Online Appendix

The Segregative Effects of Charter Schools

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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.

From the Common Core Data records for a given school year, I start by keeping 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 I cannot credibly identify coordinates *in the year of opening*. Note that the Common Core Data began reporting school coordinates in 2000-2001. For charter schools that opened between 1997-1998 and 1999-2000, I impute their opening locations using 2000-2001 coordinates.[1] Even after this replacement, I am left with two categories of charter schools to drop; These are: (i) 7 charter schools that *opened and closed* before the school year 2000-2001 and have, therefore, no coordinates reported in the CCD at any point of their lives; (ii) 27 charter schools whose coordinates in the opening year are missing in the data. For schools of type (ii), the CCD does report coordinates *at some point of the school's life*, but the earliest location reporting in the CCD can be 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

[1] The main result is robust to dropping 1997, 1998, and 1999 openings, for which entry coordinates are extracted from 2000 data.

years of operation, making the concept of "opening location" hard to think through.[2] I also drop 3 charter schools (Water's Edge Village School, which opened in 2012, and Aristotle Preparatory Academy and Stem Ed Global Society Academy, which 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. As for the 24 schools whose opening is not used as an *event* within my empirical strategy, their location information is used to define control schools for a given treatment cohort, following the procedure described in section 3. This means that, for example, the controls in the 2015 treatment cohort dataset – the latest included in my analysis – are such that they experience no opening within 5 miles of their locations until 2017.

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

[2] My main result is robust to inclusion of the 9 schools of category (ii) whose coordinates are reported within three years of the opening.

grade between pre-school and grade 3; highest grade between pre-school and grade 8); (vi) with non-missing, valid coordinates.[3] 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). 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 transform the dataset from one at the TPS-charter level into one at the TPS-opening-year level: 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-opening-year with $treated = 1$. Furthermore, if a TPS in a given year experiences more than one entry within 5 miles[4], or between 5 and 10 miles, I only consider one event per TPS-opening-year at random – which one I keep will not affect the results. Once my data set has one record per TPS-opening-year, I reshape it so that it has one record per TPS, and the *treated* status for each year between 1997 and 2017 is reported in a distinct Column. For example, a certain TPS will have the variable *treated*1998 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

[3] 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.

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

$y+2$. The results are also robust to keeping only controls that, for a given treatment cohort, experience no near entry *within* the time window plotted in the event study, but may have experienced a near entry *before* the earliest year plotted (Cengiz et al., 2019). Results are available upon request.

Table A.1: Number of Observations by Entry Cohort

Panel A: All & First Wave of Openings

	All	1997	1998	1999	2000	2001	2002	2003	2004	2005
N	26,068	2,184	2,198	2,163	2,268	2,191	2,198	2,058	2,107	2,254

Panel B: Second Wave of Openings

	2012	2013	2014	2015
N	1,596	1,960	1,617	1,274

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: The rationale for this restriction is to ensure that my measure of classroom segregation is not artificially inflated by individualized sessions (classified with the wrong subject code) or affected by other errors in reporting. This selection implies a reduction in the number of students by 35% per year on average (based on 2013-2017 records) and is driven by sections with one or two students enrolled. I interpret these records as corresponding to missclassified individualized learning sessions (e.g., sessions for English learners, students with special needs, or gifted students) and other reporting errors. If I apply a less costly criterion to courses with small sections (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	19,359,050
- 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	85,624,542
- Grades	34,617,362
- Subjects	3,166,877
- Missing racial info	3,166,877
Collapse by classroom	195,376
- Section size	98,882
- One section	95,743
- Imperfect racial breakdown	95,743
- No diversity	94,443
Average class size	21.13

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 44 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 a full school 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) ; A 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: This Table lists the charter schools that in their application files promise to add some grades one year after opening or later. Charter schools are listed by promised grade. Opening years are in parentheses. 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 2009-2010 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 j faces a near entry between $y-4$ 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

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

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

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

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

(3)
$$\hat{\alpha}_j = \bar{A}_j \frac{\sigma^2_\alpha}{\sigma^2_\alpha + \sigma^2_\epsilon / \sum_y n_{jy}}$$

I estimate σ^2_α and σ^2_ϵ via maximum likelihood, exploiting the estimated variability in A_{ijy} and $\bar{A}_j$.

