School Competition and Classroom Segregation

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Abstract

I study the effect of charter openings on racial segregation across classrooms at local traditional public schools (TPS). Exploiting almost 100 entries in North Carolina from 1997 to 2015 and student data, I compare segregation at TPS nearby the entry location to segregation at TPS farther away. I find that the announcement of a charter opening increases classroom segregation by between 6 and 14%. Charter entry raises the fraction of white TPS students classified as gifted, at the expense of non-white students. Overall, charter entry increases the gap between average white and non-white Math achievement at local TPS by 10%.

Keywords: charter schools; school competition; classroom segregation; racial inequality; gifted education

1 Introduction

The rapid growth of the charter school sector in the United States (Fiske and Ladd, 2016; LiBetti et al., 2019) has been fueled by the argument that competition in education markets is "a tide lifting all boats" (Hoxby, 2003). First, competition implies more options for parents, who will choose the best educational match for their children. Second, competition will induce traditional public schools (TPS henceforth) to increase their quality to retain students and, hence, financial resources[1] (Friedman, 1962; Hoxby, 2000a). Specifically, given that charter schools are more likely to locate in racially diverse areas (Singleton, 2019), school competition will benefit minority students who remain in the TPS system: these students constitute the focus of this paper. In turn, competition will help reduce the racial achievement gap, a historically unresolved node in the United States (Meatto, 2019).

That competition for enrollment will benefit the achievement of TPS non-white students via enhanced TPS productivity rests on three implicit assumptions. First, households value school characteristics that raise their children's test scores. Second, non-white students have high-quality outside options to their local TPS and hence a credible threat of leaving. Third, holding fixed the quality of the outside option, TPS care "enough" about retaining non-white students. If any of these conditions fail, then competition will not necessarily help narrow the racial achievement gap. For example, if white students value above all having white peers (Hastings et al., 2009; Abdulkadiroğlu et al., 2017), then TPS will retain white students by creating racially segregated classrooms, which is likely undesirable for non-white students' test scores and adult outcomes[2]. Surprisingly, while the distorting effects of school competition have been identified theoretically (McMillan, 2004; MacLeod and Urquiola, 2013, 2015), little empirical work has studied the spillover effects of charter schools on TPS classroom segregation[3].

[1] Funding for public schools is tied to student enrollment. Epple et al. (2018) study the school closing decision of a superintendent under alternative objective functions, including an enrollment-maximizing one.

[2] The literature on school peer effects has delivered mixed results on the achievement effects of racial segregation. There is some consensus on racial peer effects being stronger within than across racial groups (Hoxby, 2000b), with African American students (especially the high-achieving ones) paying the higher price in terms of test scores for a larger school share of African American students (Hanushek and Rivkin, 2009; Hanushek et al., 2009). As for ability peer effects, some, but not all papers show that high-achievers are those who benefit the most from other high-achievers (Sacerdote, 2011).

[3] An exception is Dalane and Marcotte (2021), who study the impact of charter school penetration and

In this paper I study the effect of a charter school opening on racial segregation across classrooms at local TPS in North Carolina, a state with a large and lively charter school sector whose appeal to households, especially white households, has inflamed a strong opposition among traditional pubic school leaders (Stoops, 2019; Schwartz, 2012). I exploit 97 openings of elementary charter schools occurred in North Carolina over the two waves of entries: that started in 1997 upon the approval of the North Carolina Charter School Act, and that begun in 2012 in the wake of President Obama's Race To The Top initiative. I estimate an event study specification, where "treated" and "control" schools are defined based on their distance to the charter opening location. I measure racial segregation across classrooms at North Carolina elementary TPS for the school years 1994-1995 to 2017-2018 using section- and student-level data from the North Carolina Education Research Data Center (NCERDC). For every school, year, grade, term, and Math course, I measure cross-section racial segregation using the index of dissimilarity (Duncan and Duncan, 1955), distinguishing between white and non-white students. I then obtain a school-by-year measure of classroom segregation by averaging the index first within and then across grades.

I estimate that a charter opening has a positive and statistically significant effect on classroom segregation at local TPS. The size of the increase is economically relevant: more than 6% of the pre-opening average by the time the charter school enters the third year of operation. This means that the fraction of non-white students that should change section to achieve a racially even distribution increases by 6% on average upon the opening of a charter school in the local education market. The increase in classroom segregation is larger in urban, majority non-white areas, as well as at TPS with a relatively low level of classroom segregation at the baseline.

Crucially, the increase in classroom segregation is not a mechanical by-product of the change in the TPS student body composition caused by the charter opening. To demonstrate this, I exploit the time lag between opening announcement and actual opening of entire schools or single grades to show that TPS classroom segregation rises as soon as the opening information is made available, but before students can actually transfer to or enroll in the

proximity on within-TPS income segregation in grades 3 to 8. The main difference between their work and mine is that I am able to show that the increase in classroom segregation is not a mechanical by-product of the change in the TPS student body composition caused by the charter opening.

new charter school. In the first robustness check, I restrict the set of events to the 43 charter schools whose entry is announced more than a full school year before the actual opening date. I find that TPS classroom segregation increases as soon as the opening information is disclosed. The effect in the announcement year is statistically significant, positive, and 1.4 times larger than the immediate effect estimated using the full set of entries. In the second robustness check, I focus on the 21 charter schools that open with a certain grade configuration, but in their application files commit to expand their grade offer starting from the second year of operation or later. I then estimate the immediate effect of these charter openings on classroom segregation within such "promised" grades. The coefficient is statistically significant, positive, and 2.6 times as large as the one obtained using all entries. These findings allow me to interpret the increase in classroom segregation as part of the strategic response of local TPS to charter entry. Besides, according to the results of my robustness checks, classroom segregation increases by 6.5% to 14% upon charter entry.

The rise in racial segregation is accompanied by neither an increase in ability tracking, nor a reallocation of high value added teachers across sections. Besides, average class size increases slightly for both white and non-white students, pointing out to some teachers being reallocated toward individualized learning or other non-standard teaching activities in response to competition.

Increasing racial segregation across regular classrooms can serve as a way to increase the quality of the Gifted and Talented programs that North Carolina TPS are required to offer. In the typical format, gifted students are grouped within the regular classroom and assigned a dedicated special teacher. Besides, gifted education in most North Carolina school districts is funded by the State only, and in a fixed amount that does not depend on the actual number of Gifted and Talented students. In principle, since most gifted students are white, grouping white students together has therefore the potential to reduce the number of gifted teachers that TPS need to hire, thus allowing for higher-salary teachers. The racial composition and quality of Gifted and Talented programs are dimensions that TPS can manage strategically, along with regular classroom formation. I find that charter entry comes along with an average 6% reduction in the within-school share of students classified as gifted. This reduction masks a 6% raise in the fraction of white students with the Gifted and Talented status and an over

30% reduction in the fraction of non-white students with the same status. These findings are consistent with TPS using gifted education strategically to retain white students. This is achieved partly through higher gifted spending per pupil and partly through new white student recruitment, at the expense of the non-white representation in Gifted and Talented programs.

I find that the net effect of charter entry on the within-school gap between white and non-white Math achievement at local TPS is a 10% increase. This finding relates my work to Bau (2022) in that it highlights the importance of considering schools' response to competition along the horizontal quality dimension as a potential source of inefficiency and increased inequality.

More broadly, this paper contributes to the literature on the distorting effects of competition in incomplete markets (McMillan, 2004; MacLeod and Urquiola, 2013, 2015). While most of this literature is theoretical[4], I provide empirical evidence of TPS responding to competition manipulating the allocation of educational inputs in a way that pleases white students at the expense of their non-white schoolmates. This paper also relates to the line of research that studies the effect of charter entry on local TPS outcomes, with a focus on vertical quality. The results are mixed: while some papers find competitive gains (Sass, 2006; Booker et al., 2008; Winters, 2012; Cordes, 2018; Ridley and Terrier, 2018), other show evidence of no (Bettinger, 2005; Bifulco and Ladd, 2006; Zimmer and Buddin, 2009; Slungaard Mumma, 2022) to negative (Imberman, 2011) effects. Gilraine et al. (2021) show that TPS competitive response depends on the perceived degree of substitutability between TPS and charter entrant.

This paper shifts the attention from average to distributive considerations, highlighting the importance of taking school incentives and parental preferences into account when evaluating the consequences of competition.

Furthermore, this work relates to the studies that show the potential costs of discretion in the public sector within the hiring context (Xu, 2018; Brassiolo et al., 2020; Colonnelli et al., 2020; Akhtari et al., 2022; Moreira and Pérez, 2021). I argue that a decentralized TPS system, characterized by discretion in allocating educational inputs and enrollment-

[4]Allende (2019) and Bau (2022) are noticeable exceptions.

maximizing incentives, can distort any expected competitive gains in the public education sector.

This paper is structured as follows. Section 2 describes the charter school sector in North Carolina. Section 3 explains the empirical strategy. Section 4 describes the data. Section 5 illustrates the main result. Section 6 investigates some potential mechanisms and the effect of charter entry on within-school test score inequality. Section 7 concludes.

2 Charter Schools In North Carolina

Charter schools were introduced in North Carolina in 1996, as the North Carolina Charter School Act authorized the opening of up to one hundred charter institutions within the state. The sector grew in two main waves. The earliest started in 1997, right upon the Act's approval, and finished in 2005, when the 100-cap started to bind.[5] The second wave began in the Fall of 2012 in response to the cap removal signed into law in the June of 2011 which was spurred by Obama's Race To The Top. Charter schools are predominantly concentrated in the high-density urban areas of Charlotte, Raleigh, Durham, and Chapel Hill (Figure 2).

If oversubscribed, charter schools have to admit students by lottery. Even with this restriction, charter schools can pick students through their location choice. Besides, charter schools may succeed at attracting white students through multiple channels, such as curriculum choices, expected parental commitment (e.g., weekend on-campus activities) and/or advertising. The State legislature itself may reinforce these enrollment patterns by allowing charter schools to forego offering free transportation, free meals, and after school programs (Bryant, 2017), which are oftentimes appealing to low-income families. As a matter of fact, the average charter school in my sample enrolls a percentage of white students that is significantly larger than that at local TPS (Table 1). Likely as a result, school leaders of the traditional public system have firmly and openly opposed the expansion of the charter sector in North Carolina. In a letter written on June 3, 2019[6], the Wake County Public School System, one of the largest in North Carolina, objects to the opening of five additional charter

[5] Figure 1 plots the number of operational charter schools in North Carolina from 1997 to 2017.

[6] The full text of the letter is available at https://www.documentcloud.org/documents/6188981-Wake-Schools-Letter-to-BOE.html (accessed: July 4, 2022)

schools in Wake County. They claim that, first, "The saturation of charters is (...) contributing to *de facto* segregation in Northeastern Wake County" and, second, no additional seats, innovation, or choice would be needed, "given the national reputation of Wake County's magnet program and the ability of its non-magnet schools to offer a wide range of program enhancements". In the same period, Wake County Commissioner Greg Ford tweeted that Wake County taxpayers "will fork over $42,312,228.60 to charter schools, who cherrypick their students and have absolutely no local (and very little state) accountability." (Stoops, 2019). In Chapel Hill, the proposed opening of the Howard and Lillian Lee Scholars charter school ignited a similar debate in 2012. According to the Chapel Hill-Carrboro City Schools Board, the new school would impede the district's ability to enact reforms that the charter applicants champion, while siphoning money away from traditional schools (Schwartz, 2012). These examples are representative of the hostility and concerns with which TPS often welcome charter schools within the local education market.

3 Empirical Strategy

To measure the effect of charter entry on TPS classroom segregation, I estimate an event study specification that compares TPS that face a near entry (i.e. an entry within five miles) to TPS that do not. The events are 97 openings of elementary charter schools occurred in North Carolina during the two major waves of entries: that started in 1997 and ended 2005 (54 openings), when the 100-cap first became binding, and that started in 2012. Of the latter, I consider the 43 openings occurred between 2012 and 2015. Section A.1 in the Appendix explains how I identify these 97 entries and their locations. Figure B.1 plots their distribution across school years.

I define schools located within five miles of a charter opening as treated by that charter opening (Gilraine et al., 2021). The intuition is that schools compete spatially, leveraging parents' distaste for distance (Hastings et al., 2009; Abdulkadiroğlu et al., 2017). Hence, the competitive forces exerted by a charter school will be strongest among TPS located near the charter location. As long as near and distant TPS experience parallel trends in classroom segregation before entry, any divergence between the trends *upon* entry can be interpreted as the effect *of* entry on TPS classroom segregation.

Sun and Abraham (2021) show that naive event study estimates can be biased when there is variation in the time of the treatment across units and treatment effect heterogeneity. This is because the estimated effect for one relative-time period will generally be contaminated by the causal effects of other periods and hence will not isolate the average treatment effect of interest. To rule out this possibility, I follow Cengiz et al. (2019) to mechanically ensure that no previously treated units enter the control group. I first restrict my analysis to TPS that experience one or more charter entries within a radius of 10 miles between 1997 and 2017. I define a school s treated in year c if it faces a charter entry closer than 5 miles in year c[7]. Then, for each entry cohort C_c, I keep only the observations corresponding to TPS that are treated in year c, as well as those that are not treated by year $c+2$, where 2 is the outer most relative year that I want to test: these are "clean" controls within the treatment window. Next, for each cohort-specific data set, I only keep observations within years $c-3$ and $c+2$ and I generate a data set-specific identifying variable. Lastly, I stack together all the cohort-specific data sets in relative time and estimate

$$
\begin{aligned}
D_{stc} = \alpha + \sum_{k=-3}^{-2} \beta_k 1[\tau_{tc} = k] 1[treated_{sc} = 1] + \sum_{k=0}^{+2} \gamma_k 1[\tau_{tc} = k] 1[treated_{sc} = 1] + \\
+ \delta X_{st} 1[entry\ cohort = c] + \phi_{sc} + \phi_{tc} + \epsilon_{stc}
\end{aligned}
\tag{1}
$$

where s denotes the school, t the year, c the entry cohort; D_{stc} is a measure of racial segregation across classrooms; α is a constant term; $\tau_{tc} = k$ if t is k years away from c ($\tau_{tc} = -1$ is omitted)[8]; $treated_{sc} = 1$ if school s is treated in c; X_{st} is a set of school-by-year controls (number of students and share of white students enrolled); ϕ_{sc} and ϕ_{tc} are school-by-entry-cohort and time-by-entry-cohort fixed effects; ϵ_{stc} is the residual error term. The only difference between this specification and the traditional dynamic difference-in-differences is that school and year fixed effects are saturated with indicators for the data set identifiers

[7]See Section A.2 in the Appendix for details on how I identify treated and control TPS.

