant effect of charter school presence on the percentage of male or female students, on the percentage of students whose parents graduated from college or on the percentage of students with a disability over eight years (Table 1).

TABLE 1 – EFFECT OF CHARTER OPENING ON STUDENT COMPOSITION

	Average cumulative effect per treatment unit		
Variable	6 years	8 years	10 years
Percent Female	-0.234	-0.181	-0.438
	(0.884)	(1.004)	(0.945)
Percent Economically Disadvantaged	9.162***	10.336***	10.773***
	(2.240)	(2.606)	(2.601)
Percent White	-1.353*	-1.561*	-2.050**
	(0.755)	(0.919)	(1.030)
Percent African American	2.601*	2.953**	3.545**
	(1.377)	(1.506)	(1.548)
Percent Asian	-1.813**	-2.056**	-2.295***
	(0.788)	(0.860)	(0.856)
Percent Hispanic	2.794**	3.494***	3.807***
	(1.101)	(1.192)	(1.114)
Percent College Graduate (Parent)	0.647	0.623	-0.551
	(2.273)	(2.419)	(2.439)
Percent Not High School Graduate (Parent)	4.711***	5.049***	4.874***
	(1.556)	(1.678)	(1.719)
Percent Gifted and Talented	-3.641	-4.886**	-4.793**
	(2.284)	(2.276)	(2.401)
Percent Students with Disability	1.229	1.377	1.645*
	(0.777)	(0.829)	(0.893)
N	667	742	764
Switchers	131	160	182

Notes: Average cumulative effect per treatment unit is calculated over 6, 8, and 10 years of treatment. Standard errors are in parentheses. N refers to the total number of group-time observations used to calculate the estimate and Switchers refers to the number of treated group-time observations that contribute to the estimate (not the number of unique treated schools). N and Switchers are reported as a row because the values were the same for each dependent variable. N and switchers provide a measure of the sample size used to calculate each estimate.
*** p<0.01, ** p<0.05, * p<0.1.

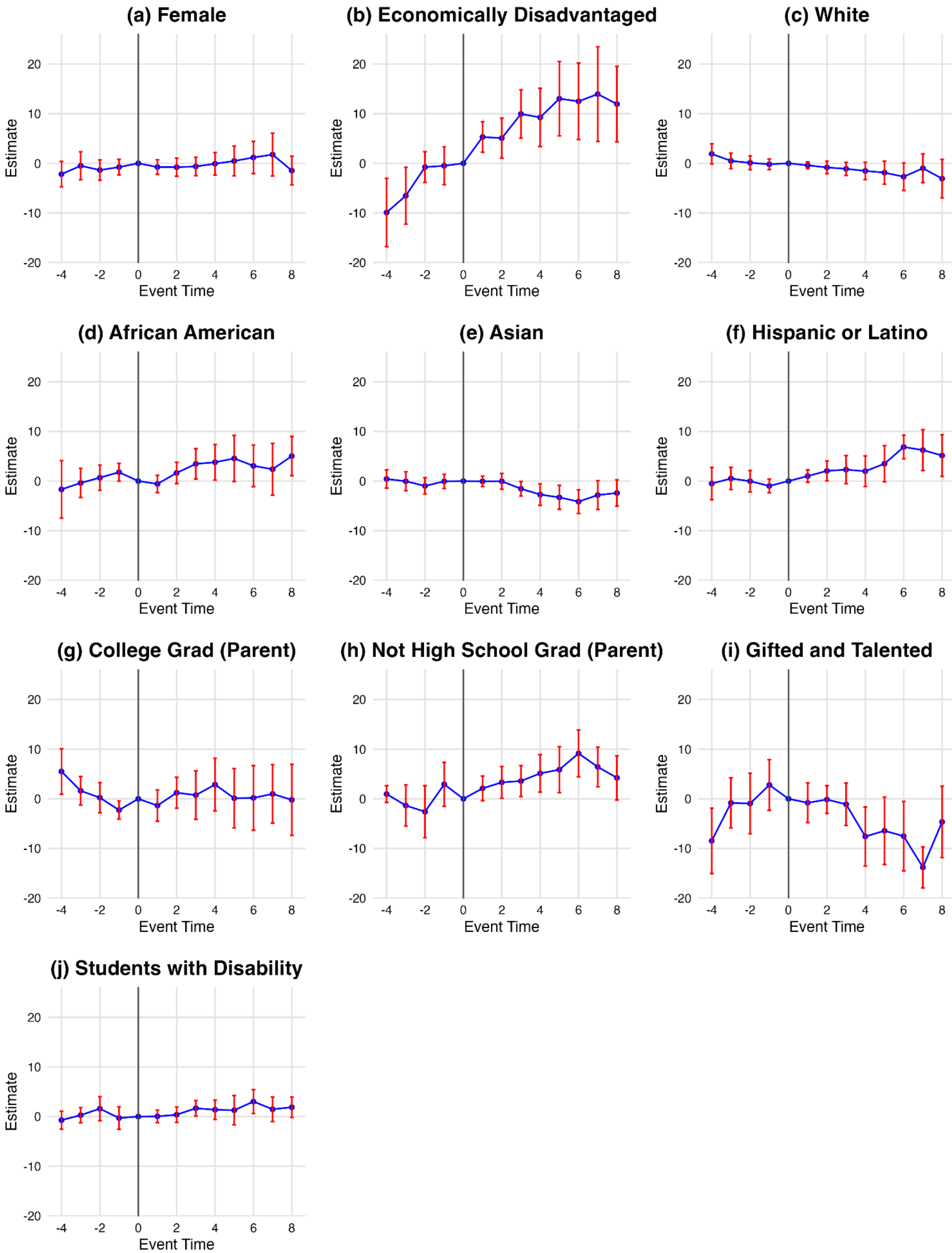


FIGURE 4. EVENT STUDY ESTIMATES - STUDENT COMPOSITION

Notes: Year 0 represents the last year before treatment. Estimates are calculated in each year relative to Year 0. Red bars show a 95% confidence interval for each estimate.

The parallel-trends assumption does not hold for some of these regressions. Most notably, there appear to be pre-treatment trends in the economically disadvantaged graph that suggests that we cannot make a causal claim about the effect of charter opening on the percentage of economically disadvantaged students in traditional public schools. There are also more subtle pre-treatment trends in the White graph that require us to interpret the post-treatment results with caution. The gifted and talented graph shows pre-treatment trends, though these trends move in the opposite direction of the post-treatment trends, which makes the interpretation less clear. The college grad and female graphs also show some sign of pre-treatment trends.

Test Scores

The introduction of a nearby charter school is associated with a decrease in test scores for students in traditional public schools. Over eight years, we see an average cumulative treatment effect of -3.413 percentage points. This effect is statistically significant at the 99% significance level. There is almost no effect in the first year after treatment, but by the third year the estimate is already less than -4. The effect takes a few years to set in on average but is relatively durable for the next five years. The parallel-trends assumption is plausible, as the placebo estimates are close to zero and do not follow a clear pre-treatment trend. The average cumulative treatment effect is robust to changes in the number of effects estimated and is statistically significant at the 95% level for effect ranges between 3 and 11 (see Appendix Tables 1A and 1B).