A.6 Details on Institutions and Sample Construction for Table D.8

Ability segregation: I obtain third grade standardized Math test scores at the student level, along with student ethnicity for the school years 2008-2009 to 2015-2016. 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 merge these data at the student level to Math course membership data for students in grade 4, school years 2009-2010 to 2016-2017. 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 graders are matched to both the Math sections in which they are currently enrolled, and their baseline ability (proxied by their third grade standardized math test score) and ethnicity. I do the same with fourth grade standardized Math test scores and course membership data for grade 5. The analysis is restricted to grades 4 and 5 because of data availability, as grade 3 is the earliest for which end-of-grade standardized test scores are available.

I use the two resulting data sets – one for grade 4 and the other one for grade 5 courses – to construct a "simulated" and an "actual" measure of classroom segregation by ability. I restrict the analysis to schools that belong to the baseline sample used in Table D.3, Column 3.

I measure classroom segregation by lagged achievement for a given school, grade, year, and term using the ordinal version of the information theory index proposed by Reardon (2009) to deal with the fact that achievement categories are intrinsically ordered and, hence, not interchangeable. For the purpose of computing the index, I define "ability" groups based on quartiles of the corresponding lagged achievement distribution. In the literature, Salvati (2022) reports that ability tracking is indeed present in lower grades, although to a lesser extent than in upper grades: According to figures reported by the National Assessment of Educational Progress (NAEP) in 2015, 30 (75)% of public schools group by lagged performance in 4th (8th) grade math courses.

As for the simulated index, for every school, year, grade, and term, I draw five different classroom configurations. In each of these configurations, the distribution of white and non-white students across sections matches the one observed in the data. Conditional on race, however, students with different baseline scores are assigned to sections randomly. I then compute Reardon (2009)'s index using the simulated classrooms and average across simulations and grades to obtain a simulated index of ability tracking for each school and year.

Test score inequality: I use end-of-grade test scores for grades 3 to 5, school years 2009-2010 to 2016-2017. I drop students with missing test scores. For students with multiple records per year and conflicting test scores, I keep only one record and use the average test score. I standardize test scores within grade and year. I restrict the analysis to schools that belong to the baseline sample used in Table D.3, Column 3.

Teacher value added: See Appendix A.5 for data and estimation strategy. I restrict the analysis to schools contributing to the test score inequality analysis.

Class size: I use student-level course membership files for the school years 2009-2010 to 2016-2017 to compute the average section size that each first to fifth grader is exposed to within math or self-contained courses. As for classroom segregation, I exclude courses with sections enrolling fewer than five or more than 40 students. I restrict the analysis to schools contributing to the test score inequality analysis.

Gifted and Talented programs: Within North Carolina public schools, students classified as gifted usually receive extra educational resources from a dedicated special teacher while being enrolled in a regular, non-specialized classroom. They are also pulled out of their regular classrooms for some time during the regular school schedule. 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.

In line with national statistics, North Carolina G&T programs over-recruit white students: In the school year 2018-2019, approximately 77% of the Gifted and Talented third to fifth graders were white, compared to 50% of the state's student body. While the empirical evidence in support of general positive effects of G&T programs is scarce (Card and Giuliano, 2014; Bui et al., 2014), G&T enrollment has been linked to significant retention effects on advantaged students (Davis et al., 2013).

I derive the fraction of gifted students at the school-by-year level from 2009-2010 to 2016-2017, grades 1 to 5 from student-level Masterbuild files. I do not distinguish between giftedness in math or reading. I restrict the analysis to schools contributing to the test score inequality analysis.