[8]Entries occurred between 2003 and 2005 and between 2013 and 2015 were authorized at least one full school year before the actual opening. For these cohorts, $k = 0$ is therefore the year before opening. See Section A.4 in the Appendix for details.

(Baker et al., 2022).

4 Data

4.1 Measuring Racial Segregation Across Classrooms

I measure racial segregation across classrooms at North Carolina elementary TPS (grades 1 to 5) using administrative records from the North Carolina Education Research Data Center (NCERDC). For every school, year, grade, term, and course, I measure cross-section racial segregation using the index of dissimilarity (Duncan and Duncan, 1955). This index measures the fraction of non-white (or white) students that should change section to achieve an even distribution and varies from zero (perfect evenness) to one (complete segregation). For each school, year, grade, term, and course, the index is defined as

$$D = \frac{1}{2} \sum_{s} \left| \frac{w_s}{W} - \frac{nw_s}{NW} \right| \tag{2}$$

where w and nw denote the number of white[9] and non-white students in each section s and W and NW are the totals. I compute D for each Math course that meets the following characteristics: (i) the course is offered to any grade between one and five; (ii) the course has only regular-sized sections with 5 to 40 students[10]; (iii) the course has more than one section; (iv) the student body composition of the course has some racial diversity (i.e., W and NW are strictly larger than zero).

I compute the school-by-year index of dissimilarity averaging D first within and then across grades[11]. The resulting data set is a panel of elementary TPS whose levels of cross-classroom racial segregation are measured from 1994 to 2017.

While the data is very suitable for studying racial segregation across classrooms and its evolution over time, the same is not true for other forms of segregation, such as segregation

[9] I follow Card and Giuliano (2014) and classify Asian students among White students. The fraction of Asian students in North Carolina is around 3%.

[10] The main result is robust to substituting (iii) with a less restrictive criterion: see Section A.3 in the Appendix for details.

[11] The main result is robust to computing a weighted average index, where each course and grade are weighted by their relative enrollment size. Results are available upon request.

by ability or by socio-economic status. The only classroom breakdown available for years 2006 and earlier is that by race and by exceptionality/impairment status. For later years, the match between classroom assignment and other individual information, such as test scores or indicators of economic disadvantage, is relatively poor. It is therefore impossible to reconstruct the exact composition of sections by ability or socio-economic status. This work focuses on racial segregation as I can get a high quality measure for it. I briefly discuss how my main results relate to ability tracking in Section 6.

4.2 Descriptive Statistics

Table 1 shows some descriptive statistics for my estimation sample. It includes 639 elementary TPS that experience at least one charter entry within 10 miles between 1997 and 2017. Of these 639 schools, 335 face some near openings (i.e. openings at a distance smaller than 5 miles) in the entry years that I consider. The median number of years in which such schools experience a near entry is one; the mean is two. Charter schools open in relatively non-white areas: the average share of white students at TPS that face near openings is 39%, much smaller than the sample average of 58%. This seems particularly true for second-wave openings, where the average share of white students at local TPS is 29%. Importantly, the average share of white students enrolled at charter schools upon opening lies consistently around 50%: this implies that the average charter school, albeit located in a relatively non-white neighborhood, succeeds at skimming white students away from local TPS. The relatively large variability in the student body composition enrolled at charter schools suggests that charter schools are heterogeneous in the type of student that they target.

Figure 3 shows the patterns in the raw dissimilarity index for schools in my estimation sample that do and do not face a near entry at 0, the normalized time of treatment. The red line plots average values of the school dissimilarity index by year for TPS that face a near entry at 0 (i.e., "treated" TPS), while the blue line plots the same averages for TPS that face no near entry up to +2 (i.e., "control" TPS). The dissimilarity index at treated and control TPS appears to follow quite similar trends before entry, which supports my identifying assumption that segregation at treated and control TPS would move in parallel in absence of entry. Upon entry, average segregation at treated TPS visibly increases relative to control TPS. As I now discuss, my main event study estimates are consistent with these patterns.

5 Results

5.1 Main result

Figure 4 shows the results obtained from estimating equation 1. The horizontal axis reports the time relative to charter opening. The vertical axis measures the change in average segregation estimated off equation 1. The coefficients at -3 and -2 are not significantly different from zero, supporting the identifying assumption that treated and control schools would move in parallel in absence of entry. Differently, the post-entry coefficients are statistically significant and positive.

The first column of Table 2 reports the estimated coefficients and standard errors. Racial segregation increases by almost 5% of the pre-entry average right upon entry, and up to 6.3% by +2. In other terms, charter entry increases the average fraction of TPS students that should be moved to a different section to achieve an even distribution by more than 6% within two years. Using a different measure of segregation, Monarrez et al. (2022) study the causal effect of charter schools on racial segregation across schools within the public system. Exploiting between-grade differences in charter expansion and charter openings for identification, the authors find that, on average, charters have caused a 6 percent decrease in the relative likelihood of Black and Hispanic students being exposed to schoolmates of other racial or ethnic groups. The authors define the magnitude of the effect modest. If 6 percent can be thought of as a modest effect in absolute terms also in my context, the estimate is highly economically significant once contextualized. First, the average treated school in my sample faces a near charter entry in two distinct years. This implies that the overall increase in classroom segregation is twice as large for the average treated school. Second, 6 percent is a lower bound for my effect, as illustrated in Section 5.2. Third, my result complements that in Monarrez et al. (2022): charter openings increase racial segregation not only across schools, via parental choice, but also within traditional public schools, due to TPS competitive response. Accounting for both mechanisms significantly change the conclusion on the magnitude of the increase in racial segregation due to the expansion of the charter sector.

The results in Figure are almost identical if classroom segregation is measured for Reading and English Language Arts courses (see B.2 in the Appendix).

5.2 Ruling out mechanical changes

A concern with the result in Figure 4 is that segregation could increase mechanically as TPS students leave to join the newly opened charter schools. The idea that segregation indices may not be comparable over time if the population composition varies is called margin-dependence in the segregation literature. (Elbers, 2021) notes that, theoretically, the dissimilarity index is not margin-dependent in terms of population composition (in this context, the racial composition of the course)[12]. Notwithstanding, I exploit two features of the institutional context (illustrated in Section A.4 of the Appendix) to rule out that the increase in Figure 4 is mechanical.

First, for the entry cohorts 2003 to 2005 and 2013 to 2015, the outcome of the application process was announced more than a year before the actual opening[13]. For example, charter schools approved to open for the 2013-2014 school year were shortlisted by June 2012, and so TPS in the 2012-2013 school year would know whether a charter school would open nearby the following year. This timing implies that, for such entry cohorts, local TPS had time to respond before the opening happened. At the same time, any change in TPS segregation before the charter opening cannot be driven by compositional changes induced by the charter opening and can therefore be interpreted as the "clean" TPS competitive response. Figure 5 shows the results obtained from estimating equation 1 using as events only the 43 openings occurred within the time periods 2003-2005 and 2013-2015. The coefficient of interest is that at 0, the announcement, pre-opening year. The pattern looks quite similar to that in Figure

[12]The dissimilarity index is in fact margin-dependent with respect to relative category (section) sizes.

[13]Gilraine et al. (2021) use an analogue strategy to isolate the TPS test score response to 2013 charter entries in North Carolina, differentiating between charter schools that are horizontally differentiated vs not. Figlio and Hart (2014) exploit a similar setup in Florida, where public schools started to feel competitive pressure before students could move as access to private school vouchers would only become available the following year.

4, where I use all entries. Specifically, as reported in Table 2, the coefficient at 0 obtained using pre-announced entries only (column 2) is statistically significant, positive, and 40% larger in magnitude as the one obtained using all entries (column 1). This result implies that my estimated effect is stable as if I limit my analysis to a context where the flow-out of students is not a concern by construction.

Second, I exploit the fact that 21 of my charter schools open with a certain grade configuration, but in their application files commit to expand their grade offer starting from the second year of operation or later. Once again, finding that classroom segregation at local TPS increases within such "promised" grades even before the charter school starts to offer them would point out to a TPS competitive response that is not driven by students flowing out. Figure 6 shows the results that I obtain as I estimate equation 1: (i) restricting the set of events to the 21 charter schools that commit to some grade expansion; (ii) recalculate my index of segregation at TPS, including only the grades that the charter school opening nearby commits to add in the near future. More precisely, I create five data sets (one for each grade between 1 and 5), following the procedure described in Section 3. When constructing the data set for grade g, I only consider as events the charter entries that promise they would add grade g to their offer. Also, the dissimilarity index at TPS is computed within grade g only. I then proceed by appending these five data sets and estimating equation (1), replacing school-by-entry-cohort fixed effects with school-by-grade-by-entry-cohort fixed effects. Once again, the coefficient of interest in Figure 6 is that at 0, when none of the "promised" grades has been activated yet. The coefficient is positive and statistically significant. Column 3 in Table 2 reveals that the coefficient is 2.6 times as large as the one obtained using all entries (column 1).

Overall, this evidence shows that my main result survives as I restrict myself to contexts where TPS know that a charter school will open close by, but the opening has not yet taken place. The increase in classroom segregation reported in Figure 4 can therefore be interpreted as part of the strategic response of local TPS to charter entry.

5.3 Other Robustness Checks

Distance cutoff: A potential concern with Figure 4 is that the results are specific to the choice of the 5-mile treatment cutoff and would not generalize to alternative distance-based definitions of treatment. Figure B.3 in the Appendix shows what I obtain as I use a continuous measure of distance to define treatment instead of the 5-mile cutoff. Specifically, I re-estimate equation 1 (using my main sample of TPS located within 10 miles of a charter opening) measuring treatment through a continuous measure of distance between the TPS and charter school locations. My continuous measure is 10, the maximum distance in my sample, minus actual distance: it tends to 10 for very close TPS, to 0 for relatively distant TPS, while I set it equal to 0 for control TPS, i.e. those that experience no entry within 10 miles up to the entry year under analysis. If smaller distance implies a stronger competitive threat, then I expect a larger treatment effect for relatively larger values of my continuous measure. This is exactly what Figure B.3 shows. The coefficient at 0 implies that a one-mile reduction in TPS-charter distance increases the estimated treatment effect by 0.002, which is remarkably close to the estimate implied when using the 5-mile cutoff[14].

Dissimilarity index: A further concern with Figure 4 is potential misspecification: by definition, my dependent variable ranges from 0 to 1, but the predicted values from an OLS regression can never be guaranteed to lie within the unit interval. I address this drawback by re-estimating equation 1 with the following transformation of the original dependent variable

$$D'_{stc} = log\left(\frac{D_{stc}}{1 - D_{stc}}\right) \tag{3}$$

which can take on any real value[15]. My results do not change in any significant manner as I transform the dependent variable according to equation 3 (see Figure B.4 in the Appendix).

Lastly, a potential issue with the dissimilarity index is that it measures deviation from

[14] Among treated schools in our main specification, the average distance to the newly opened charter school is 2.35 miles. Among control schools, it is 6.78 miles. The estimate of 0.002 per mile implies a difference in classroom segregation between treated and control schools of 0.009 (0.002 times the difference between 6.78 and 2.35), which is very close to that displayed in Figure 4 (coefficient at 0).

[15] The transformed variable is not defined for $D_{stc} = 0$ or $D_{stc} = 1$, which occurs for a negligible number of observations in my sample.

evenness, not from randomness. Under some circumstances, even random allocation might deliver a non-null index of dissimilarity, implying that it is impossible to tell apart systematic segregation from segregation obtained by pure chance. For example, a small overall proportion of non-white students is more likely to be unevenly distributed across classrooms by chance, compared to a larger minority group. This issue is particularly acute with small classroom sizes (Allen et al., 2015). The econometrics literature has proposed two main approaches to deal with the so-called small unit bias (D'Haultfœuille et al., 2021). For computational convenience, I follow Carrington and Troske (1997) and correct the naive dissimilarity index used in Figure 4 by subtracting from it an estimate of the dissimilarity index under random assignment, i.e. under no systematic segregation[16]. Figure B.5 in the Appendix shows that my results are qualitatively unaltered as I use the corrected index, suggesting that the change in segregation that I observe upon charter entry is not driven by an increase in the severity of the small unit bias.

5.4 Treatment Effect Heterogeneity

Figure B.6 in the Appendix plots the coefficients obtained from estimating the following variation of equation (1)

$$D_{stc} = \alpha + \gamma 1[\tau_{tc} \geq 0]1[treated_{sc} = 1] + \delta X_{st}1[entry\ cohort = c] + \phi_{sc} + \phi_{tc} + \epsilon_{stc} \tag{4}$$

separately for each charter opening. The only difference between (4) and (1) is that (4) averages pre- and post-entry periods into one single pre- and one single post-entry period.

Of the 78 coefficients plotted in Figure B.6[17], 45 are positive and 33 are negative. The median coefficient (0.006) is less than half the size of the mean (0.013), while the third quartile (0.041) is more than twice as large in magnitude as the first quartile (-0.018). Coefficients are larger on average for charter entries that take place in urban areas (48 entries; average coefficient 0.016) as opposed to entries in rural areas (30 entries; average coefficient 0.009),

[16] I compute the corrected index using the Stata command *segregsmall*.

[17] I cannot obtain an estimate for 19 of the 97 entries as sample size is too small.

although the difference is not statistically significant. Besides, treatment effects are larger and positive on average in areas that are majority non-white (Figure B.7) and where racial segregation across classrooms is smaller at the baseline (Figure B.8). Taken together, these results suggest that the TPS response is stronger where: (i) competition is relatively more intense; (ii) white students are a minority, meaning more likely to be unsatisfied with their local TPS and hence more "marginal"; (iii) the cost of increasing classroom segregation at the margin is small, as baseline segregation is relatively low to start with.

6 Mechanisms

This section explores whether increased racial segregation is accompanied by a reallocation of other educational inputs, such as high-ability peers (conditional on race), high value added teachers, or smaller sections. It also explores how within-school test score inequality among TPS responds to charter entry.