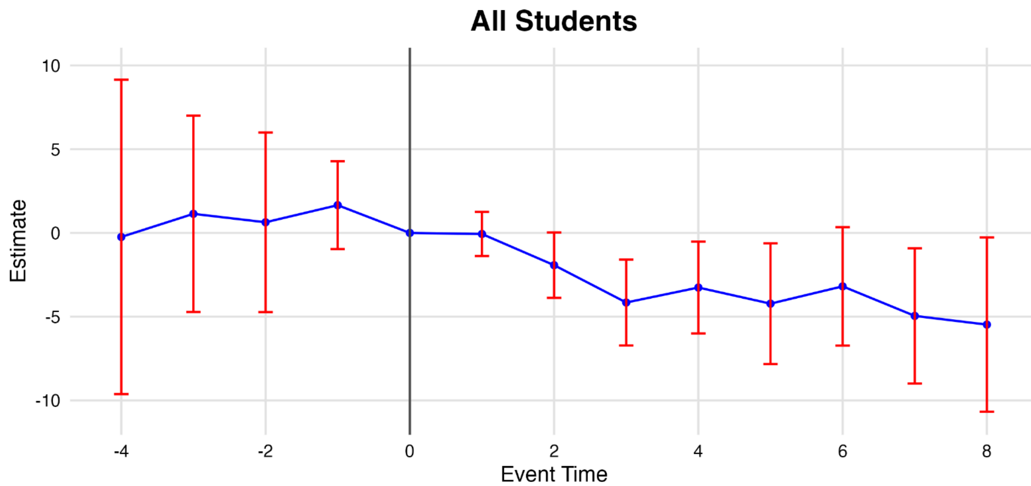


FIGURE 5. EVENT STUDY ESTIMATES - TEST SCORES (ALL STUDENTS)

Notes: Year 0 represents the last year before treatment. Estimates are calculated in each year relative to Year 0. Red bars show a 95% confidence interval for each estimate.

Importantly, because there are signs of changes in student composition associated with charter school introduction, we cannot rule out that this decrease was simply caused by higher-performing students leaving public schools. However, we can look at how scores change within each subgroup to get a better understanding of whether the change in scores is simply driven by changes in student composition, or by other factors as well. In this case, I do find similar decreases in student test scores following the introduction of a charter school within many of the demographic groups explored in the student composition regressions. For some subgroups, the effect is even stronger; for example, male students, non-economically disadvantaged students, African American students, and Hispanic or Latino students all saw a greater impact than the overall student body (See Table 2). Female students and economically disadvantaged students saw slightly smaller effects, but both saw an average cumulative treatment effect of about -3 percentage points. Furthermore, the parallel-trends assumption is satisfied for most of the subgroup regressions. African American students are a possible exception, but the graph shows rising test scores before the charter entry, followed by declining scores afterward, so this trend wouldn't seem to bias the results after the charter entry. It is important to note that consistent results between the regression on all students and the regressions on student subgroups does not rule out that composition changes are driving the decrease in test scores. It might be the case, for example, that unobserved characteristics such as high student or parent motivation are driving students to leave charters at higher rates, and therefore biasing these results.

I ran the regressions for all of the subgroups explored in the composition section, but subgroups with fewer students had more missing results and therefore not enough data to make conclusions. The regression of White student test scores, for example, only has one or two observations per estimate. I include the graphs of subgroups with fewer than 100 total switchers contributing to the 8-year average cumulative effect in the appendix for reference (See Appendix Figure 3).

TABLE 2 – EFFECT OF CHARTER OPENING ON TEST SCORES

	Average cumulative effect per treatment unit		
Variable	6 years	8 years	10 years
Percent Proficient (All Students)	-2.803***	-3.413***	-3.587**
	(1.025)	(1.178)	(1.458)
N, Switchers	660, 131	735, 160	757, 182
Percent Proficient (Male)	-3.464***	-4.335***	-4.664***
	(1.111)	(1.324)	(1.690)
N, Switchers	651, 131	724, 160	746, 182
Percent Proficient (Female)	-2.176	-3.109*	-3.026
	(0.755)	(1.673)	(1.860)
N, Switchers	651, 131	726, 160	748, 182
Percent Proficient (Economically Disadvantaged)	-2.640**	-3.164**	-3.445*
	(1.308)	(1.510)	(2.012)
N, Switchers	564, 123	634, 150	656, 171
Percent Proficient (Not Economically Disadvantaged)	-4.352***	-4.359***	-4.685***
	(1.447)	(1.592)	(1.673)
N, Switchers	523, 92	574, 107	602, 118
Percent Proficient (African American)	-3.561***	-4.622***	-5.217***
	(1.194)	(1.235)	(1.302)
N, Switchers	594, 125	638, 151	674, 170
Percent Proficient (Hispanic or Latino)	-3.562*	-4.150*	-3.704
	(2.000)	(2.368)	(2.729)
N, Switchers	355, 92	425, 114	441, 129

Notes: Subgroups with at least 100 total switchers through 8 years were included. Results for the excluded subgroups can be found in the appendix. Average cumulative effect per treatment unit is calculated over 6, 8, and 10 years of treatment. Standard errors are in parentheses. N refers to the total number of group-time observations used to calculate the estimate. Switchers refers to the number of treated group-time observations that contribute to the estimate (not the number of unique treated schools). N and switchers provide a measure of the sample size used to calculate each estimate. *** p<0.01, ** p<0.05, * p<0.1.