B Overidentification Restriction

For any given "stacked" data set c, my design relies on three distinct groups of schools: (i) *treated*, i.e., those located within 0-5 miles of an opening occurring in the entry cohort c; (ii) *not-yet-treated*, i.e., schools that have not yet been closer than 5 miles from an opening, but will be later in the time window of analysis; (iii) *never-treated*, i.e., schools that never experience a charter opening within 5 miles during the time window of analysis but are part of the sample as they are "somewhat close" (5-10 miles) to some of my opening events. (Within Gilraine et al. 2021 "differential exposure" strategy, these are the control units.) The reliance on two distinct control groups can be seen as an overidentification restriction that may provide me with additional power.

I explore this possibility by conducting three types of analysis: (a) an informal overidentification test; (b) a direct comparison between the two control groups; (c) the baseline event study analysis dropping the *never-treated* units. Below, I provide more details on how each test has been executed and describe the results.

Overidentification test. I estimate the following specification:

$$
\begin{aligned}
D_{stc} = \alpha &+ \sum_{k=-4}^{-2} \beta_k \mathbb{1}[\tau_{tc} = k]\mathbb{1}[treated_{sc} = 1] + \sum_{k=0}^{+2} \gamma_k \mathbb{1}[\tau_{tc} = k]\mathbb{1}[treated_{sc} = 1] \\
&+ \sum_{k=-4}^{-2} \phi_k \mathbb{1}[\tau_{tc} = k]\mathbb{1}[nevertreated_{sc} = 1] + \sum_{k=0}^{+2} \psi_k \mathbb{1}[\tau_{tc} = k]\mathbb{1}[nevertreated_{sc} = 1] \\
&+ \delta_c X_{st} + \phi_{sc} + \phi_{tc} + \epsilon_{stc}
\end{aligned}
\tag{4}
$$

This is the baseline specification (equation (1) in the paper) augmented by the part in blue, which is a set of interactions between distance to treatment (τ_{tc}) dummies and an indicator that takes value 1 for never-treated units only. The rationale for this test is to compare the two control groups within the baseline analysis setup. Non-significant $\widehat{\phi}_k$'s and $\widehat{\psi}_k$'s, and $\widehat{\beta}$'s and $\widehat{\gamma}_k$'s similar to those obtained from equation (1) indicate that never-treated units are "good" controls. Alternatively, finding $\widehat{\phi}_k$'s and $\widehat{\psi}_k$'s significantly different from zero and substantially different $\widehat{\beta}$'s and $\widehat{\gamma}_k$'s would indicate that the never-treated units (i.e., schools 5-10 miles away) are themselves treated, thereby introducing the possibility that equation (1) does not adequately address the bias introduced by the staggered timing of the events. Appendix Table B.1 reports the estimated coefficients and suggests that never-treated units are "good" controls in the sense just defined: The treatment effects are qualitatively unaltered as the baseline specification is augmented by the never-treated interactions, which are not statistically significant. This holds within both the *pre-announced* *entry* and the *promised grade* identification strategies.

Direct comparison between control groups. I run the baseline event-study specification (equation (1) in the paper) dropping treated units from the sample and letting the coefficients measure the average difference between not-yet-treated and never-treated units. The zero effects shown in Figure B.1 of this note indicate that the two control groups do not evolve differentially after the (normalized) time of the event, corroborating the interpretation that never-treated units are good controls in this context.

Dropping never-treated schools. I run the baseline event-study specification (equation (1) in the paper) dropping never-treated units from the sample and letting the coefficients measure the average difference between not-yet-treated and treated units. The results are shown in Figure B.2 and qualitatively similar to the ones reported in the paper, indicating that my main findings do not rely on the inclusion of never-treated units in the control group.