Given the data limitations described in Section 4, the analyses presented here only rely on charter openings occurred between 2012 and 2015. Table 3 reports estimates for equation 4, separately for 1997-2005 and 2012-2015 openings. The coefficient of interest, $\hat{\gamma}$, is positive for both entry waves. Specifically, 2012-to-2015 entries lead to an average increase in racial segregation across classrooms equal to more than 5% of the pre-entry mean over the first three years of charter operation.

6.1 Ability Segregation

Whether increasing racial segregation will raise segregation by ability, too, is *a priori* ambiguous and depends on multiple factors, such as the magnitude of the racial achievement gap within courses[18], the baseline allocation of ability across sections, as well as TPS incentives. For example, TPS may care about both student retention and test scores, for accountability or reputation reasons. If only racial segregation helps retain the students that would otherwise leave, while minimizing ability segregation is beneficial to average test

[18] In the North Carolina school population, white students significantly outperform their non-white peers. This is shown in Figure B.9, which plots the percentiles of the distribution of standardized Math test scores in third grade for white and non-white students between 2007 and 2018.

scores, then TPS will segregate by race in the way that has the smallest possible impact on the magnitude of ability tracking.

I shed light on the effect of charter entry on TPS ability segregation by estimating equation 4, changing the dependent variable into a measure of tracking across sections. As a proxy for ability, I use end-of-grade math test scores in third grade, the earliest for which achievement measures are available. I then compute an index of cross-classroom ability segregation at the school-by-year level for grades 4 and 5[19]. I measure segregation by ability using the ordinal version of the information theory index proposed by Reardon (2009) to deal with the fact that ability categories are intrinsically ordered and, hence, not interchangeable. The index is defined as

$$I = \sum_{m=1}^{M} \frac{t_m}{T\nu}(\nu - \nu_m) \tag{5}$$

where m denotes sections, t_m is number of students in section m, T is the total number of students, while ν is defined as

$$\nu = \frac{1}{K-1} \sum_{j=1}^{K-1} -[c_j log_2 c_j + (1-c_j) log_2 (1-c_j)] \tag{6}$$

with $K = 4$, the number of ordered categories (quartiles, in this case) and c_k the cumulative proportion of students in quartile k or lower. Quartiles are defined at the school-by-year-by-grade-by-term-by-course level. As for racial segregation, I average I first within and then across grades to obtain a measure of ability segregation by school-year.

To understand whether the change in ability segregation that I observe is the mechanical by-product of the increase in racial segregation shown in Figure 4, or if rather TPS intentionally respond to charter entry along the lines of ability tracking, I compare the estimates obtained with two distinct measures of ability segregation. One measure relies on the classroom formation that I observe in the data and captures, therefore, the actual level of ability

[19]See Section A.6 for details.

segregation in a given school and year. The other one is simulated and measures the level of ability segregation that one would observe if students were randomly assigned to sections, under the only constraint that the number of white and non-white students per section has to match the actual one[20].

Table 4 shows the results obtained from estimating equation 4 with the actual (column 1) and simulated (column 2) index of ability segregation as the dependent variable, for the entry cohorts 2012 to 2015. The estimated effect on actual ability segregation is negative, but highly imprecise (column 1). This means that, upon charter entry, students are allocated across sections in a way that increases racial segregation without altering segregation by ability in any statistically detectable manner. Interestingly, the increase in ability segregation is non significantly different from zero even if students are randomly assigned to classrooms, conditional on the racial configuration (column 2). The lack of statistical significance under random allocation suggests that, in my sample, there is a decent amount of overlap between the white and nonwhite within-course test score distributions. In other terms, non-white students perform "well enough", relative to white students, to prevent ability segregation from increasing mechanically as racial segregation goes up.

6.2 Other Classroom-Related Educational Inputs

Teacher quality and class size: Classroom assignment determines not only the peer composition that students are exposed to, but also other educational inputs such as teacher effectiveness and class size. I estimate whether charter entry has a differential impact on the teacher quality and section size that white and non-white students are exposed to. I measure teacher quality through value added, which I estimate using a Parametric Empirical Bayes estimator (Morris, 1983; Rothstein, 2010; Chetty et al., 2014) and exploiting the possibility of linking student test scores and characteristics to teacher identifiers in the data.[21] I then obtain estimates for equation 4 where the outcome variables are: (i) the average teacher value added to which white students are exposed; (ii) the average teacher value

[20]For every school, year, grade, term, and course, I draw five different classroom configurations under random assignment, and compute five simulated indices. The right panel of Table 4 shows the results obtained using the average of the five indices as the dependent variable.

[21]See Section A.5 in the Appendix for details.

added to which non-white students are exposed; (iii) the average class size that white students experience; (iv) the average class size that non-white students experience. Table 5 reports the results. I estimate no significant effect on teacher value added (columns 1 and 2), implying that TPS do not reallocate teachers to white or non-white students based on their effectiveness as the competitive incentives change. However, I find that both white and non-white students face a modest (smaller than 1%) increase in average class size (columns 3 and 4) that gains magnitude and precision starting from the first year after opening (see Figure B.10 in the Appendix). This result is consistent with some teachers being shifted from general classrooms, whether mostly white or non, to individualized learning or other non-standard teaching activities.

Gifted status: One of the potential side consequences of increasing racial segregation across classrooms is that TPS might be able to increase the quality of their Gifted and Talented programs. Most North Carolina Gifted and Talented students receive their education from a dedicated special teacher while grouped within their regular classrooms[22]. Since most Gifted and Talented Students are white[23], and gifted programs in North Carolina are generally funded by the State in a fixed amount that does not depend on the actual number of Gifted and Talented students, grouping white students together can imply higher quality of gifted education through the hiring of fewer gifted teachers at a higher salary.

More broadly, how TPS use gifted education to retain white students rests on a trade-off between instructional quality and number of white students enrolled in the program. On the one hand, white students classified as Gifted and Talented before charter entry can be retained through an increase in the quality of gifted education. Quality can be increased by either grouping white students within the same regular classrooms, along the lines described

[22] The supply of gifted education is mandated to all North Carolina TPS by the State General Statuses, Article 9B. Each district is responsible for compiling a three-year plan, establishing student identification criteria, instructional contents, as well as class and grouping formats. The most common instructional format consists of grouping gifted students within the regular classroom, while assigning them a dedicated teacher. Oftentimes, extra activities for gifted students are organized at the school or district level, beyond the standard school schedule. Funding for gifted education comes mostly from the State, which pays each district a fixed amount ($1,340.97 as of 2018-19) times 4% of the district Average Daily Enrollment, regardless of the actual number of gifted students. Districts are not allowed to transfer out any portion of such funds. Gifted education for charter schools is neither mandated nor funded by the State.

[23] For the school year 2016-2017, slightly less than 5% of non-white and 17% of white students in NCERDC data are classified as Gifted and Talented. More than 77% of the Gifted and Talented students are white.

above, or by reducing the school share of students enrolled in the Gifted and Talented program. On the other hand, recruiting new white students in the Gifted and Talented program might also serve as a retention device by increasing the relative appeal of TPS to a larger number of white households. Table 5 shows that charter entry comes along with a 6% raise in the fraction of white students with the Gifted and Talented Status (column 5) and an over 30% reduction in the fraction of non-white students with the same status (column 6) at local TPS, relative to TPS farther away. The net effect is a 6% average reduction in the school share of Gifted and Talented students[24]. Overall, these results are consistent with TPS using gifted education strategically to retain white students. This is achieved partly through higher gifted spending per pupil and partly through new white student recruitment, at the expense of the non-white representation in Gifted and Talented programs.

6.3 Test Score Inequality

What is the overall effect of charter entry on within-school test score inequality at TPS? In this paper I show that a charter opening increases cross-classroom racial segregation at local TPS by more than 6%. While the allocation of teacher value added and class size remains virtually unaffected, charter entry comes along with smaller and yet "whiter" Gifted and Talented programs. In the perspective of test scores, there is no consensus in the literature on the extent to which peers and gifted education actually contribute to learning. The results on racial and ability peer effects are quite mixed. Some consensus has emerged on the importance of non-linearities (Hoxby and Weingarth, 2005; Hanushek and Rivkin, 2009; Lavy et al., 2012; Imberman et al., 2012; Burke and Sass, 2013), with some, but not all papers showing that high-achievers are those who benefit the most from other high-achievers (Sacerdote, 2011). At the same time, racial peer effects seem stronger within than across racial groups (Hoxby, 2000b), with African American students (especially the high-achieving ones) paying the higher price in terms of test scores for a larger school share of African American peers (Hanushek and Rivkin, 2009; Hanushek et al., 2009). As for gifted education, the literature finds no to small average effects (Bui et al., 2014), with non-white and disadvantaged students having the largest gains (Card and Giuliano, 2014).

[24] Results are available upon request.

All considered, the net effect on charter entry on the distribution of test scores within TPS is *a priori* ambiguous.

Table 5 shows the results obtained for equation 4, where the outcome variables are the within-school difference between the 90th and 10th percentile of the Math test score distribution (column 7), and the within-school gap between average white and non-white Math performance (column 8). All the variables are measured at the grade-by-school-by-year level, using standardized end-of-grade Math test scores for grades three to five, and then averaged across grades within school[25]. Table 5 reveals that both coefficients are positive and statistically significant. In terms of magnitude, charter entry increases the 90th-to-10th-percentile range by about 2.5% and the racial Math gap by 10% of the pre-entry average. This result is important in light of the evidence that TPS respond to charter entry by increasing their vertical quality and, therefore, improving average outcomes (Sass, 2006; Booker et al., 2008; Winters, 2012; Cordes, 2018; Ridley and Terrier, 2018; Gilraine et al., 2021). My findings underscore the importance of considering the horizontal quality response, too, as average improvements may come along with increased inequality. In Bau (2022)'s work, the dimension of horizontal quality at work is the school curriculum choice: as competition intensifies, profit-maximizing private schools choose their instructional content in a way that advantages wealthier students, who are the most marginal, increasing inequality and reducing overall learning. Here, the engine is classroom composition and peer quality: TPS optimally embed white households' preferences and respond to competition by segregating white students across regular classrooms and gifted programs. This increases the extent to which non-white students lag behind their white peers.

7 Concluding remarks

In this paper I exploit almost 100 charter school openings in North Carolina and rich student-level administrative data to show that racial segregation across Math classrooms at TPS increases as a charter school opens in the physical proximity. The effect is statistically significant and large: more than 6% by the time the charter school enters the third year of

[25] See Section A.6 in the Appendix for details.

operation. Importantly, I demonstrate that the increase in classroom segregation is not a mechanical by-product of the change in the TPS student body composition caused by the charter opening. Specifically, I exploit the time lag between opening announcement and actual opening of entire schools or single grades to show that TPS classroom segregation increases as soon as the opening information is made available, but before students can actually transfer to or enroll in the new charter school.

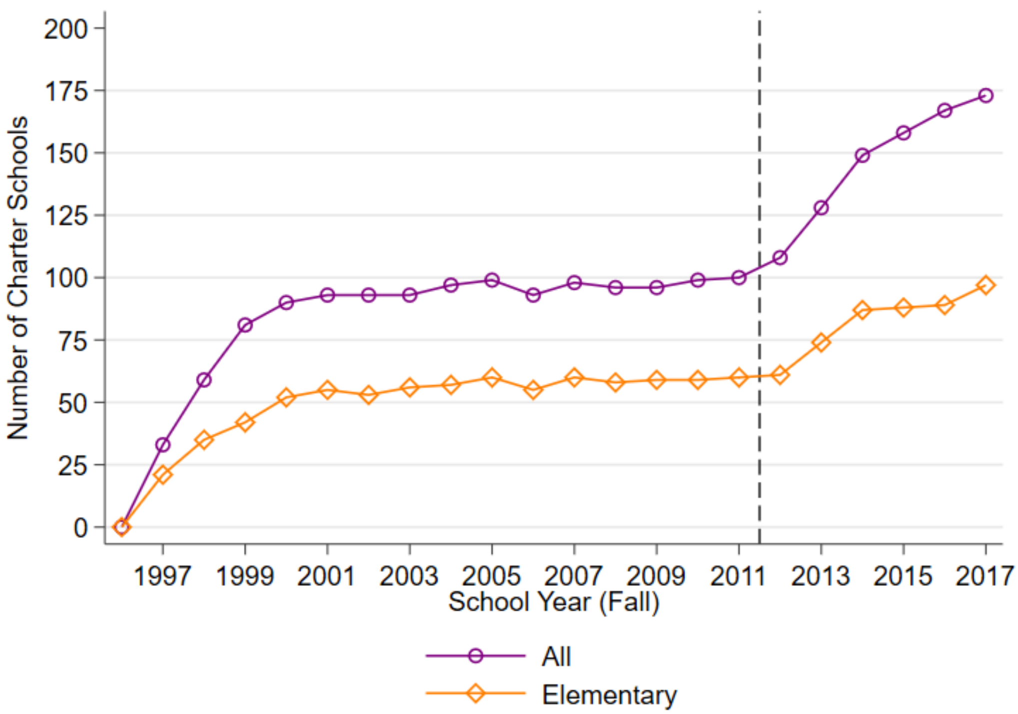
Higher racial segregation is not accompanied by a reallocation of high value added teachers across white and non-white students, and average class size increases slightly for both racial groups. However, charter entry comes along with a 6% raise in the fraction of white students with the Gifted and Talented Status and an over 30% reduction in the fraction of non-white students with the same status at local TPS. The net effect is a 6% average reduction in the school share of Gifted and Talented students. My results are consistent with TPS trying to increase the perceived quality of education for white students by both making regular classrooms and gifted programs more racially homogeneous, and spending more money per Gifted and Talented student. Overall, I find that charter entry increases test score dispersion within local TPS. Specifically, the average gap between white and non-white Math achievement decreases by almost 10% of the pre-entry average.