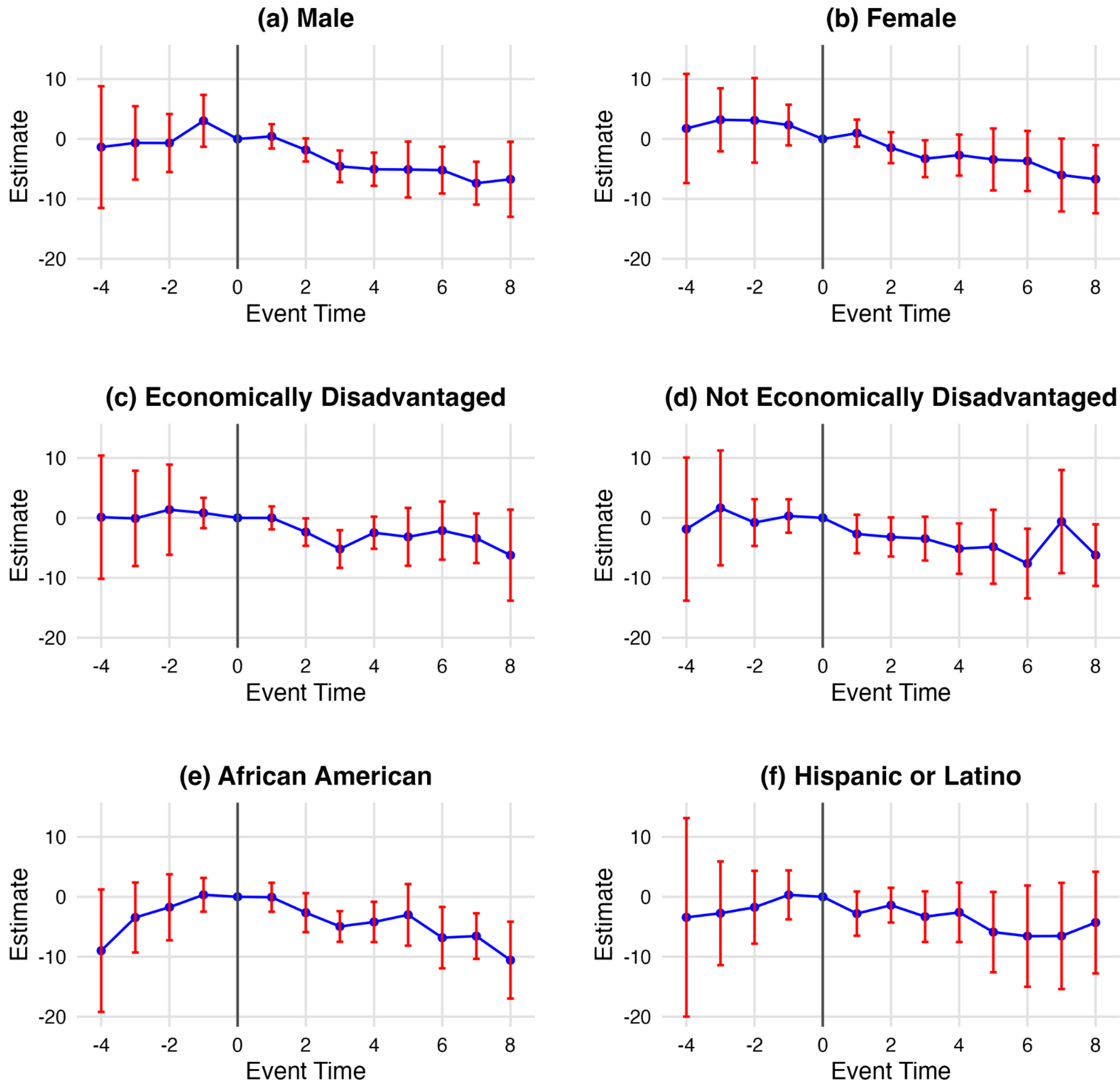


FIGURE 6. EVENT STUDY ESTIMATES - TEST SCORES (WITHIN SUBGROUPS)

Notes: Subgroups with at least 100 total switchers through 8 years are included. Results for the excluded subgroups can be found in the appendix. Year 0 represents the last year before treatment. Estimates are calculated in each year relative to Year 0. Red bars show a 95% confidence interval for each estimate.

Teachers

Charter entry is associated with an increase in full-time equivalent (FTE) teachers and a decrease in student-teacher ratio (STR). FTE teachers see an average cumulative effect of about

2 teachers over eight years, significant at the 90% level. STR has an average cumulative effect of about -2.7 over eight years, significant at the 95% level. We can visualize this as schools hiring two new teachers and class sizes falling by 2.7 on average. These two outcomes are linked, as STR is the number of students in a school divided by the number of teachers.

TABLE 3 – EFFECT OF CHARTER OPENING ON TEACHER STAFFING

	Average cumulative effect per treatment unit		
Variable	6 years	8 years	10 years
Full-Time Equivalent Teachers (FTE)	1.537*	1.938*	1.825*
	(0.813)	(0.995)	(1.078)
N, Switchers	701, 134	761, 170	792, 201
Student-Teacher Ratio (STR)	-2.332**	-2.698**	-3.063**
	(0.987)	(1.179)	(1.228)
N, Switchers	699, 134	759, 170	790, 201

Notes: Average cumulative effect per treatment unit is calculated over 6, 8, and 10 years of treatment. Standard errors are in parentheses. N refers to the total number of group-time observations used to calculate the estimate and switchers refers to the number of treated group-time observations that contribute to the estimate (not the number of unique treated schools). N and switchers provide a measure of the sample size used to calculate each estimate.
*** p<0.01, ** p<0.05, * p<0.1.

These results seem counterintuitive in some ways. We might expect that the entry of charter schools, which tends to draw enrollment (and therefore funding) away from public schools, would lead to a decrease in the number of teachers a public school could employ. Furthermore, a charter school would be an additional competitor in the hiring market, which could make it harder for public schools to hire qualified teachers. The decreased STR is not surprising, as we expect enrollment to fall following the introduction of a charter school, and decreasing student enrollment and increased teacher hiring necessarily reduce STR. However, it is interesting that test scores continued to fall in spite of decreasing STR, as lower student-teacher ratios tend to correlate strongly with better student test scores (Word et al. 1990). By year 3, STR in treated schools falls about 3 below that of untreated schools, and this effect remains relatively persistent for the next 5+ years. The parallel trends assumption seems visually plausible for these two outcomes.

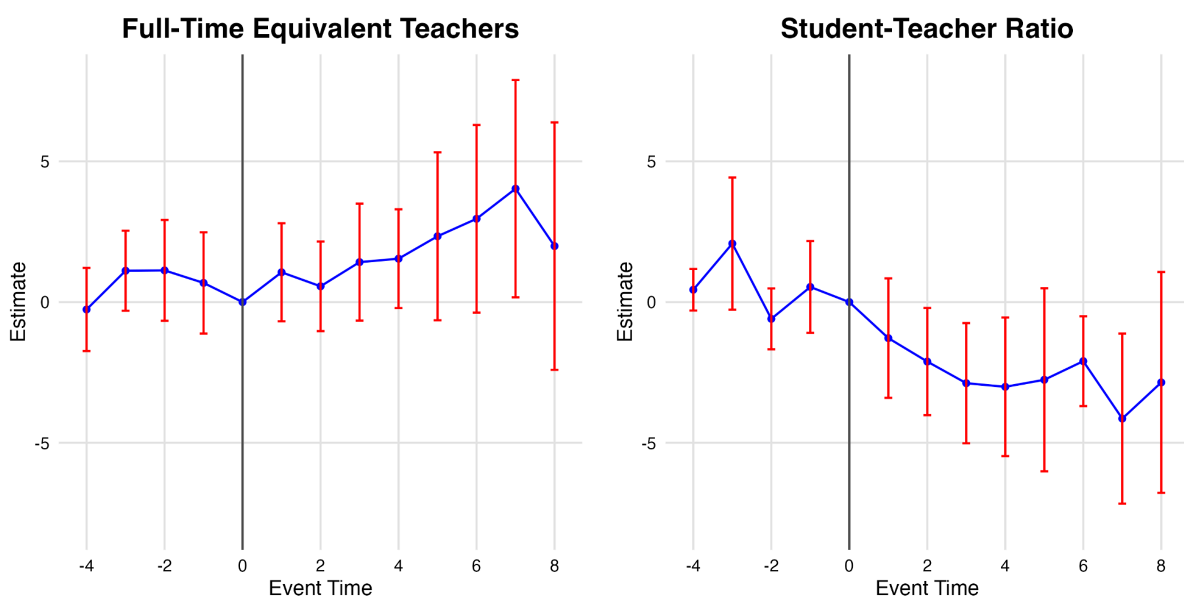


FIGURE 7. EVENT STUDY ESTIMATES - TEACHER STAFFING

Notes: Year 0 represents the last year before treatment. Estimates are calculated in each year relative to Year 0. Red bars show a 95% confidence interval for each estimate.

Potential Mechanisms

The mechanism by which charter entry may have led to decreased test scores in traditional public schools is not obvious. It seems likely that changes in student composition contributed to these decreases, as we see a clear increase in the percentage of students in traditionally lower-scoring groups. It also seems plausible that peer effects may have impacted test scores, as clustering lower-achieving students together tends to further reduce their outcomes (Burke and Sass 2013). It might be the case (as Board Policy 5115 suggests) that decreases in enrollment caused by charter entry led to budgetary stress, which may have forced traditional public schools to fire qualified teachers or cut back important programs as fixed costs such as rent made up a greater percentage of revenue (Oakland Unified School District 2021). However, the increase in full-time equivalent teachers following the entry of a charter school does not necessarily support this claim. A more detailed and nuanced examination of traditional public school responses to charter school competition would be required to diagnose the mechanisms by which charter entry may lead to decreased traditional public school test scores. Furthermore, it is likely the case that the mechanisms vary widely from district to district and school to school.