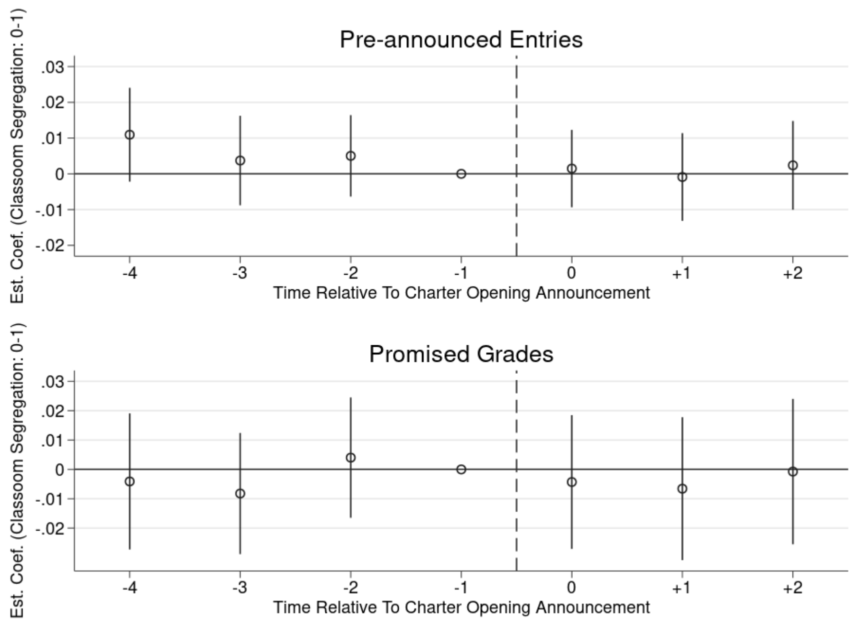
Overall, I interpret these results as suggestive of never-treated units being good controls within the context under analysis.

Table B.1: Point 1 – Over-Identification Test

VARIABLES	(1) Pre-announced entries	(2) Pre-announced entries	(3) Promised grades	(4) Promised grades
4 to event x Treated	0.004	0.013	0.009	0.005
	(0.007)	(0.010)	(0.016)	(0.019)
3 to event x Treated	0.001	0.004	0.003	-0.004
	(0.007)	(0.009)	(0.018)	(0.020)
2 to event x Treated	-0.002	0.003	0.008	0.011
	(0.006)	(0.008)	(0.016)	(0.018)
0 to event x Treated	0.013**	0.015*	0.042**	0.037*
	(0.006)	(0.008)	(0.019)	(0.021)
1 from event x Treated	0.010	0.011	0.028*	0.022
	(0.006)	(0.009)	(0.017)	(0.020)
2 from event x Treated	0.010	0.013	0.028	0.026
	(0.007)	(0.009)	(0.017)	(0.020)
4 to event x Never-treated		0.009		-0.005
		(0.007)		(0.012)
3 to event x Never-treated		0.003		-0.008
		(0.006)		(0.010)
2 to event x Never-treated		0.004		0.004
		(0.006)		(0.010)
0 to event x Never-treated		0.002		-0.006
		(0.005)		(0.012)
1 from event x Never-treated		0.001		-0.007
		(0.006)		(0.012)
2 from event x Never-treated		0.003		-0.002
		(0.006)		(0.013)
Observations	11,270	11,270	5,629	5,629
R-squared	0.755	0.755	0.542	0.543