This paper contributes to show that segregation is a complex phenomenon that can increase both across schools, via institutional interventions or households' choices (Monarrez et al., 2022), and within schools, via strategic classroom formation. Neglecting the latter channel will lead to an incomplete picture of the consequences of school competition on the allocation of educational resources across students. In a policy perspective, this work also paves the way for analyzing student allocation and segregation across schools under a counterfactual scenario where gifted programs and other forms of within-school tracking are banned. This goal is particularly timely, as similar interventions are evaluated in large public school systems such as New York City as equalizing devices (Lee and Siemaszko, 2021). This type of analysis will require estimating a structural model where households have preferences over the level of racial segregation across sections, and TPS form classrooms strategically to maximize enrollment. This behavior is not only a potential source of inequality, but also introduces an inefficiency, as long as the goal of student retention moves TPS away from increasing productivity (vertical quality) and toward raising classroom segregation (horizontal quality)[26]. A key question that the structural model will allow me to answer is, whether TPS set classroom segregation strategically in response to an increase in competition whoever the marginal student is, or only when the marginal student is white. This is relevant in a policy perspective: promoting the entry of charter schools that cater to non-white students will help reduce TPS classroom segregation in the former scenario, but not in the latter.

[26]This trade-off is analogue to that between effort and reputation in MacLeod and Urquiola (2015).

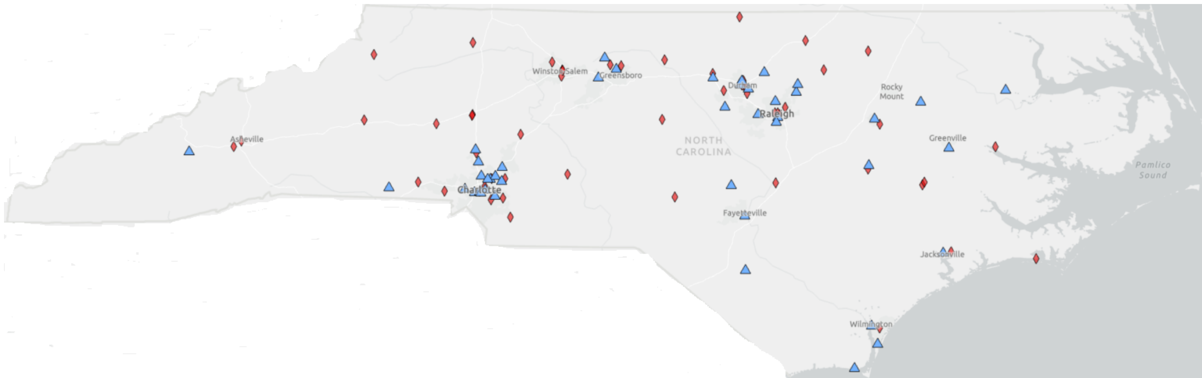
Figures

Figure 1: Number of Charter Schools in North Carolina by Year



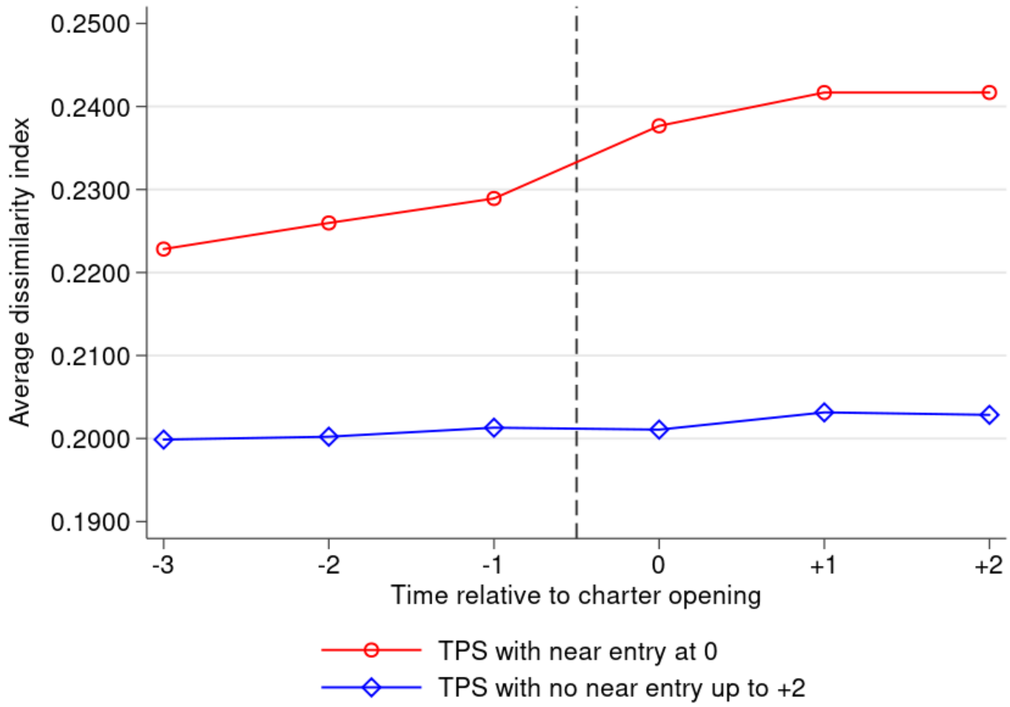
Notes: This figure displays the number of regular charter schools by year in North Carolina from 1996-97 to 2017-2018. The vertical dashed line represents the lifting of the 100 charter school cap enacted for the 2012-13 school year.

Figure 2: Locations of Elementary Charter School Openings: First and Second Wave



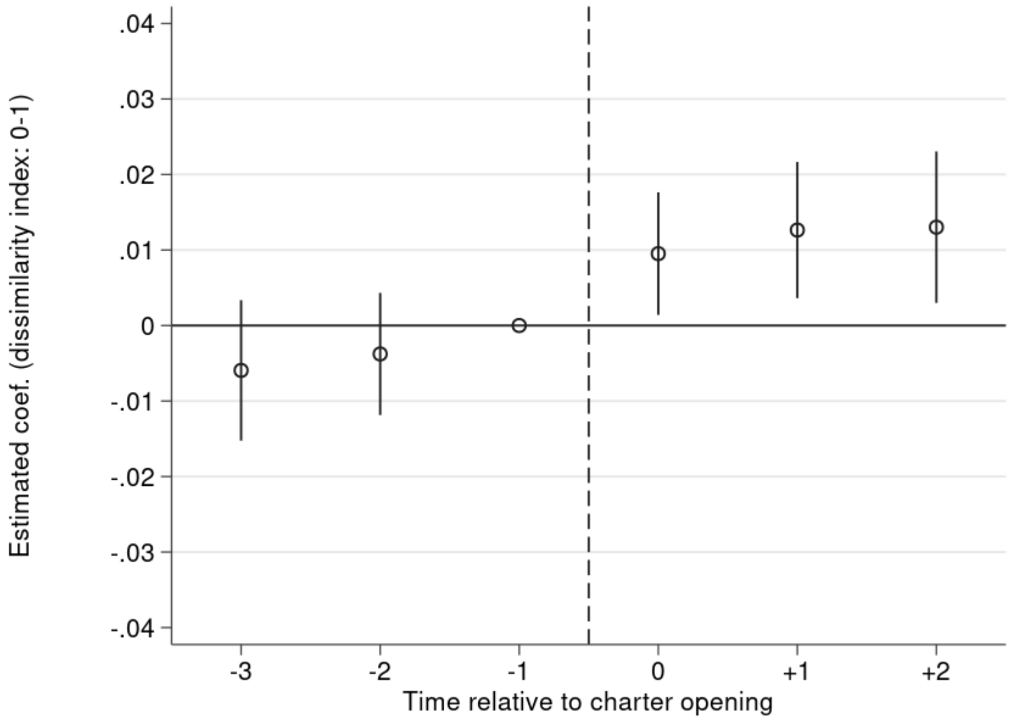
Notes: Red diamonds are for first-wave openings (1997-2005). Blue triangles are for second-wave openings (2012-2015).

Figure 3: Trends in Dissimilarity Index Over Time by Treated and Control TPS



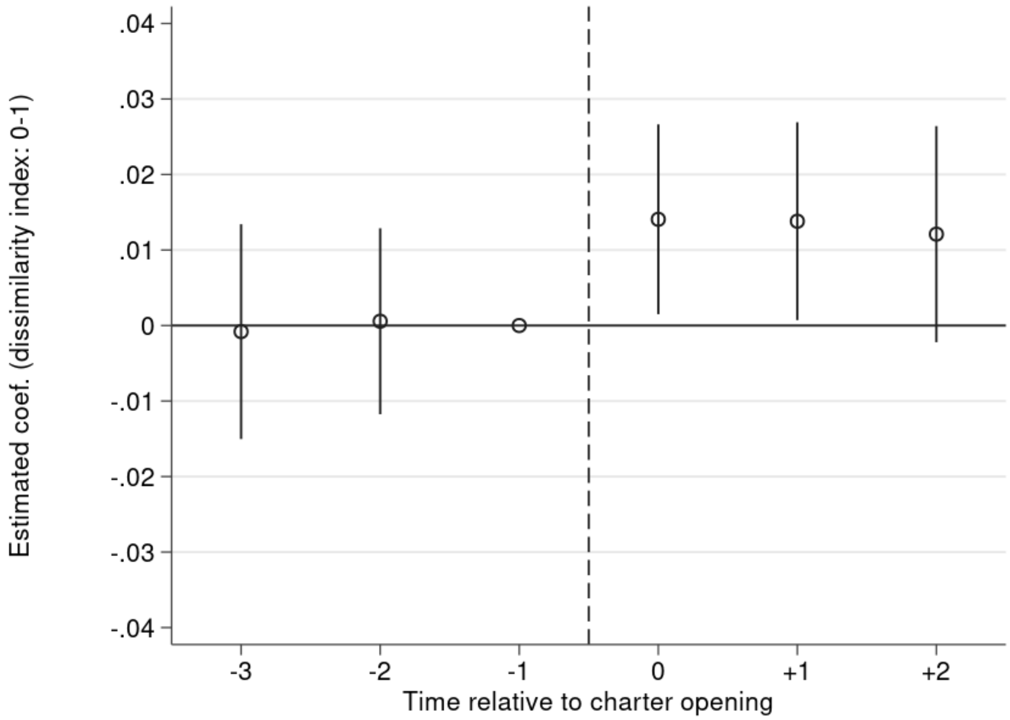
Notes: This figure shows raw averages of the dissimilarity index by year at “treated” and “control” TPS. Treated TPS face a charter entry within 5 miles at 0. Control TPS face no charter entry within 5 miles up to +2. The vertical dashed line separates the relative time periods before opening from the relative time periods after opening.

Figure 4: Event Study Estimates



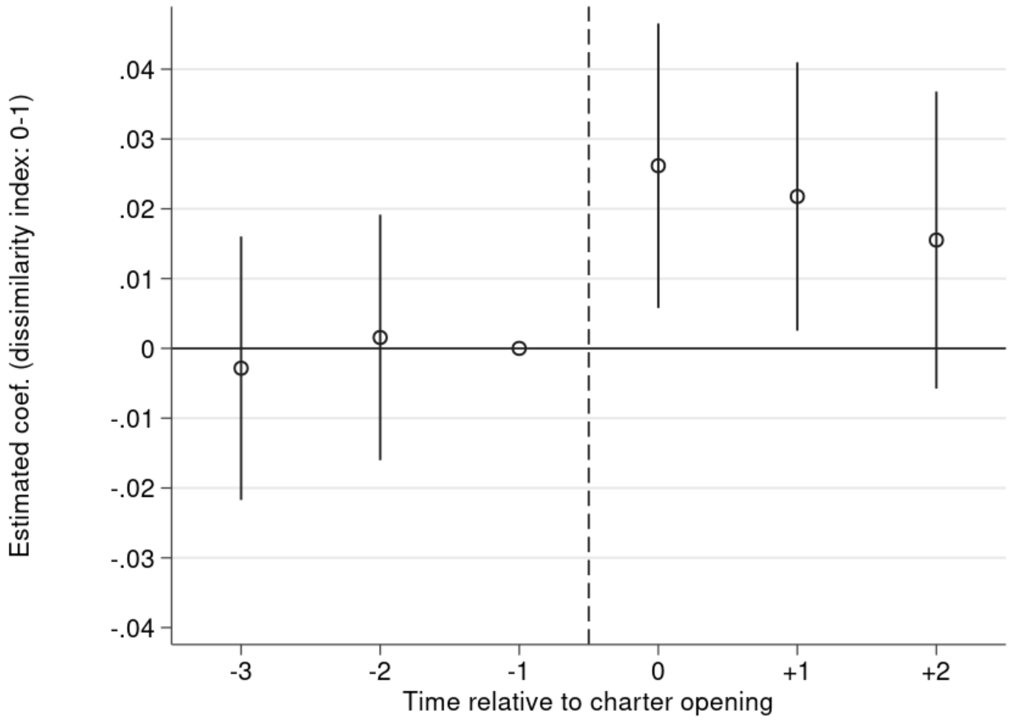
Notes: This figure displays the estimates obtained from estimating equation 1, where the outcome variable is the school-by-year dissimilarity index for Math courses, grades 1 to 5. The events are 97 elementary charter openings occurred between 1997 and 2005, and between 2012 and 2015. Zero is the normalized year of opening for entry cohorts 1997 to 2002 and 2012, while it is the normalized year of opening announcement (i.e. the year before actual opening) for the other entry cohorts. The panel is balanced. Standard errors are clustered at the school-by-entry-cohort level.

Figure 5: Robustness: Event Study Estimates with Pre-Announced Entries



Notes: This figure displays the estimates obtained from estimating equation 1, where the outcome variable is the school-by-year dissimilarity index for Math courses, grades 1 to 5. The events are 43 elementary charter openings occurred between 2003 and 2005, and between 2013 and 2015. Zero is the normalized year of opening announcement (i.e. the year before actual opening). The panel is balanced. Standard errors are clustered at the school-by-entry-cohort level.

Figure 6: Robustness: Event Study Estimates within Promised Grades



Notes: This figure displays the estimates obtained from estimating equation 1, where the outcome variable is the school-by-year dissimilarity index for Math courses. The events are the elementary charter openings that promise to add some grades one year after opening or later. In my sample, one school commits to add grade 1, two commit to grade 2, six to grade 3, eleven to grade 4, and seventeen to grade 5. For each entry, the corresponding TPS dissimilarity index is computed within the promised grades only. Zero is the normalized year of opening for entry cohorts 1997 to 2002 and 2012, while it is the normalized year of opening announcement (i.e. the year before actual opening) for the other entry cohorts. The panel is balanced. Standard errors are clustered at the school-by-grade-by-entry-cohort level.