Robustness Checks

Changes in Radius

The test score results are somewhat robust to small changes in the radius used to calculate the treatment variables, though at significantly smaller or larger radii the effects become statistically insignificant. A 0.75-mile radius yields an average cumulative effect of -0.926 over eight years and is not statistically significant. A 1.25-mile radius yields a stronger effect than a 1-mile radius, with an average cumulative effect of -5.337 over eight years, significant at the 99% level. 0.5- and 1.5-mile radii both have negative average cumulative effect per treatment units as well, but the confidence intervals are very large, and the effects are not significant. The

0.5-mile, 0.75-mile and 1.25-mile plots follow a similar trend as the 1-mile plot, while the 1.5-mile plot does not follow a clear trend. See Appendix Figure 4 for event study plots.

Discrete vs. Binary Treatment

While I ultimately chose to use the binary variable as the primary treatment, the discrete variable yields very similar results. For the impact on test scores of all students, the average cumulative effect per treatment unit is -2.427 after eight years, and is statistically significant at the 99% level. This pattern holds for all of the regressions that I tested; the average cumulative effect per treatment unit tends to be slightly lower because there are more levels of treatment (values range from 0 to 6), but the direction and pattern of effects are very similar. There were a few advantages to using the binary treatment. First, as seen in Appendix Figure 1, Oakland has a diverse school district that includes both urban and suburban areas with varying levels of density. More urban areas tend to have more students and schools in a given radius. It might be the case that observing a higher number of charter schools near a TPS is more indicative that the TPS is in an urban area than that it is facing very high levels of competition. Similarly, it may be the case that we are more likely to see a higher number of charter schools in a one-mile radius when charters are smaller or serve niche goals and therefore do not directly compete. This would introduce confounding variable bias into the regression. Overall, I think an interesting extension of this research would be to use the number of charter seats available in a given radius as a treatment, because this would capture a more accurately weighted measure of charter competition.

The second advantage of the binary treatment is that the interpretation is clearer. With a binary variable, the effect of treatment is simply the effect of charter entry into the radius of a traditional public school. With the discrete variable, the treatment can change from 2 to 4 in a given year, which becomes more difficult to interpret. Furthermore, it may be the case that the difference between 0 and 1 charter schools is not comparable to the difference between 5 and 6 charters. See Appendix Figures 5, 6, and 7 for event study plots.

Entry vs. Exit

In this study, I limit the regression to the impact of charter entry, even though the model is capable of handling both entry and exit. In other words, the model can combine changes from 0 to 1 as a positive treatment and changes from 1 to 0 as a negative treatment. However, I focus on charter entry for two reasons. First, there were far fewer instances of charter exit in my sample and therefore estimates of charter exit were likely to be noisy. Second, the impact of charter entry does not necessarily seem analogous to the reverse impact of charter exit. In other words, we would not expect the closing of a charter school to have the exact opposite impact as the opening of a charter. Focusing on charter entry allows for a clearer interpretation of the treatment. The impact of including charter exit in the regression is not large; the combined regression has slightly larger confidence intervals, but the shape and magnitude remain similar. See Appendix Figure 8 for event study plots.

Grade Level Disaggregation

Effects were not consistent across elementary, middle, and high schools. Charter entry had a slight positive average cumulative effect on TPS elementary school performance. The average cumulative effect after eight years was 1.604. Middle schools and high schools both saw negative effects after eight years of -2.603 and -4.177, respectively. However, none of these effects were statistically significant, in large part because of the small sample sizes. This would be an interesting area to explore in more detail with a larger sample. See Appendix Figure 9 for event study plots.

VI. CONCLUSION

Broadly, I find the following results: (1) charter entry is associated with an increased percentage of lower-scoring student groups in traditional public schools, (2) charter entry is associated with a decrease in test scores in traditional public schools, and (3) charter entry is associated with an increase in full-time equivalent teachers and decrease in student-teacher ratio in traditional public schools. When considering these results, it is important to recall school districts are highly heterogeneous, and the effects of introducing charter schools vary greatly based on district-specific factors. In other words, these results should not be assumed to generalize to other districts, even in California or the Bay Area. That said, these results do not support the claim that charter school entry creates positive externalities for traditional public-school students. Charter entry is associated with increased concentration in traditional public schools of students of color, low-income students, students whose parents did not graduate from high school, and students with a disability. Charter entry is also associated with lower proficiency rates in traditional public schools even within subgroups, indicating that the decrease in performance may be the result of more than changes in composition. However, the increase in full-time equivalent teachers is both surprising and promising, and suggests that traditional public schools may be attempting to respond to competitive pressure, even when facing the greatest budget constraints.

Ultimately, these results only tell part of the story of the impact of charter schools on the public education landscape. Even if charter schools directly generate negative externalities for traditional public schools, the overall impact of charter schools on public school students also depends on the impact of charter schools on charter school students. The impacts of charter entry on traditional public school student composition, test scores, and staffing are certainly useful pieces of information for policy makers to consider when deciding whether to approve or encourage new charters in Oakland, but are far from the only relevant factors.

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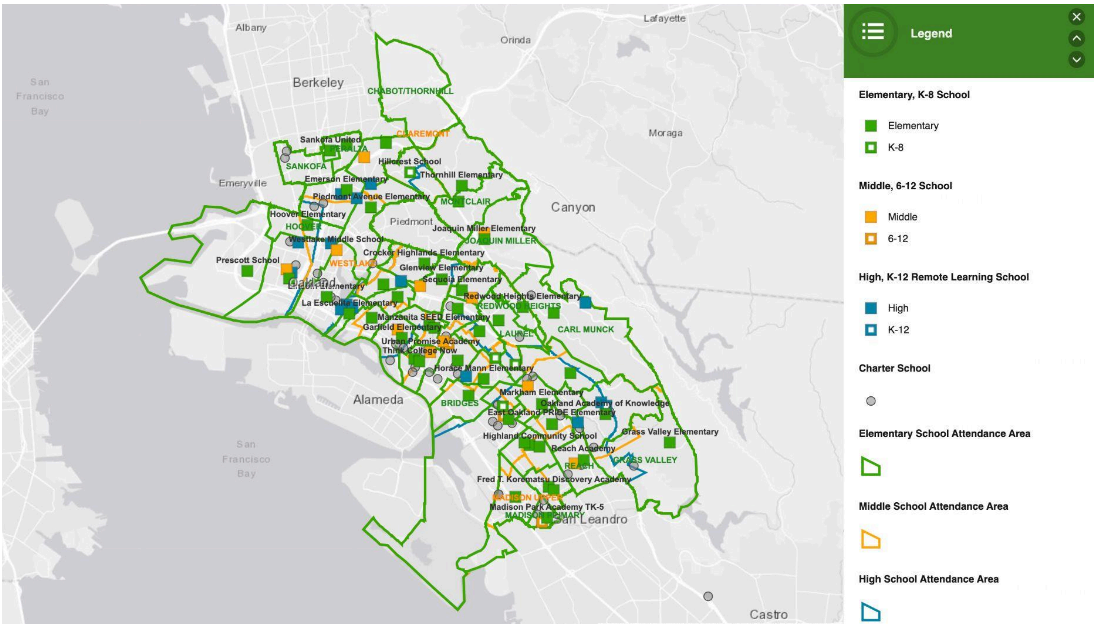
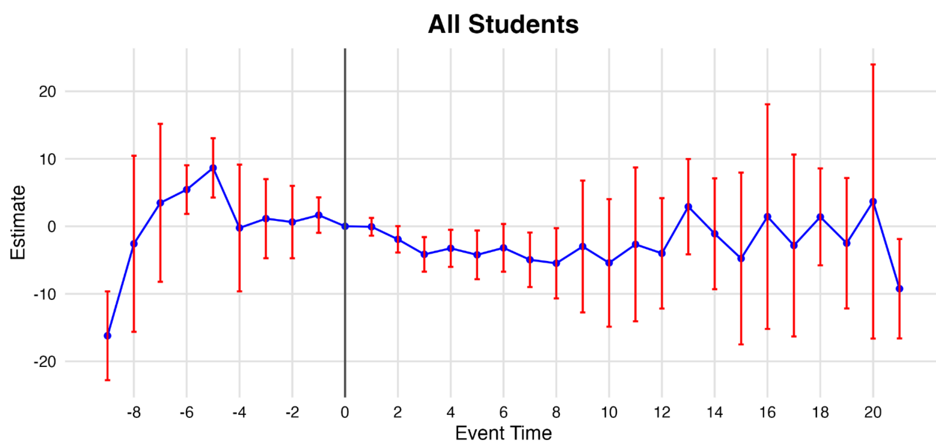
APPENDIX