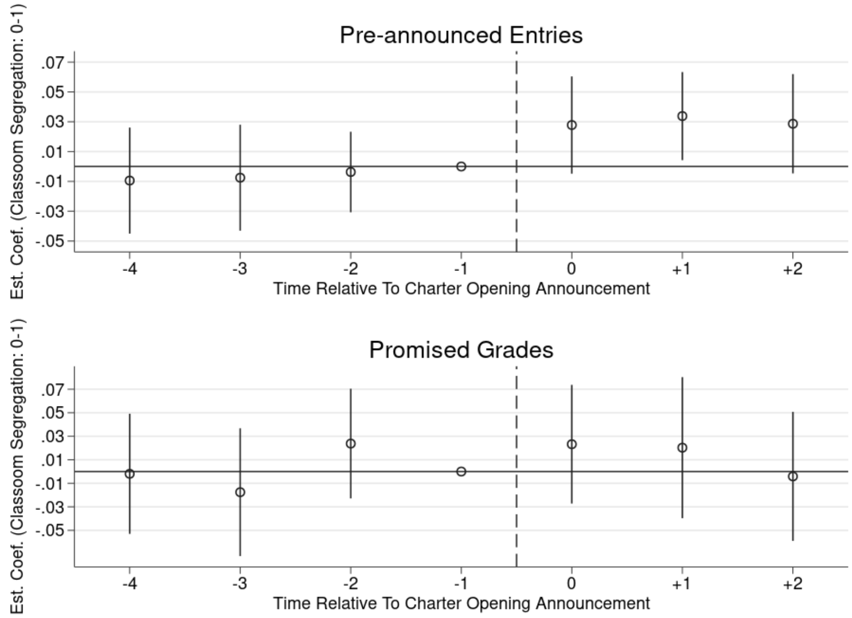
Notes: This Table reports point estimates for equation (1) in the paper – columns 1 and 3 – and its augmented version reported in equation (1) of this note – columns 2 and 4. Columns 1 and 2 rely on pre-announced entries only. Columns 3 and 4 restrict the analysis to grades that the entrant charter schools promise they would start to offer in their second year of operation. See section A.4 in the paper 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.

Figure B.1: Point 1 - Direct comparison between not-yet- and never-treated schools



Notes: This Figure displays the results obtained from estimating equation (1) in the paper, where the outcome variable is the school-by-year dissimilarity index for Math courses, grades 1 to 5. Treated units are dropped from the sample, and the coefficients measure average differences between not-yet-treated and never-treated control units. The panel is balanced. Standard errors are clustered at the school-by-entry-cohort level. *Top panel*: The events are 44 elementary charter openings that occurred between 2003 and 2005, and between 2013 and 2015. Zero is the normalized year of the opening announcement (i.e. the year before the actual opening). *Bottom panel*: 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. The grade-specific analysis excludes the terminal grade, grade 5; Including it does not qualitatively change the results, although the effects are less precisely estimated. 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 the opening announcement (i.e. the year before actual opening) for the other entry cohorts.

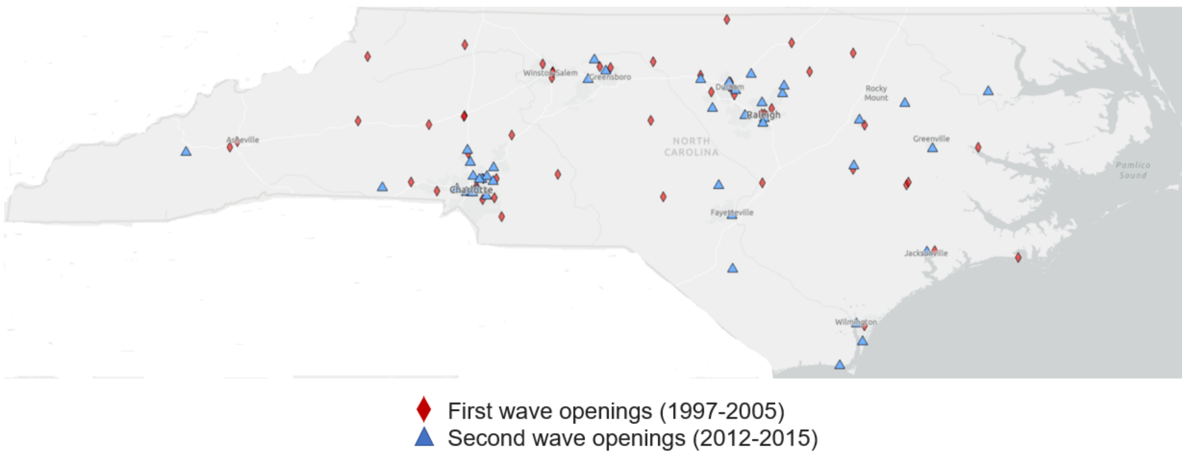
Figure B.2: Point 1 - Dropping never-treated units