Tables

Table 1: Descriptive Statistics

	(1) Full Sample	(2) First Wave	(3) Second Wave
N. Observations	23,622	18,096	5,562
Dissimilarity Index	0.21 (0.12)	0.20 (0.13)	0.22 (0.11)
School Size	588.43 (196.35)	560.37 (195.30)	552.06 (199.62)
School White Share	58.19 (27.26)	60.76 (25.93)	49.78 (29.71)
School White Share At Treated TPS	39.06 (28.66)	47.06 (27.53)	28.98 (26.30)
N. TPS	639	565	362
N. TPS With Near Opening	335	299	160
Median N. Years With Near Opening	1	1	2
Average N. Years With Near Opening	2.06	2	2.68
N. Charter Entries	97	54	43
School White Share Upon Opening	49.08 (34.08)	47.22 (36.55)	51.40 (30.97)

Notes: These summary statistics refer to the sample used to estimate equation (1) and are based on NCERDC and Common Core Data.

Table 2: Event Study Estimates

VARIABLES	(1) All entries (Figure 4)	(2) Pre-announced entries only (Figure 5)	(3) Promised grades only (Figure 6)
(time to event = -3)#treatment	-0.006	-0.001	-0.003
	(0.005)	(0.007)	(0.010)
(time to event = -2)#treatment	-0.004	0.001	0.002
	(0.004)	(0.006)	(0.009)
(time to event = 0)#treatment	**0.010****	**0.014****	**0.026****
	(0.004)	**(0.006)**	**(0.010)**
(time to event = 1)#treatment	0.013***	0.014**	0.022**
	(0.005)	(0.007)	(0.010)
(time to event = 2)#treatment	0.013**	0.012*	0.016
	(0.005)	(0.007)	(0.011)
School size	Y	Y	Y
School white share	Y	Y	Y
School-by-cohort FE	Y	Y	Y
Year-by-cohort FE	Y	Y	Y
Observations	23,622	9,942	18,342
R-squared	0.790	0.762	0.559
Mean dependent variable at -1	0.205	0.210	0.186

Notes: This table reports point estimates for equation 1 in the paper. Column 1 uses all entries. Column 2 relies on pre-announced entries only. Column 3 restricts the analysis to grades that the entrant charter schools promise they would start to offer in their second year of operation or later. See Section A.4 in the Appendix for details. The panel is balanced. Standard errors are clustered at the school-by-entry-cohort level in column 1 and column 2, and at the school-by-grade-by-entry-cohort level in column 3. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Difference-in-Differences Estimates by Entry Wave

VARIABLES	(1) All entries	(2) 1997-2005 entries	(3) 2012-2015 entries
DiD coef. (post#treatment)	0.015***	0.017***	0.012**
	(0.004)	(0.005)	(0.005)
School size	Y	Y	Y
School white share	Y	Y	Y
School-by-cohort FE	Y	Y	Y
Year-by-cohort FE	Y	Y	Y
Observations	23,622	18,096	5,526
R-squared	0.790	0.802	0.731
Mean dependent variable pre-entry	0.204	0.198	0.224

Notes: This table reports estimates of equation 4 in the paper, separately by entry wave. The panel is balanced. Standard errors are clustered at the school-by-entry-cohort level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Difference-in-Differences Estimates, Ability Segregation

VARIABLES	(1) Actual	(2) Simulated
DiD Coef. (post#treatment)	-0.011	-0.004
	(0.008)	(0.004)
School size	Y	Y
School white share	Y	Y
School-by-cohort FE	Y	Y
Year-by-cohort FE	Y	Y
Observations	1,548	1,554
R-squared	0.651	0.650
Mean dependent variable pre-entry	0.075	0.054

Notes: This table displays the estimates obtained from equation 4 in the paper using the 43 charter openings occurred between 2012 and 2015. The dependent variable in the column (1) is the actual index of ability segregation. The dependent variable in column (2) is the average of five simulated indices obtained from five different random allocations of students to sections, under the only constraint that the number of white and non-white students per section has to match the actual one. The panel is balanced. Standard errors are clustered at the school-by-entry-cohort level.*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5: Difference-in-Differences Estimates: Mechanisms and Test Score Inequality

VARIABLES	(1) VA W	(2) VA NW	(3) CS W	(4) CS NW	(5) AIG W	(6) AIG NW	(7) P90- -P10	(8) Racial TS gap
DiD coef.	-0.005	-0.003	0.187	0.208	0.010*	-0.018***	0.057**	0.050***
	(0.006)	(0.006)	(0.253)	(0.241)	(0.006)	(0.003)	(0.026)	(0.018)
Size	Y	Y	Y	Y	Y	Y	Y	Y
% White	Y	Y	Y	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Observations	5,724	5,724	4,902	4,902	5,892	5,892	3,588	3,588
R-squared	0.733	0.749	0.610	0.624	0.804	0.620	0.454	0.678
Mean dep. var.	0.020	0.016	25.361	25.437	0.160	0.058	2.328	0.491

Notes: This table displays the estimates obtained from equation 4 in the paper using the 43 charter openings occurred between 2012 and 2015. The dependent variable in columns (1) and (2) is teacher value added (see Section A.5 for estimation details). The dependent variable in columns (3) and (4) is average section size. The dependent variable in columns (5) and (6) is the fraction of Gifted and Talented students. Odd (even) numbered columns display the estimated effect on white (non-white) students. The dependent variable in column (7) is the difference between 90th and 10th percentiles of the test score distribution calculated within grade and then averaged across grades, within school. The dependent variable in column (8) is the difference between average white and non-white test scores calculated within grade and then averaged across grades, within school. The panel is balanced. Standard errors are clustered at the school-by-entry-cohort level.*** p<0.01, ** p<0.05, * p<0.1.

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A Data Appendix

A.1 Identifying Charter Openings

My events are 97 openings of regular-type elementary charter schools occurred in North Carolina within the time periods 1997-2005 and 2012-2015.

I use the Common Core Data (CCD) to build a panel data set of the charter schools operating in North Carolina between the school years 1997-1998 and 2017-2018.

I keep observations (school-year) with either of the following statuses: "School was operational at the time of the last report and is currently operational" or "School has been opened since the time of the last report".

I then drop schools whose entry coordinates are not available, i.e. schools for which the location coordinates are not available for the year of opening. Specifically, I drop: (i) 7 charter schools that opened and closed before the school year 2000-2001, the earliest year for which coordinates are available in the CCD; (ii) 27 charter schools whose entry coordinates are not reported. For schools of type (i) I have no coordinates at all, because they opened and closed before the CCD started to include location information. For schools of type (ii) I do have some locations, but lagged for up to seven years relative to the year of opening. I exclude these schools as they may have had no fixed location for their first few years of operation, making the concept of "opening location" hard to think through. Note that, since the Common Core Data has no school coordinates at all for school years 1997-1998 to 1999-2000, I systematically use 2000-2001 coordinates for entries happened before 2000. The 27 schools in category (ii) have missing coordinates in spite of this replacement. My replacement choice subsumes the risk that the charter schools in my sample change location within three years of opening: if at this step I keep the 9 (out of 27) entries with a delay in reporting coordinates of no longer than three years, my main result is unaltered. The main result also remains virtually identical if I drop 1997, 1998, and 1999 openings for which entry coordinates are extracted from 2000 data. I also drop 3 charter schools (1 than opened in 2012, 2 that opened in 2013) that have no TPS within 5 miles (see Section A.2).

Next, for each school, I only keep the earliest observation. At this point all my observations report the following status: "School has been opened since the time of the last report".

Finally, I keep charter schools that in the year of opening are classified as regular (neither special education, nor vocational, nor alternative) and elementary, according to the CCD definition (lowest grade between pre-school and grade 3; highest grade between pre-school and grade 8).

I am left with 121 entries of regular-type elementary charter schools that opened in North Carolina between 1997 and 2017 and have valid entry coordinates in the Common Core Data. My events are the 97 of these 121 openings that occurred between 1997 and 2005, or between 2012 and 2015. The first wave starts in 1997, right upon the approval of the North Carolina Charter School Act. I set the end date of the first entry wave at 2005 because that is when the 100-cap to the number of charter schools allowed in North Carolina first becomes virtually binding, with 99 operating charter institutions. The second wave starts in 2012 with the cap lift. I set the end date of the second entry wave at 2015 because I can measure my dependent variable up to 2017, and I am interested in the treatment effect of charter entry for up to 2 years since the time of opening. I exploit the location information of the remaining 24 schools to define clean control groups, following the procedure suggested by Cengiz et al. (2019) and described in Section 3.

A.2 Identification of Treated and Control TPS

I start by creating a list of regular elementary TPS operating in North Carolina within the time window under analysis. Specifically, I keep the schools that the Common Core Data in either of the school years 1997-1998 and 2007-2008 define as: (i) regular (neither special education, nor vocational, nor alternative); (ii) neither charter nor magnet; (iii) located in North Carolina; (iv) operational; (v) elementary, according to the CCD definition (lowest grade between pre-school and grade 3; highest grade between pre-school and grade 8); (vi) with non-missing, valid coordinates[27]. I end up with a list of 1,250 TPS.

I then create a data set where each record corresponds to one of the 151,250 (121 times 1,250) possible pairs made of one charter school (121 in total) and one TPS (1,250 in total).

[27] I drop the 53 schools whose reported locations in 1997 and 2007 are farther apart than half a mile. For the remaining schools, I take the median coordinates.

I use this data set to compute the physical distance between any charter school and any TPS in my sample. Next, I drop all pairs with a distance above 10 miles and I create a treatment indicator variable (*treated*) equal to 1 if distance is below 5 miles, 0 if distance is between 5 and 10 miles. Then, I clean the data set to have one record per TPS-year. Specifically, if a TPS within the same year faces both relatively near entries ($treated = 1$) and distant entries ($treated = 0$), then I consider that TPS treated for the school year, i.e. I only keep records for that TPS-year with $treated = 1$. Furthermore, if a TPS in a given year experiences more than one entry within 5 miles[28], or between 5 and 10 miles, I only consider one event per TPS-year: which one I keep will not affect the results. Once my data set has one record per TPS-year, I reshape and adjust it so to have one record per TPS and the *treated* status for each year between 1997 and 2017 reported in a distinct column. For example, a certain TPS will have the variable $treated1998$ equal to 1 if the TPS faces a near entry in 1998 (less than 5 miles), while equal to 0 if the TPS faces either a distant entry in 1998 (between 5 and 10 miles), or no entry within 10 miles in 1998. I do not distinguish between the two scenarios because that is not necessary, given how I build the estimation sample: specifically, I will define a control TPS for year 1998 any school that experiences no near entry until 2000 (see Section 3 in the paper).

The result is a data set of North Carolina elementary TPS that experience at least one entry within 10 miles between 1997 and 2017. The variables in the data set indicate the treatment status of each TPS for each of the entry years under analysis. Table A.1 reports the number of observations used to estimate (1) using all entry cohorts, or each cohort separately. Net of the number of entries, later entry cohorts have fewer observations because the requirement to enter as a control TPS becomes stricter and stricter as I move from 1997 to 2015. In other terms, while control TPS in the 1997 data set are all TPS that experienced no near entry (i.e., entry closer than 5 miles) between 1997 and 1999, control TPS in the 2015 data set are all TPS that experienced no near entry between 1997 and 2017. The results are in all respects similar if I adopt a looser definition of control unit and, for each entry cohort y, where $y \geq 2012$, I define a TPS control if it faces no near entry between 2012 and $y + 2$. Results are available upon request.

[28]Of the treated TPS-year combinations, around 91% have only one near entry. The other ones have two.

Table A.1: Number of Observations by Entry Cohort

Panel A: First Wave of Openings

	All	1997	1998	1999	2000	2001	2002	2003	2004	2005
N	23,622	2,184	1,956	2,214	2,052	1,938	1,974	1,866	1,902	2,010

Panel B: Second Wave of Openings

	2012	2013	2014	2015
N	1,362	1,674	1,398	1,092

A.3 Constructing a Measure of Cross-Classroom Racial Segregation

I measure racial segregation across classrooms at North Carolina elementary TPS using Classroom and Course Membership data for the school years 1994-1995 through 2017-2018. For the years 1994 to 2012 I use the Classroom data files: for each section (or classroom), the data report how many students are enrolled, as well as their racial breakdown. For the school years 2013 to 2017, classroom data are no longer available: I then exploit individual-level Course Membership data files, which report course enrollment and ethnicity for every student enrolled.

For each year of data, I only consider courses with normal-sized sections, i.e. sections with a number of students enrolled between 5 and 40. This selection implies a reduction in the number of otherwise valid records by up to 50% and is driven by sections with one or two students enrolled. I interpret these records as corresponding to missclassified individualized learning sessions. As I apply a less costly criteria, i.e. drop sections with one or two students enrolled without eliminating the entire courses that have some, my main result is robust. Of the remaining records, I keep: (i) sections / individuals in grades 1 to 5; (ii) sections / individuals in Math or self-contained courses; (iii) courses with valid and consistent racial information; (iv) courses with two or more sections; (v) courses with some racial diversity, i.e. a strictly positive number of both white and non-white students. Table A.2 reports how many observations I drop as I apply each of these criteria.

Table A.2: Cleaning of Classroom Data

Classroom Data (1994-2012)	**Number of Observations**
Data	19,259,050
- Grades	4,503,795
- Subjects	852,036
- Missing racial info	852,036
- Section size	629,921
- One section	601,478
- No diversity	585,672
Average class size	21.04

Individual Course Membership Data (2013-2017)	**Number of Observations**
Data	87,362,035
- Grades	35,328,129
- Subjects	3,238,586
- Missing racial info	3,238,586
Collapse by classroom	195,174
- Section size	94,393
- One section	91,172
- Imperfect racial breakdown	91,172
- No diversity	89,906
Average class size	21.19

A.4 Announcements of Opening and Grade Expansion

In Section 5 I show that the increase in classroom segregation that I observe upon charter entry is not a mechanical by-product of the change in the TPS student body composition due to the charter opening itself. To do so, I exploit two features of the timing of charter openings.

The first feature involves the 46 out of 100 charter schools that opened between 2003 and 2005 or between 2013 and 2015. For these entry cohorts, the outcome of the application process was announced more than a year before the actual opening. This implies that, in the year before opening, local TPS knew that a charter school would open close by, while students would not be able to switch to or enroll in that same charter school right away. Showing that classroom segregation increases as soon as a charter opening is announced confirms that the increase genuinely captures the TPS competitive response, and is not driven by students flowing out.