APPENDIX FIGURE 1. MAP OF OAKLAND PUBLIC AND CHARTER SCHOOLS

Notes: Data from OUSD. Map shows 2024-2025 schools and boundaries.



APPENDIX FIGURE 2. EVENT STUDY ESTIMATES - TEST SCORES (EXTENDED)

Notes: This is the widest possible graph for the binary 1-mile specification. There are nine placebo estimates and 21 post-treatment estimates. Very small sample sizes contribute to the large error bars at either end of the graph. Year 0 represents the last year before treatment. Estimates are calculated in each year relative to Year 0. Red bars show a 95% confidence interval for each estimate.

Appendix Table 1A – Effect of Charter Opening on Test Scores (Range Robustness Check)

Variable: Percent Proficient (All Students)

Event estimates included	Average cumulative effect per treatment unit
1 year	-0.063
	(0.672)
N, Switchers	322, 26
2 years	-0.957
	(0.738)
N, Switchers	404, 50
3 years	-2.050**
	(0.843)
N, Switchers	514, 75
4 years	-2.351***
	(0.911)
N, Switchers	570, 97
5 years	-2.673***
	(0.981)
N, Switchers	621, 115
6 years	-2.803***
	(1.025)
N, Switchers	660, 131
7 years	-3.109***
	(1.105)
N, Switchers	697, 145

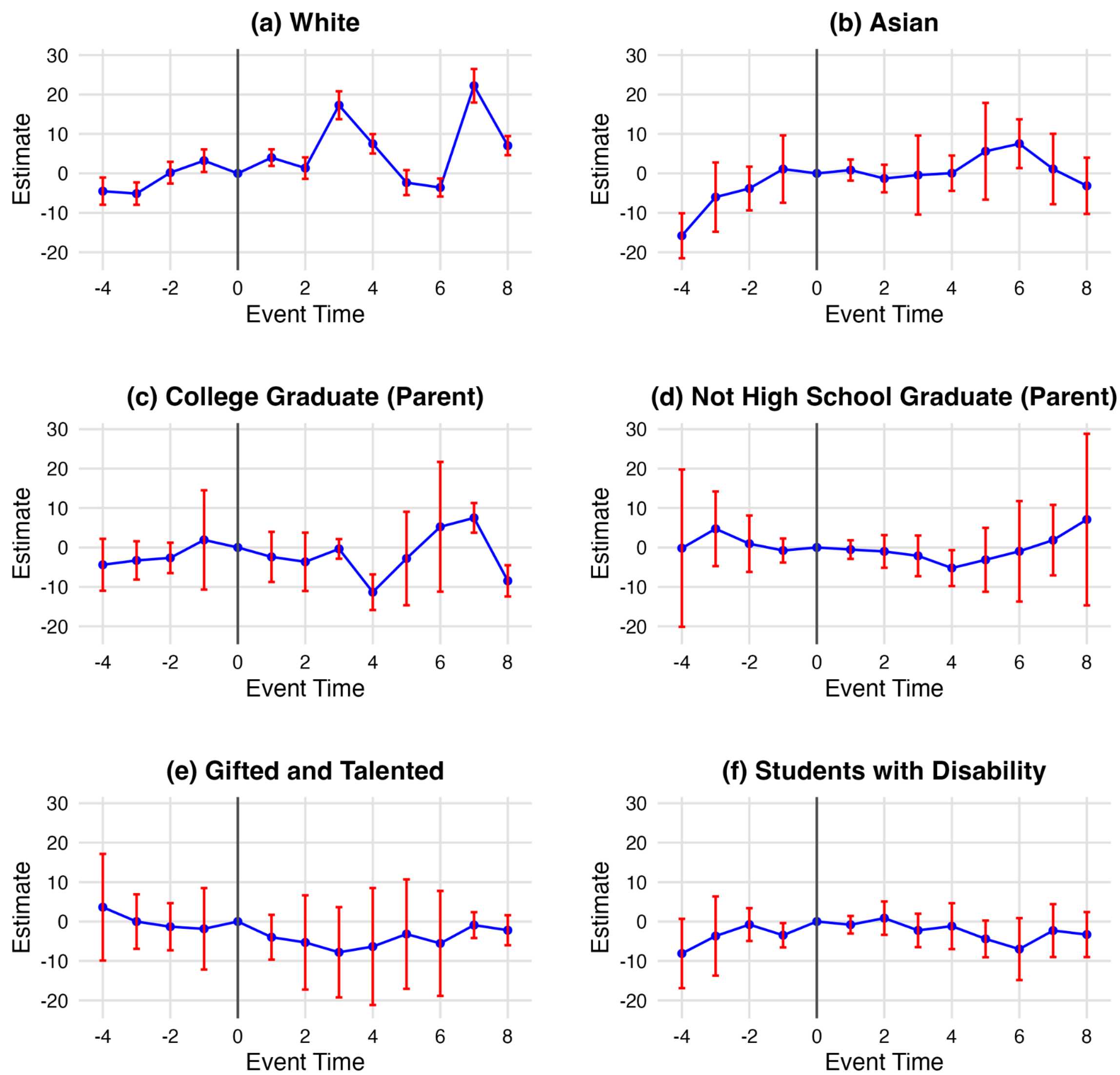
Notes: "Event estimates included" refers to the number of years included in the calculation of the average cumulative effect per treatment unit. Standard errors are in parentheses. N refers to the total number of group-time observations used to calculate the estimate. Switchers refers to the number of treated group-time observations that contribute to the estimate. N and switchers provide a measure of the sample size used to calculate each estimate. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table 1B – Effect of Charter Opening on Test Scores (Range Robustness Check)

Variable: Percent Proficient (All Students)