Notes: This Figure displays the results obtained from estimating equation (1) in the paper, where the outcome variable is the school-by-year dissimilarity index for Math courses, grades 1 to 5. Never-treated units are dropped from the sample, and the coefficients measure average differences between not-yet-treated and treated units. The panel is balanced. Standard errors are clustered at the school-by-entry-cohort level. *Top panel*: The events are 44 elementary charter openings that occurred between 2003 and 2005, and between 2013 and 2015. Zero is the normalized year of the opening announcement (i.e. the year before the actual opening). *Bottom panel*: 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. The grade-specific analysis excludes the terminal grade, grade 5; Including it does not qualitatively change the results, although the effects are less precisely estimated. 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 the opening announcement (i.e. the year before actual opening) for the other entry cohorts.

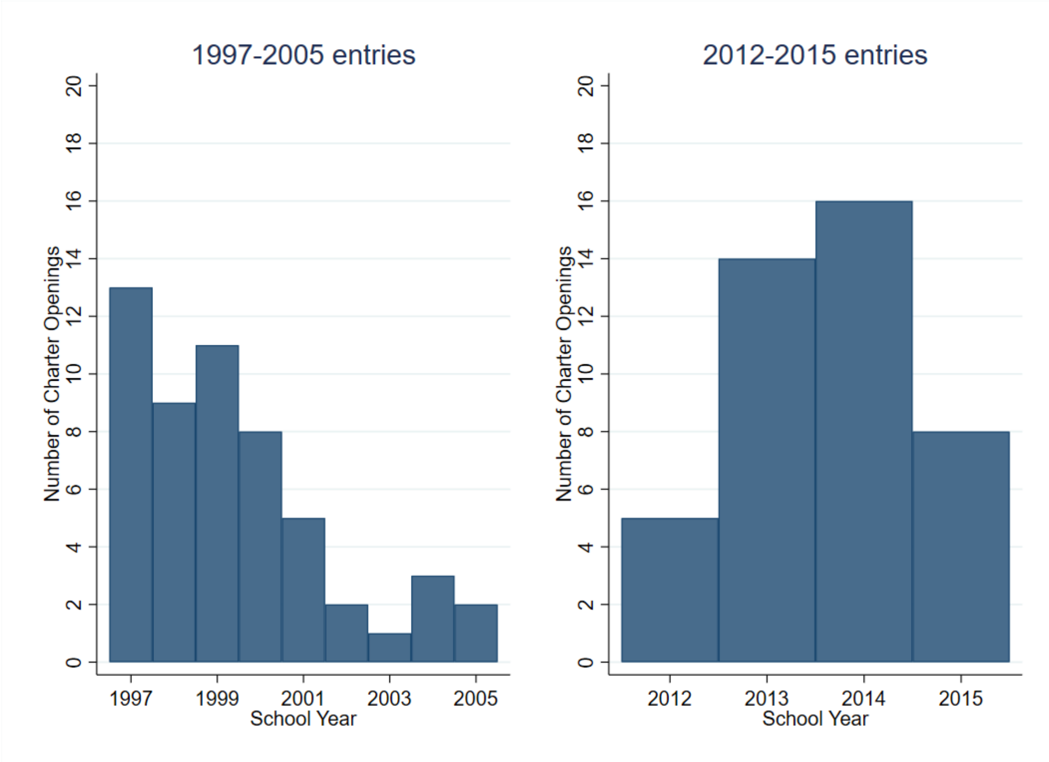
C Figures

Figure C.1: Locations of Elementary Charter School Openings: First and Second Wave