Charter schools approved to open in the Fall of 2004 were selected as final candidates by May 15, 2003 and recommended to begin the preliminary planning year by August 7, 2003 (`http://www.ncpublicschools.org/charter_schools/new_school.html`, retrieved via Wayback Machine [October 2003 saving] on June 26, 2022.). Applicants approved to open in the Fall of 2005 were recommended to begin the preliminary planning year by July 1, 2004 (`http://www.ncpublicschools.org/charter_schools/new_school.html`, retrieved via Wayback Machine [March 2004 saving] on June 26, 2022.). There is no such timing information for schools applying to open in the Fall of 2003. However, the only 2003 entry that I exploit is Central Park - The Community School for Children, whose application was submitted in August 2001 and indicated the Fall of 2002 as intended opening date. As this opening seems delayed, I conjecture that near TPS may have started to respond since the Fall of 2002. As for more recent entry cohorts, applicants for the school year 2013-2014 were shortlisted in June 2012 (`https://www.dpi.nc.gov/students-families/alternative-choices/charter-schools/applications`, last accessed on June 26, 2022), while applicants for the school year 2014-2015 were shortlisted in July 2013 and granted preliminary approval in September 2013 (`http://www.ncpublicschools.org/charterschools/applications/2014-15/`, retrieved via Wayback Machine [November 2015 saving] on June 26, 2022). Applicants for the 2015-2016 school year were voted to move into the planning year in September 2014 (`http://www.ncpublicschools.org/charterschools/applications/2015-16/`, retrieved via Wayback Machine [November 2015 saving] on June 26, 2022), while the NCDPI website does not mention any earlier shortlisting. Even if there was no earlier shortlisting, TPS had enough time to alter their class rosters for the Spring term in response to the entry approval. The results are very similar if I exclude 2003 and 2015 entries from the analysis. Results are available upon request.

The second feature that I exploit is that 21 of the 97 charter schools that I study open with a certain grade configuration, but promise in their application files to start to offer other grades from the second year of operation or later. Once again, finding that classroom segregation at local TPS increases within such "promised" grades even before the charter school starts to offer them points out to a TPS competitive response that is not driven by students flowing out. Table A.3 lists charter schools by promised grade, as well as their opening years.

Table A.3: List of Charter Schools That Commit to Grade Expansion, by Grade

Grade Promised	**Schools (Opening Year)**
Grade 1	The New Dimensions (2001)
Grade 2	The New Dimensions (2001) ; Socrates Academy (2005) *Columbus Charter School (2007)*
Grade 3	Healthy Start Academy (1997) PreEminent Charter (2000) A Child's Garden School (2001) Central Park – The Community School for Children (2003) Socrates Academy (2005) *Columbus Charter School (2007)* Douglass Academy (2013)
Grade 4	Healthy Start Academy (1997) ; Children's Village Academy (1997) Washington Montessori (2000) ; A Child's Garden School (2001) Central Park – The Community School for Children (2003) Children's Community School (2004) Socrates Academy (2005) *Columbus Charter School (2007)* Corvian Community School (2012) Willow Oak Montessori (2013) Douglass Academy (2013) Reaching All Minds Academy (2014)
Grade 5	Healthy Start Academy (1997) ; Research Triangle Charter (1999) Children's Village Academy (1997) ; Washington Montessori (2000) Union Academy (2000) ; Child's Garden School (2001) Hope Elementary (2001) Central Park – The Community School for Children (2003) Children's Community School (2004) Socrates Academy (2005) *Columbus Charter School (2007)* *Wilmington Preparatory Academy (2007)* *Union Independent School (2011)* High Point College Preparatory Academy (2012) Corvian Community School (2012) Willow Oak Montessori (2013) Douglass Academy (2013) ; Reaching All Minds Academy (2014) Wayne Preparatory Academy (2014) ; Thunderbird Prep (2014)

Notes: PreEminent Charter and Central Park – The Community School for Children opened one year after the intended date, but with the initial grade configuration reported in the application.

A.5 Constructing Teacher Value Added

I estimate teacher value added for 3rd to 5th grade teachers and school years 2006-2007 through 2016-2017. I follow Rothstein (2010) very closely. First, I use student-level Masterbuild data to obtain a student-by-year data set with test scores, lagged test scores and covariates (e.g. gender; ethnicity; economic, disability, and gifted status). Next, I match students to their classrooms and Math teachers using individual-level Course Membership files, while keeping track of the number of students that each teacher teaches each year. Then, after some data cleaning, for each teacher j I drop observations for year y if the school that employs teacher i faces a near entry between $y-2$ and y. I do this to exclude from my value added estimates any immediate effort response to competition that occurs within teacher. The final step is estimating teacher value added. I start from estimating

$$A^*_{ijy} = \beta X_{ijy} + \alpha_{jt} + \epsilon_{ijy}, \quad i = 1, 2, ..., n_{jy} \tag{7}$$

where i denotes the student; A is the math test score; X includes own and classroom demographics (ethnicity; gender; socio-economic, English learner, disability, and gifted status) and lagged test scores; α_{jt} is a teacher-by-year fixed effect; $\epsilon_{ijy} \sim \mathcal{N}(0, \sigma^2_\epsilon)$ is the error term; n_{jy} is the number of students taught by teacher j in year y. The regression includes also controls for class and cohort size and grade-by-year fixed effects. Let $A_{ijy} = A^*_{ijy} - \beta X_{ijy}$, I obtain estimates for teacher value added using the fixed effect (MLE) estimator

$$\bar{A}_j \equiv \frac{\sum_y n_{jy}\bar{A}_{jt}}{\sum_y n_{jy}} \sim \mathcal{N}\big(\alpha_j, \frac{\sigma^2_\epsilon}{\sum_y n_{jy}}\big) \tag{8}$$

with $\bar{A}_{jy} = \frac{1}{n_{jy}}\sum_{i=1}^{n_{jy}} A_{ijy}$. I then make the parametric assumption that $\sigma_j \sim \mathcal{N}(0, \sigma^2_\alpha)$. This leads to the Parametric Empirical Bayes Estimator for teacher value added

$$\hat{\alpha}_j = \bar{A}_j \frac{\sigma^2_\alpha}{\sigma^2_\alpha + \sigma^2_\epsilon / \sum_y n_{jy}} \tag{9}$$

I estimate σ^2_α and σ^2_ϵ off the estimated variability in A_{ijy} and $\bar{A}_j$.

A.6 Details on Sample Construction for Mechanisms Analysis

Ability segregation: I obtain third grade standardized Math test scores at the student level, along with student ethnicity for the school years 2006-2007 to 2017-2018. I drop students who have multiple test scores on record for the same year whenever such test scores disagree. I also drop students for whom no racial information is available on record. Besides, for students that take third grade multiple times, I keep the most recent performance.

I then merge these data to Math course membership data for students in grade 4 and 5. I then drop courses with: (i) more than 20% of the students with missing test scores; (ii) any sections with fewer than 5 or more than 40 students enrolled; (iii) one section only. The result is a data set where fourth and fifth graders are matched to the Math sections in which they are enrolled in grade 4 and 5, as well as to their third grade standardized Math test score and ethnicity.

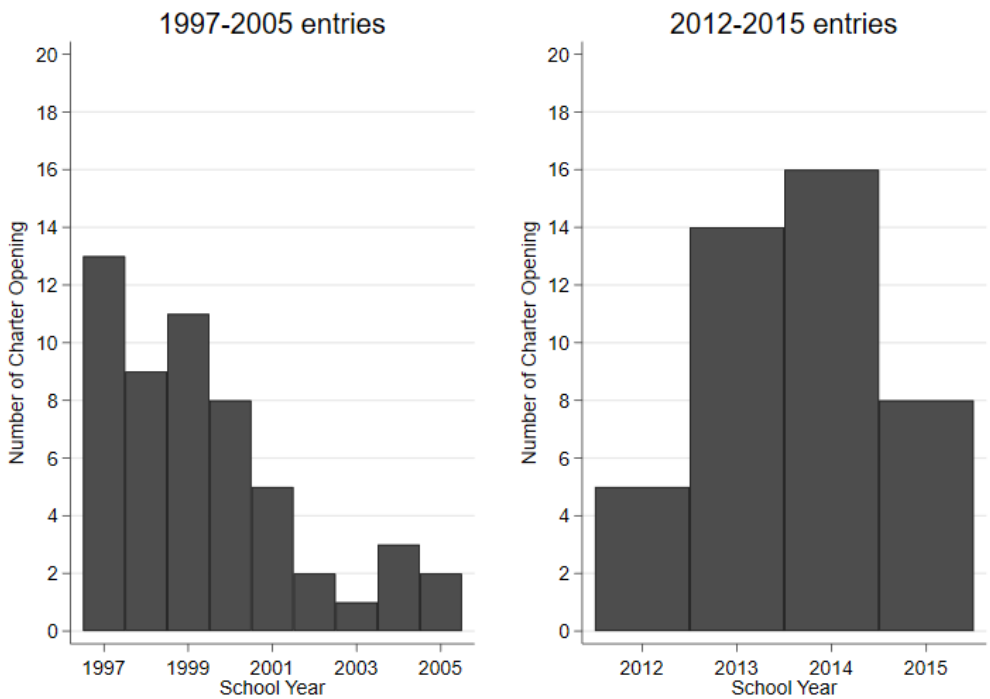
Next, I calculate third-grade test score quartiles for every course and define a set of four variables that locate each student in the corresponding quartile of the course-specific test score distribution. I then use the Stata command *rankseg* to compute the ordinal entropy index proposed by Reardon (2009) for every Math course. The output is a measure of segregation within courses, across sections, by ability as it is measured by third grade Math achievement.

Class size: I use student-level course membership files for the school years 2006-2007 to 2017-2018 to compute the average section size that each first to fifth grader is exposed to within Math courses. I only exclude sections with fewer than 5 or more than 40 students enrolled. I also drop students enrolled in more than four courses per year (1.22% of the observations). I then restrict the analysis to schools that belong to the baseline sample used in Table 3, column 3.

Test score inequality: I use end of grade test scores for grades 3 to 5, school years 1996-1997 to 2017-2018. I drop students with missing test scores, as well as students with multiple records per year and conflicting test scores. I also drop schools with fewer than 10 students per grade and racial group (white vs non-white). Then, I standardize test scores within year, computing means and standard deviations off the full samples, and compute: (i) within-grade test score variance; (ii) the within-grade difference between 90th and 10th percentiles of the test score distribution; (iii) the average difference between white and non-white test scores within grade. I then average (i), (ii) and (iii) across grades, within school. I restrict the analysis to schools that belong to the baseline sample used in Table 3, column 3.

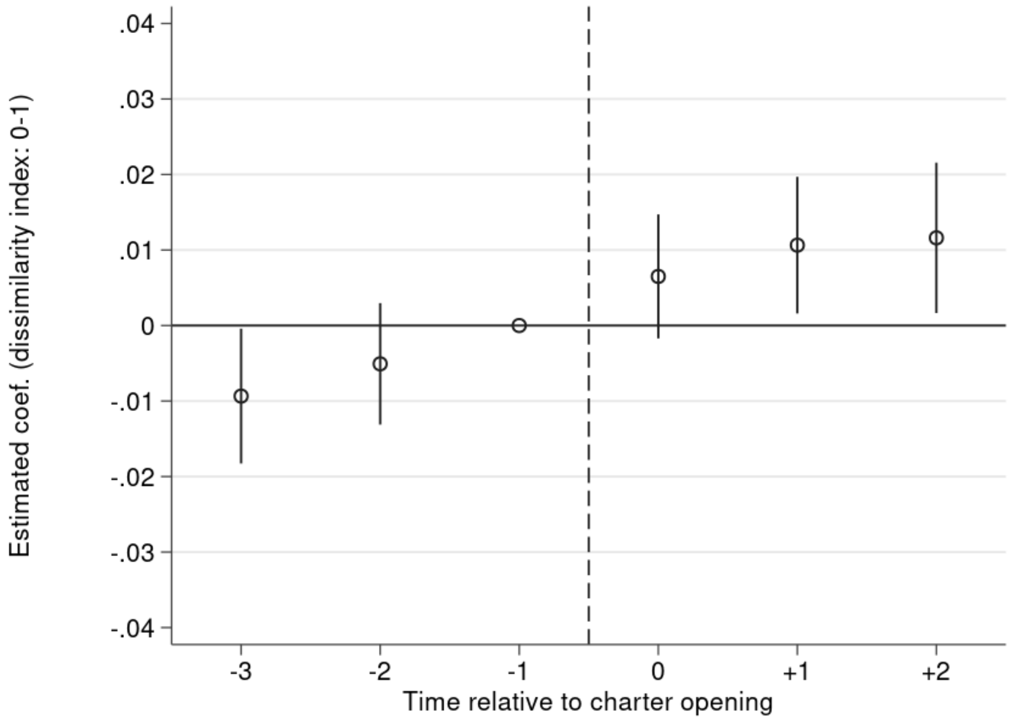
B Figures

Figure B.1: Distribution of Charter Openings Over Time, First and Second Wave



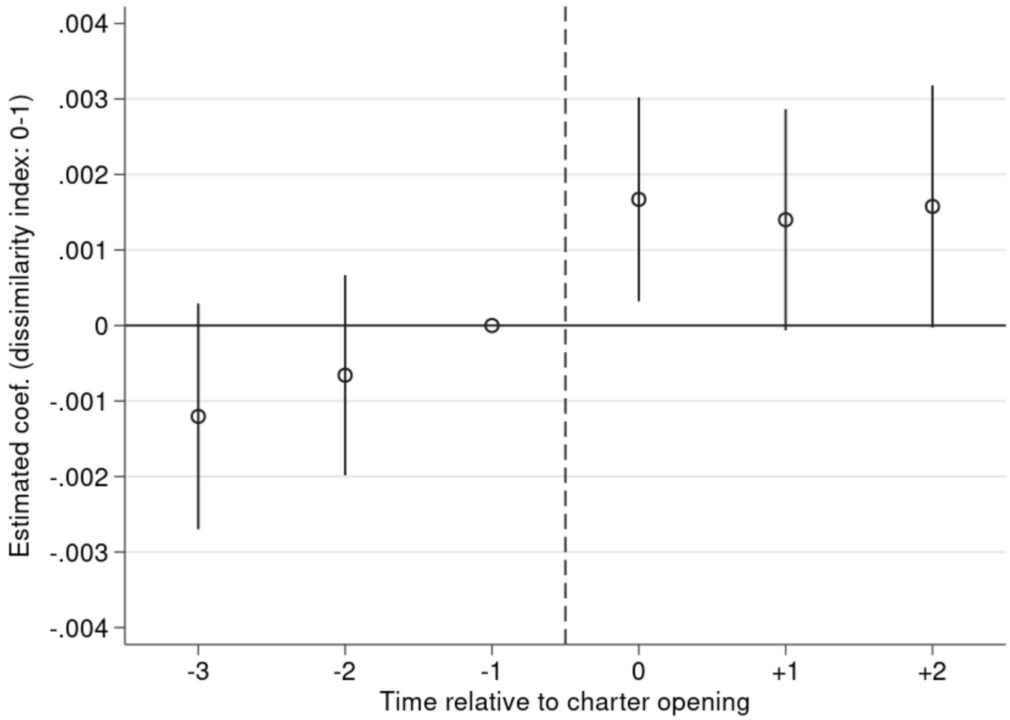
Notes: This figure displays the over time distribution of the 97 elementary charter openings included in my sample. The left panel is for the first wave, while the right panel is for the second wave.