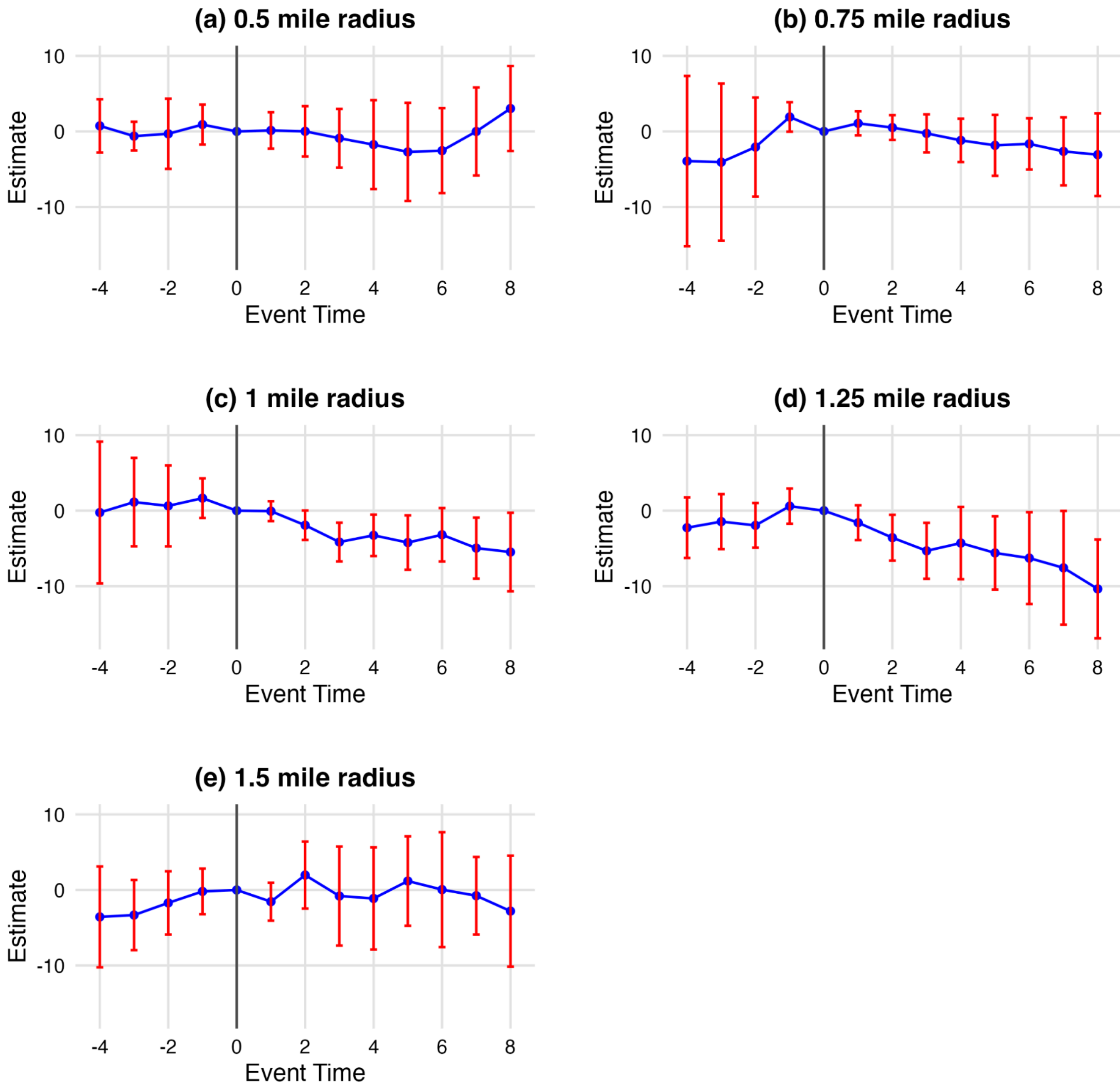
Event estimates included	Average cumulative effect per treatment unit
8 years	-3.413***
	(1.178)
N, Switchers	735, 160
9 years	-3.446***
	(1.328)
N, Switchers	747, 172
10 years	3.587**
	(1.458)
N, Switchers	757, 182
11 years	3.601**
	(1.745)
N, Switchers	770, 195
12 years	-3.710*
	(1.929)
N, Switchers	784, 209
13 years	3.470*
	(1.953)
N, Switchers	792, 217
14 years	3.409*
	(1.988)
N, Switchers	800, 225

Notes: "Event estimates included" refers to the number of years included in the calculation of the average cumulative effect per treatment unit. Standard errors are in parentheses. N refers to the total number of group-time observations used to calculate the estimate. Switchers refers to the number of treated group-time observations that contribute to the estimate. N and switchers provide a measure of the sample size used to calculate each estimate. *** p<0.01, ** p<0.05, * p<0.1.



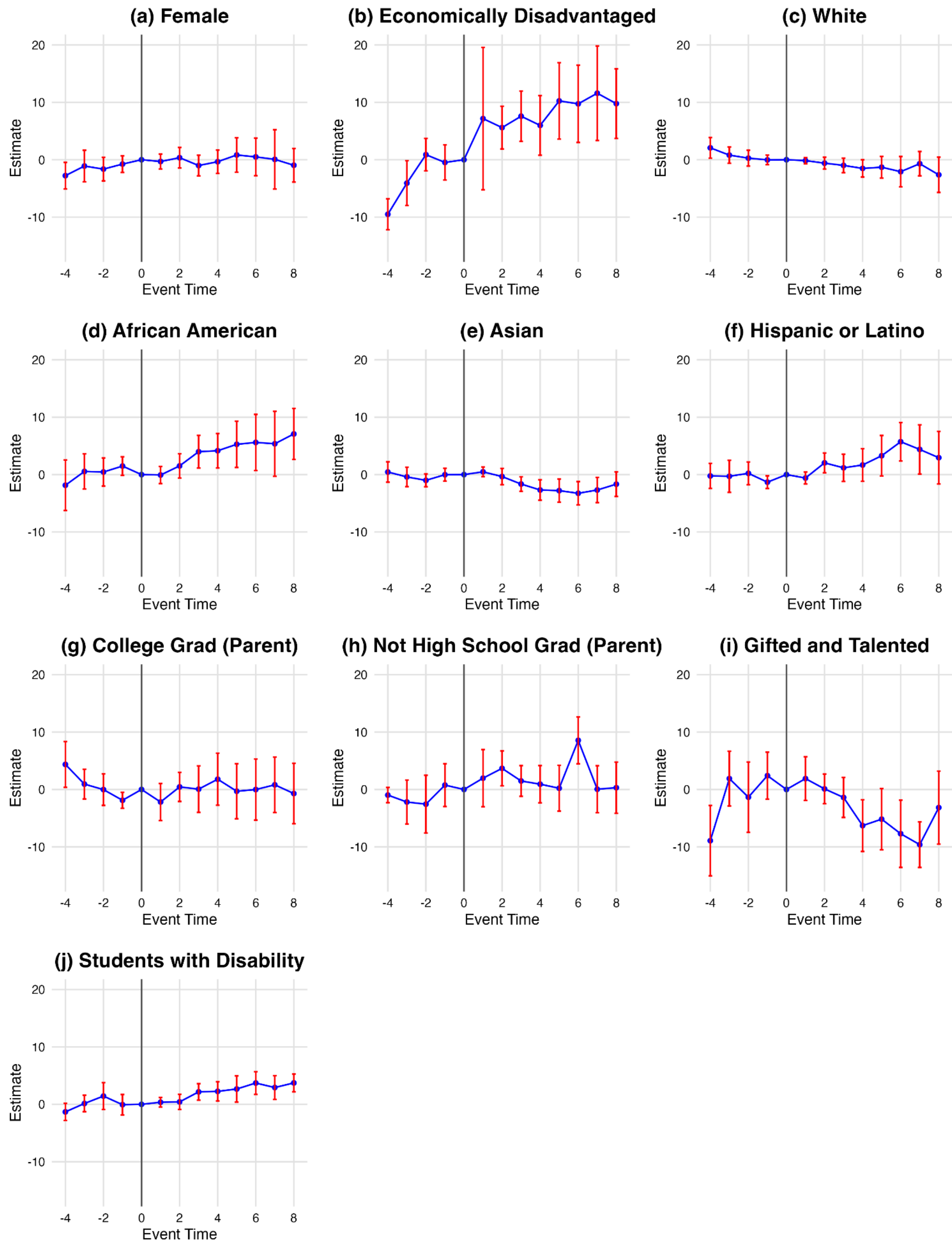
APPENDIX FIGURE 3. EVENT STUDY ESTIMATES - TEST SCORES (EXCLUDED SUBGROUPS)

Notes: Subgroups with fewer than 100 total switchers through 8 years are included in this figure. Year 0 represents the last year before treatment. Estimates are calculated in each year relative to Year 0. Red bars show a 95% confidence interval for each estimate.