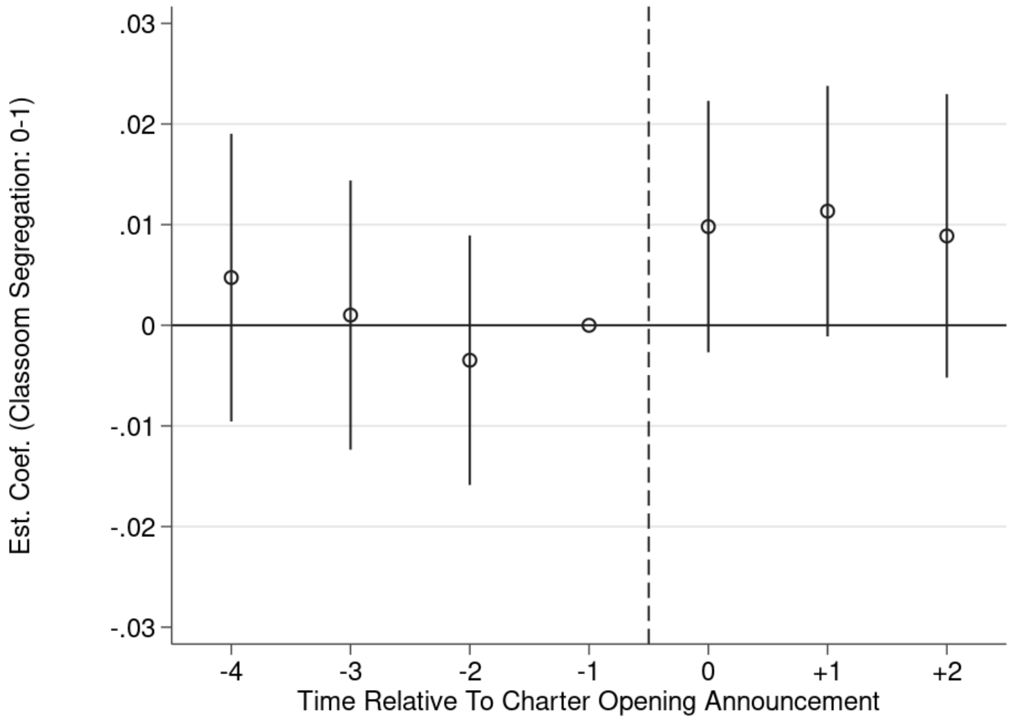
Notes: This Figure shows the locations of the 97 elementary charter schools that I exploit in this paper. Red diamonds (blue triangles) are charters that open between 1997 and 2005 (2012 and 2015.)

Figure C.2: Distribution of Charter Openings Over Time, First and Second Wave



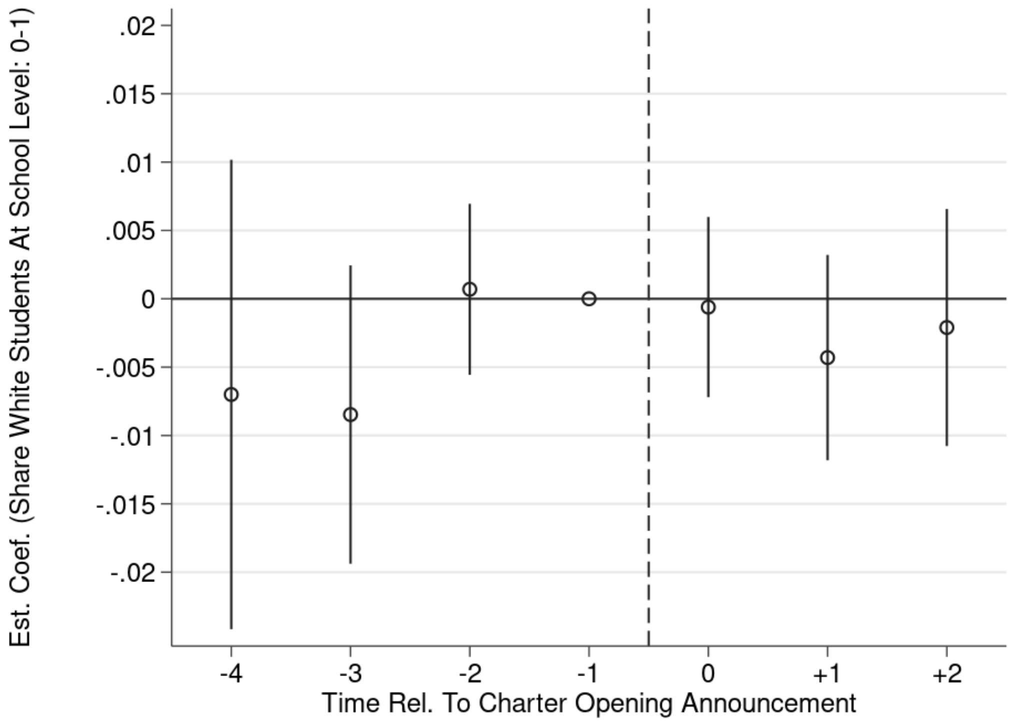
Notes: This Figure displays the over time distribution of the 97 elementary charter openings included in my sample. The left panel is for the first wave, while the right panel is for the second wave.