Figure B.2: Event Study Estimates: Reading and English Language Arts



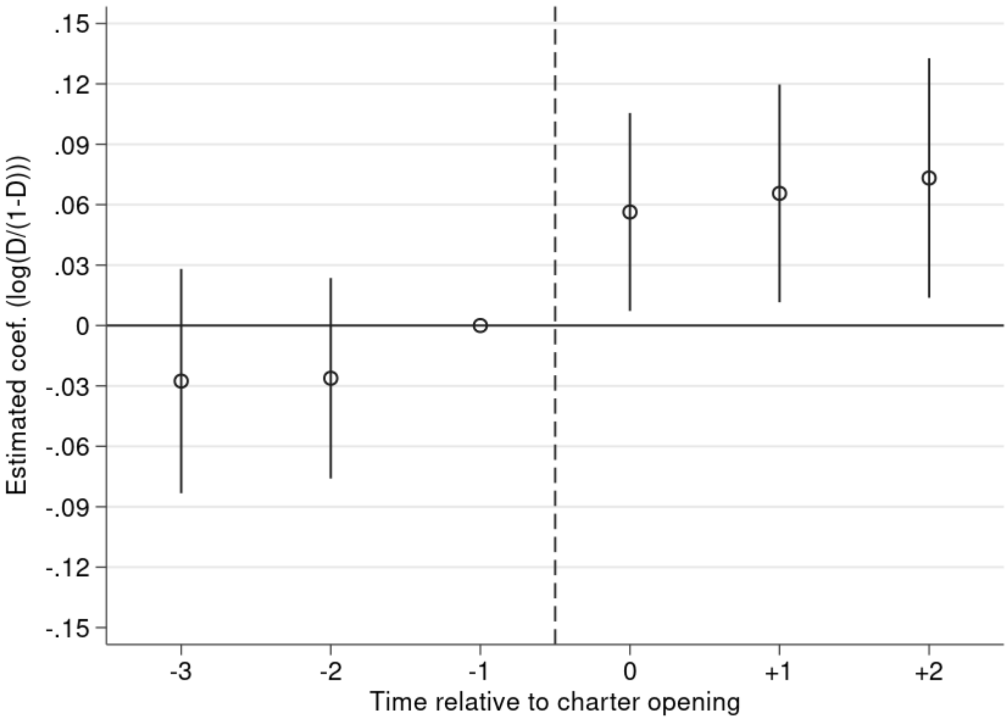
Notes: This figure displays the estimates obtained from estimating equation 1, where the outcome variable is the school-by-year dissimilarity index for Reading and English Language Arts courses, grades 1 to 5. The events are 97 elementary charter openings occurred between 1997 and 2005, and between 2012 and 2015. Zero is the normalized year of opening for entry cohorts 1997 to 2002 and 2012, while it is the normalized year of opening announcement (i.e. the year before actual opening) for the other entry cohorts. The panel is balanced. Standard errors are clustered at the school-by-entry-cohort level.

Figure B.3: Robustness: Event Study Estimates with Continuous Distance



Notes: This figure displays the results obtained from estimating a modified version of equation (1), where the outcome variable is the school-by-year dissimilarity index for Math courses, grades 1 to 5. I substitute the binary definition of treatment, based on the 5-mile distance cutoff, with a continuous measure of treatment, equal to 10, the maximum distance in my sample, minus actual distance. This measure tends to 10 for very close TPS, to 0 for relatively distant TPS, while I set it equal to 0 for control TPS, i.e. those that experience no entry within 10 miles up to the entry year under analysis. The events are 97 elementary charter openings occurred between 1997 and 2005, and between 2012 and 2015. Zero is the normalized year of opening for entry cohorts 1997 to 2002 and 2012, while it is the normalized year of opening announcement (i.e. the year before actual opening) for the other entry cohorts. The panel is balanced. Standard errors are clustered at the school-by-entry-cohort level.

Figure B.4: Robustness: Event Study Estimates with Transformed Dependent Variable

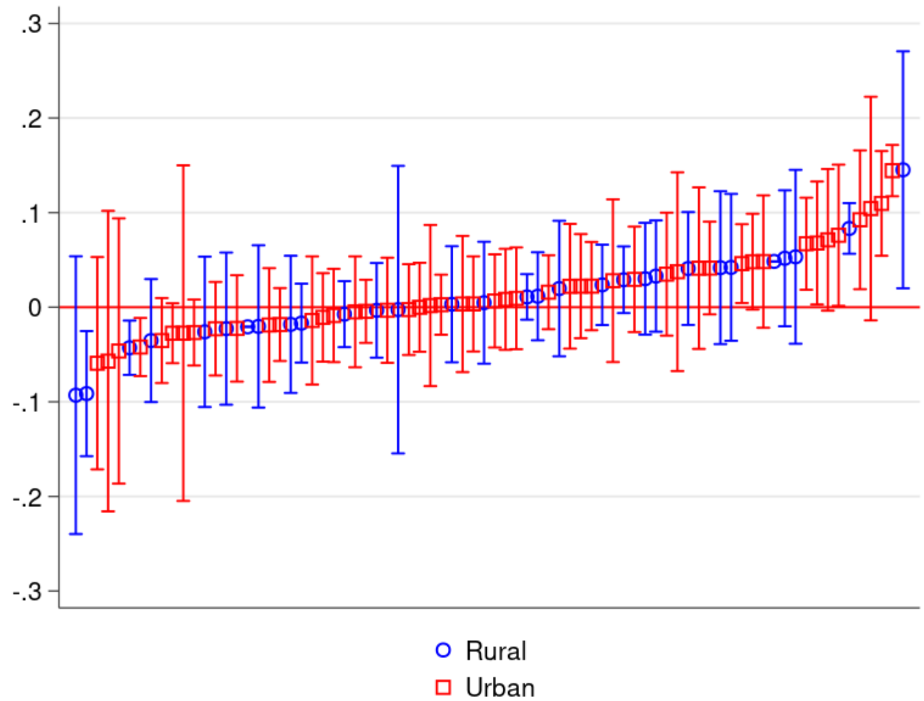


Notes: This figure displays the estimates obtained from estimating equation 1, where the outcome variable is the school-by-year dissimilarity index for Math courses, grades 1 to 5, transformed according to equation 3. The events are 97 elementary charter openings occurred between 1997 and 2005, and between 2012 and 2015. Zero is the normalized year of opening for entry cohorts 1997 to 2002 and 2012, while it is the normalized year of opening announcement (i.e. the year before actual opening) for the other entry cohorts. The panel is balanced. Standard errors are clustered at the school-by-entry-cohort level.

Figure B.5: Robustness: Event Study Estimates with Dissimilarity Index Corrected for Small Unit Bias

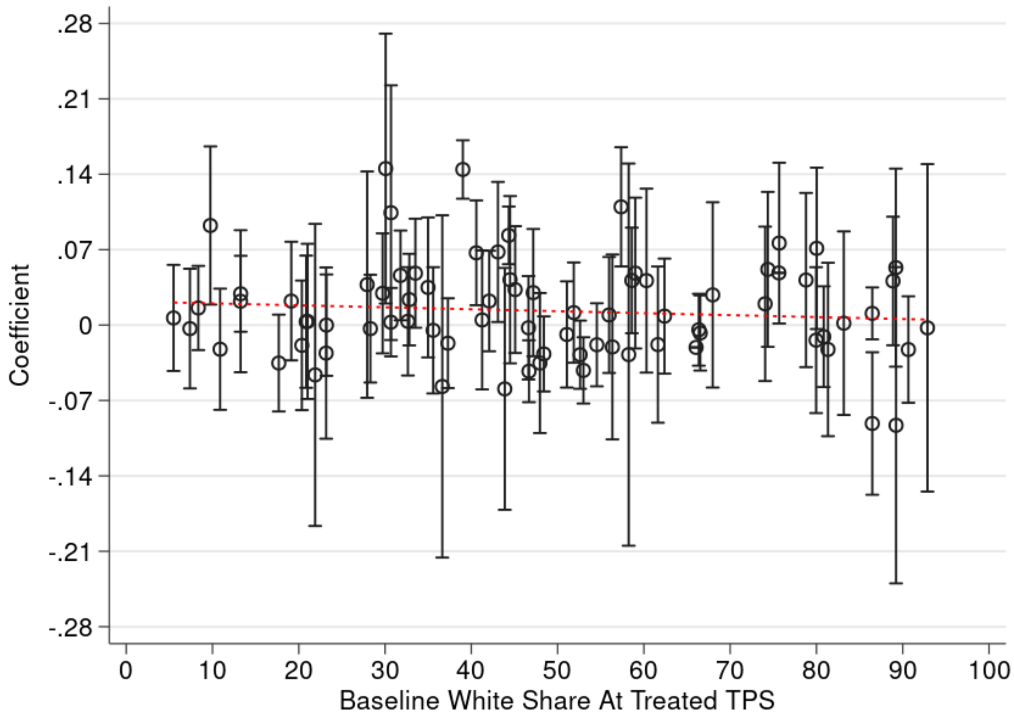
Notes: This figure displays the estimates obtained from estimating equation 1, where the outcome variable is the school-by-year dissimilarity index for Math courses, grades 1 to 5, corrected for the small unit bias issue à la Carrington and Troske (1997). The events are 97 elementary charter openings occurred between 1997 and 2005, and between 2012 and 2015. Zero is the normalized year of opening for entry cohorts 1997 to 2002 and 2012, while it is the normalized year of opening announcement (i.e. the year before actual opening) for the other entry cohorts. The panel is balanced. Standard errors are clustered at the school-by-entry-cohort level.

Figure B.6: Plot of Event-Specific Diff-in-Diff Coefficients



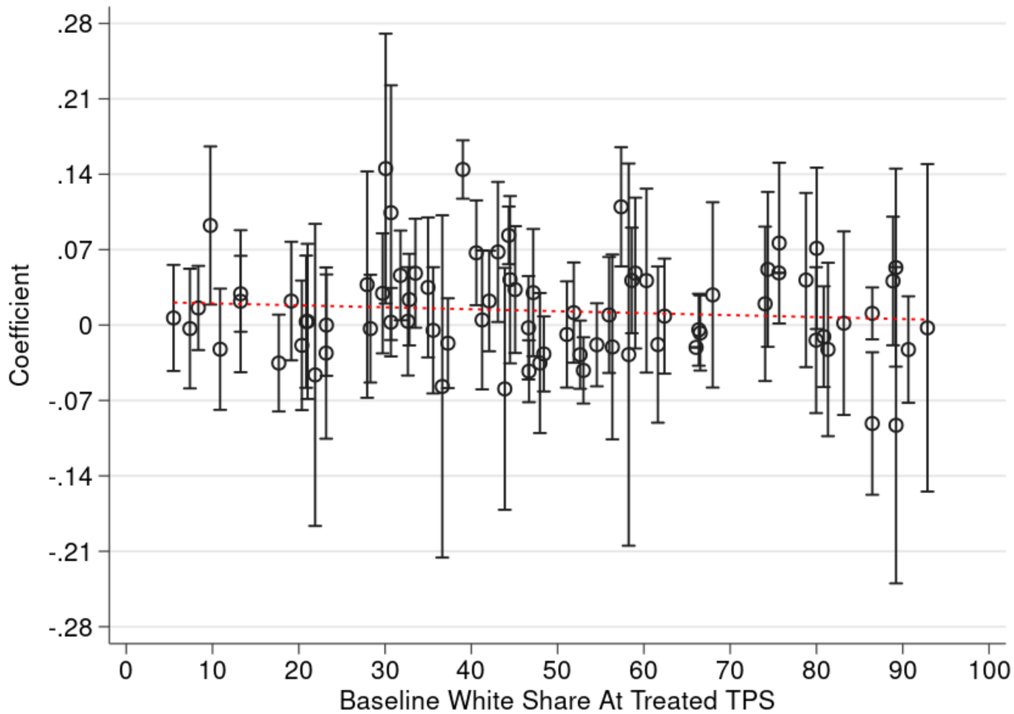
Notes: This figure plots the coefficients obtained from estimating equation 4 separately for each charter opening. I can only obtain an estimate for 78 of the 97 entries, for which the sample size is large enough. Charter openings are classified as urban if they occur in an area with a population density of 0 - 1,000 people per square mile, while they are classified as rural if located in any area with higher density. Density is calculated as of 2021 and obtained from the ArcGis map available here `https://nyuds.maps.arcgis.com/home/item.html?id=a8407298de7e48078a2bc9cdd76c79af` (last access: July 26, 2022).