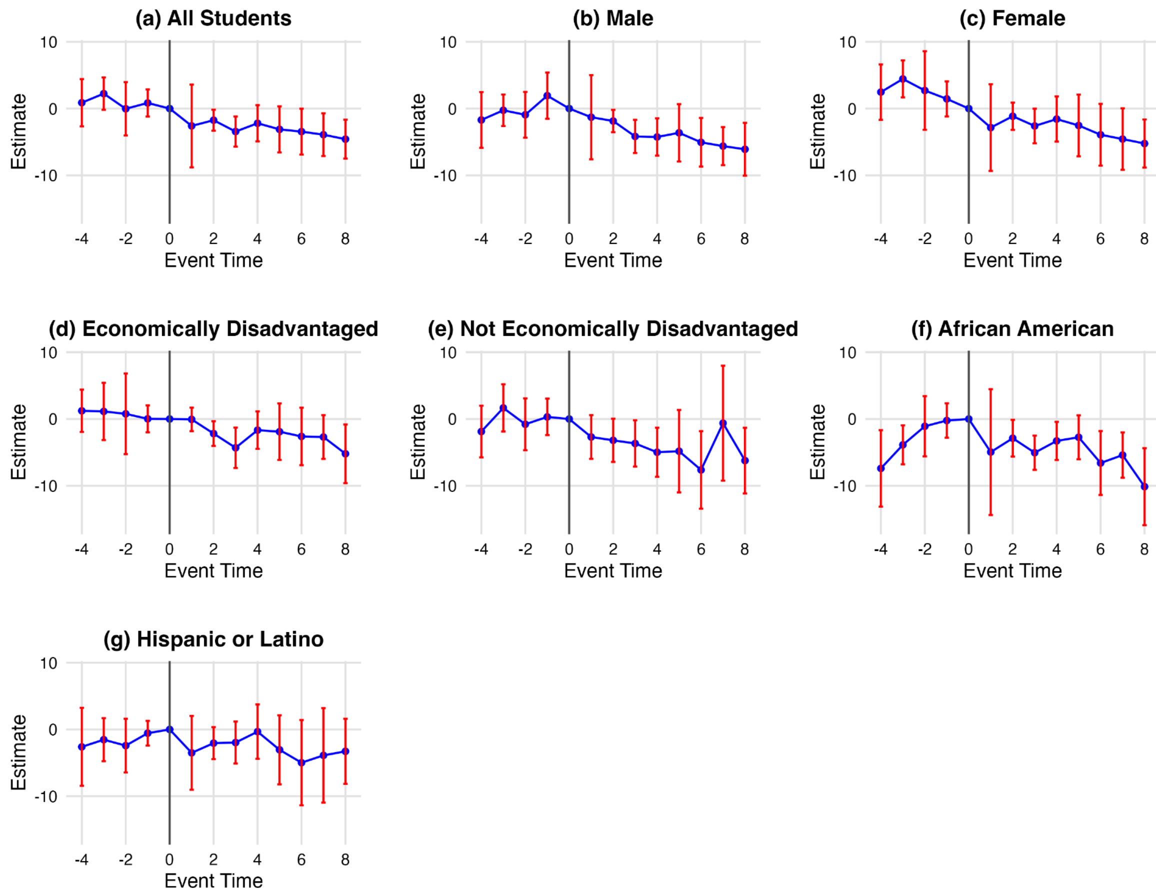
APPENDIX FIGURE 4. EVENT STUDY ESTIMATES - TEST SCORES (CHANGES IN RADIUS)

Notes: Subtitles represent changes in the treatment specification. Year 0 represents the last year before treatment. Estimates are calculated in each year relative to Year 0. Red bars show a 95% confidence interval for each estimate.



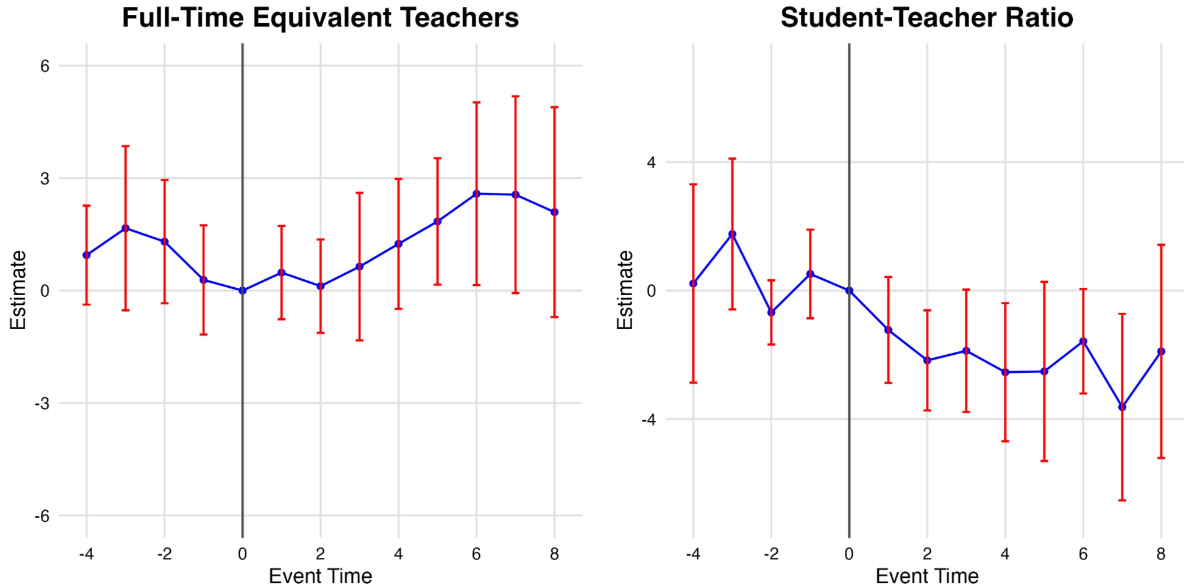
APPENDIX FIGURE 5. EVENT STUDY ESTIMATES - STUDENT COMPOSITION (DISCRETE TREATMENT)

Notes: This figure mirrors the graphs in Figure 4, but uses a discrete treatment instead of a binary treatment. Year 0 represents the last year before treatment. Estimates are calculated in each year relative to Year 0. Red bars show a 95% confidence interval for each estimate.