Figure B.7: Correlation between Coefficients and Baseline White Share



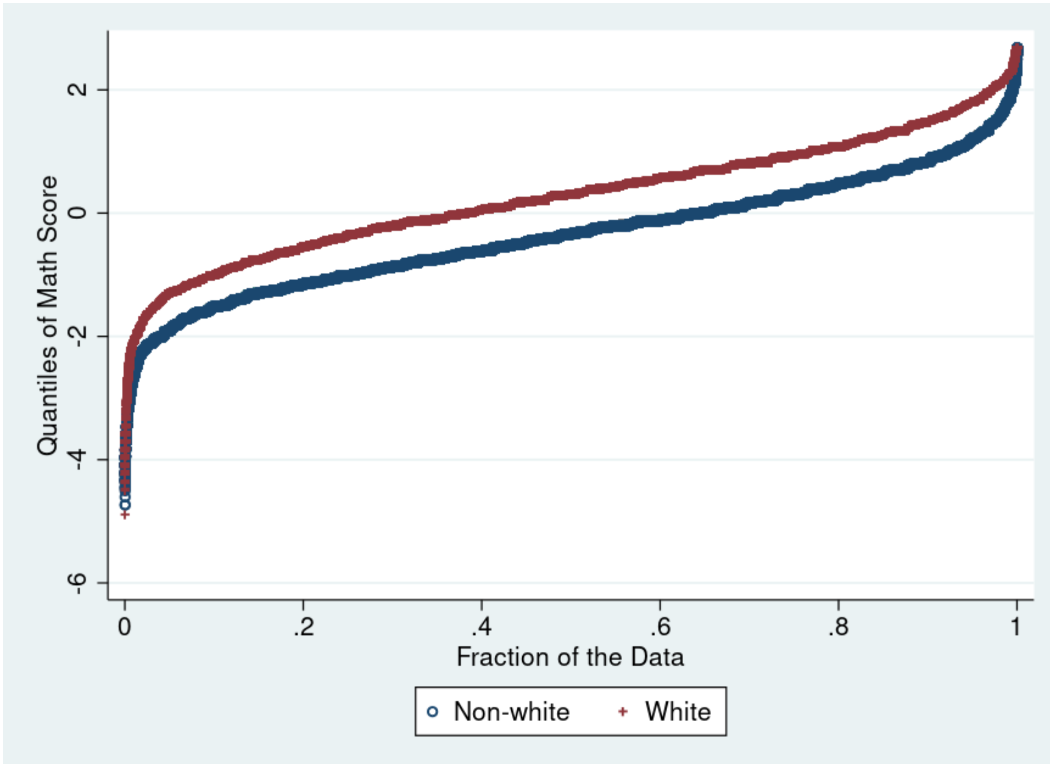
Notes: This figure plots the coefficients obtained from estimating equation 4 separately for each charter opening against the baseline average share of white students at local TPS. I can only obtain an estimate for 78 of the 97 entries, for which the sample size is large enough.

Figure B.8: Correlation between Coefficients and Baseline Classroom Segregation



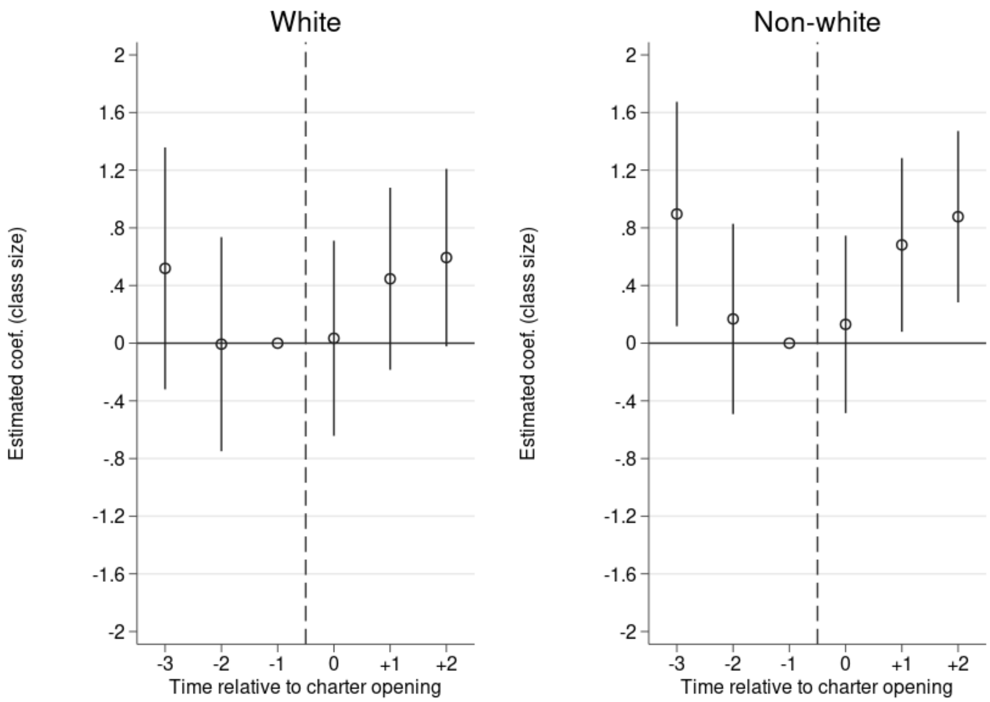
Notes: This figure plots the coefficients obtained from estimating equation 4 separately for each charter opening against the baseline average classroom segregation (i.e. racial dissimilarity index) at local TPS. I can only obtain an estimate for 78 of the 97 entries, for which the sample size is large enough.

Figure B.9: Plot of Test Score Distribution for White and Non-White Students



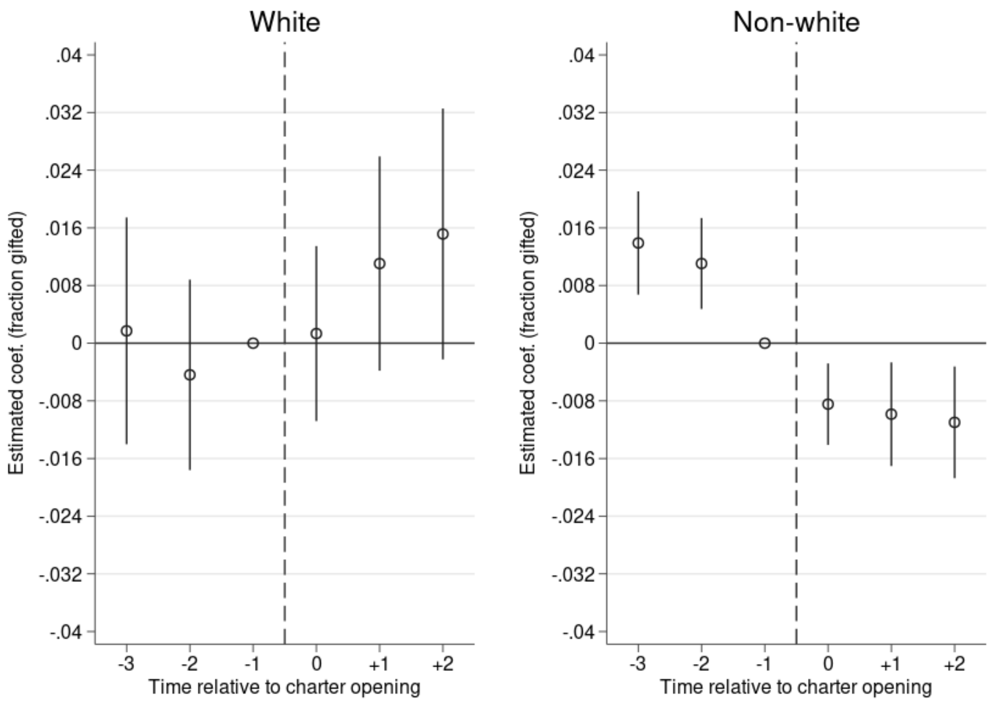
Notes: this figure plots the percentiles of the distribution of standardized Math test scores in third grade for white and non-white students between 2007 and 2018.

Figure B.10: Event Study Estimates: Class Size



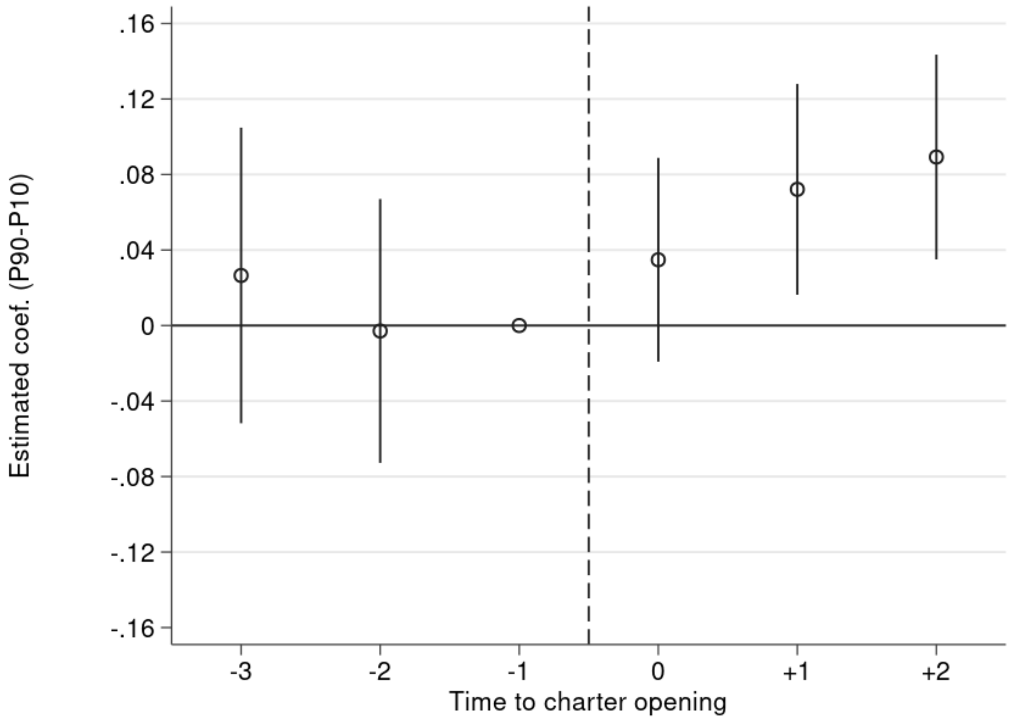
Notes: this figure displays the estimates obtained from estimating equation 1, where the outcome variable is average class size experienced by white (left panel) and non-white (right panel) students at the school-by-year level. The events are 43 elementary charter openings occurred between 2012 and 2015. Zero is the normalized year of opening for entry cohort 2012, while it is the normalized year of opening announcement (i.e. the year before actual opening) for the other entry cohorts. The panel is balanced. Standard errors are clustered at the school-by-entry-cohort level.

Figure B.11: Event Study Estimates: Share of Gifted and Talented



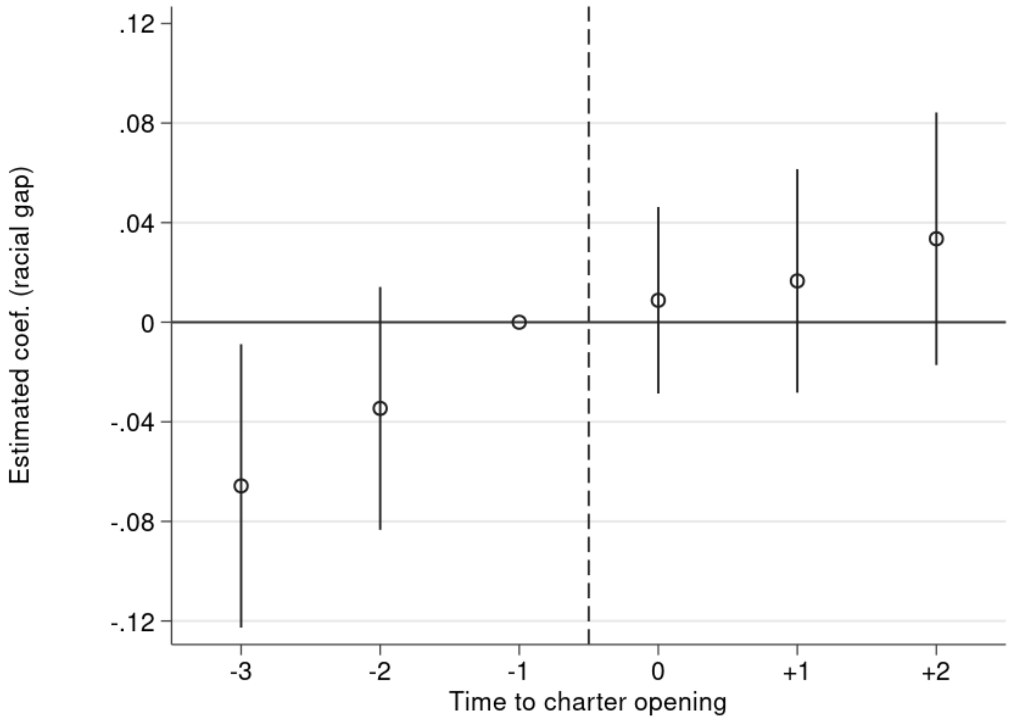
Notes: this figure displays the estimates obtained from estimating equation 1, where the outcome variable is the share of white (left panel) and non-white (right panel) students with the Gifted and Talented status at the school-by-year level. The events are 43 elementary charter openings occurred between 2012 and 2015. Zero is the normalized year of opening for entry cohort 2012, while it is the normalized year of opening announcement (i.e. the year before actual opening) for the other entry cohorts. The panel is balanced. Standard errors are clustered at the school-by-entry-cohort level.

Figure B.12: Event Study Estimates: Test Score Dispersion



Notes: this figure displays the estimates obtained from estimating equation 1, where the outcome variable is the within-grade difference between 90th and 10th percentile of the Math end-of-grade test score distribution, then averaged across grades within school. The events are 43 elementary charter openings occurred between 2012 and 2015. Zero is the normalized year of opening for entry cohort 2012, while it is the normalized year of opening announcement (i.e. the year before actual opening) for the other entry cohorts. The panel is balanced. Standard errors are clustered at the school-by-entry-cohort level.

Figure B.13: Event Study Estimates: Racial Test Score Gap



Notes: this figure displays the estimates obtained from estimating equation 1, where the outcome variable is the within-grade difference between average white and non-white Math standardized test scores, then averaged across grades within school. The events are 43 elementary charter openings occurred between 2012 and 2015. Zero is the normalized year of opening for entry cohort 2012, while it is the normalized year of opening announcement (i.e. the year before actual opening) for the other entry cohorts. The panel is balanced. Standard errors are clustered at the school-by-entry-cohort level.