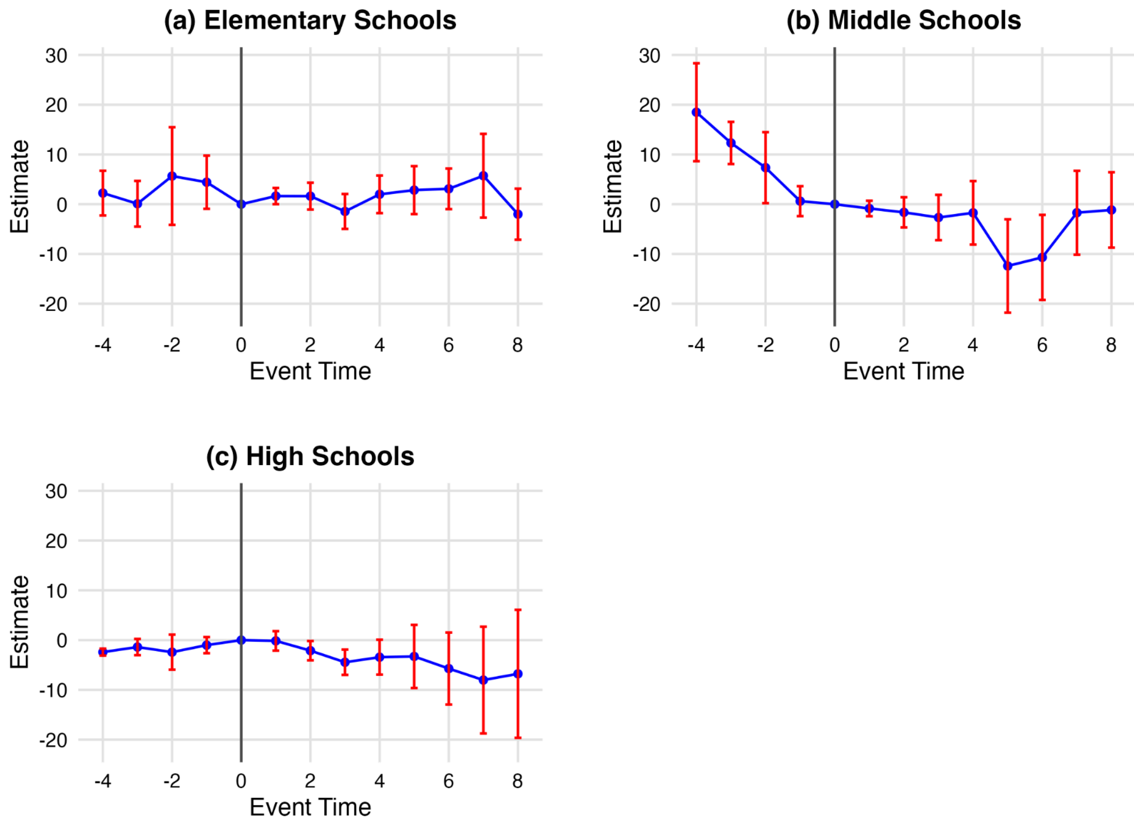
APPENDIX FIGURE 6. EVENT STUDY ESTIMATES - TEST SCORES (DISCRETE TREATMENT)

Notes: This figure mirrors the graphs in Figures 5 and 6, but uses a discrete treatment instead of a binary treatment. Year 0 represents the last year before treatment. Estimates are calculated in each year relative to Year 0. Red bars show a 95% confidence interval for each estimate.



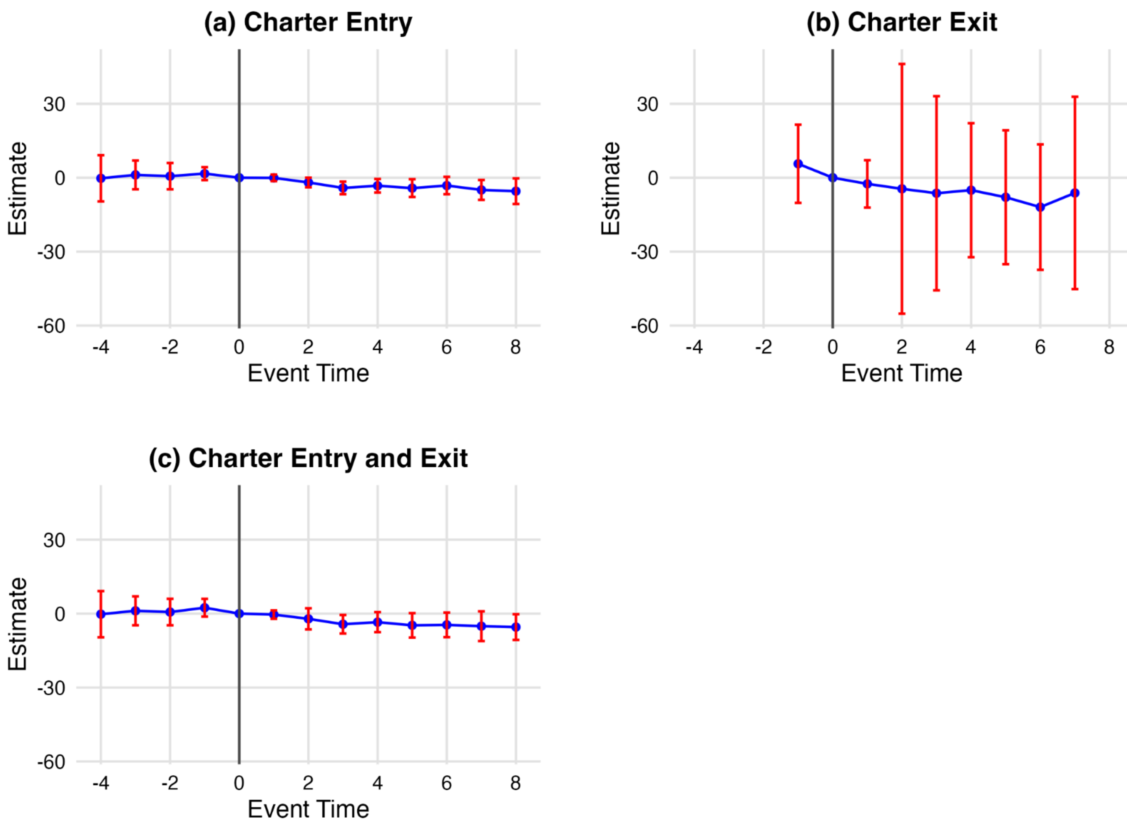
APPENDIX FIGURE 7. EVENT STUDY ESTIMATES - TEACHER STAFFING (DISCRETE TREATMENT)

Notes: This figure mirrors the graphs in Figure 7, but uses a discrete treatment instead of a binary treatment. Year 0 represents the last year before treatment. Estimates are calculated in each year relative to Year 0. Red bars show a 95% confidence interval for each estimate.



APPENDIX FIGURE 8. EVENT STUDY ESTIMATES - TEST SCORES (GRADE LEVEL DISAGGREGATION)

Notes: Subtitles indicate the treated group. Number of observations is low, and results should be interpreted with caution. Year 0 represents the last year before treatment. Estimates are calculated in each year relative to Year 0. Red bars show a 95% confidence interval for each estimate.



APPENDIX FIGURE 9. EVENT STUDY ESTIMATES - TEST SCORES (ENTRY VS. EXIT)

Notes: Subtitles indicate whether the regression includes just charter entry, just charter exit, or both entry and exit as a treatment. When both are included, the estimates measure the effect of charter entry combined with the negative effect of charter exit. The charter exit regression has very few observations, and therefore very large standard errors. Year 0 represents the last year before treatment. Estimates are calculated in each year relative to Year 0. Red bars show a 95% confidence interval for each estimate.