Figure C.3: Event Study Estimates: Reading and English Language Arts



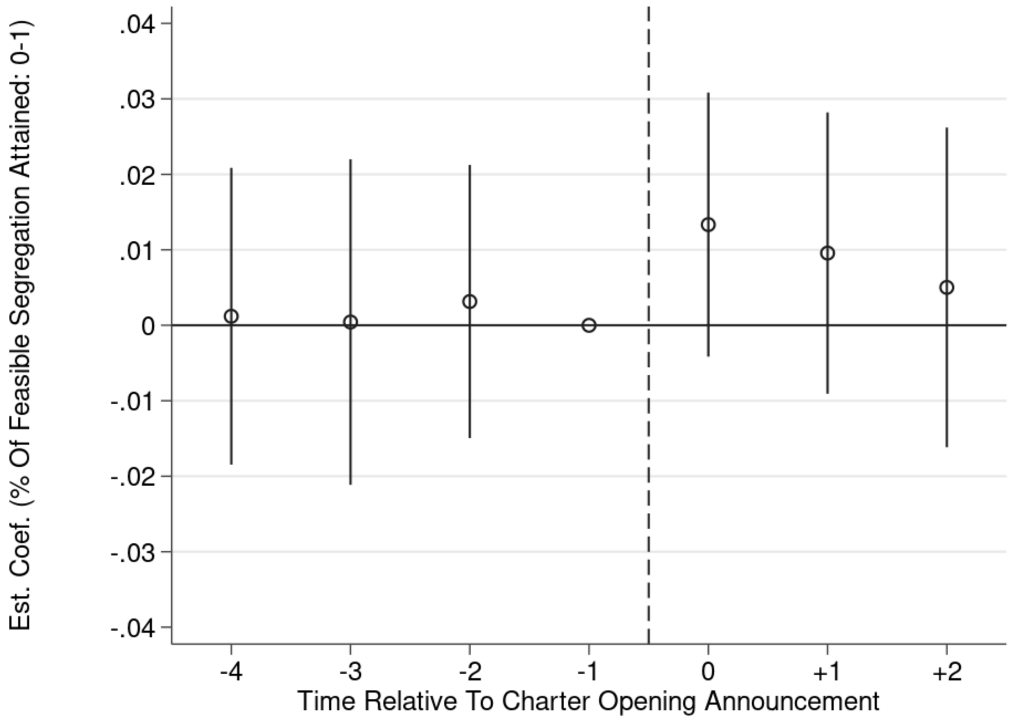
Notes: This Figure displays the estimates obtained from estimating equation (1), where the outcome variable is the school-by-year dissimilarity index for Reading and English Language Arts courses, grades 1 to 5. The events are 44 elementary charter openings occurred between 2003 and 2005, and between 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 C.4: Event Study Estimates: Share of White Students at the School-Grade Level



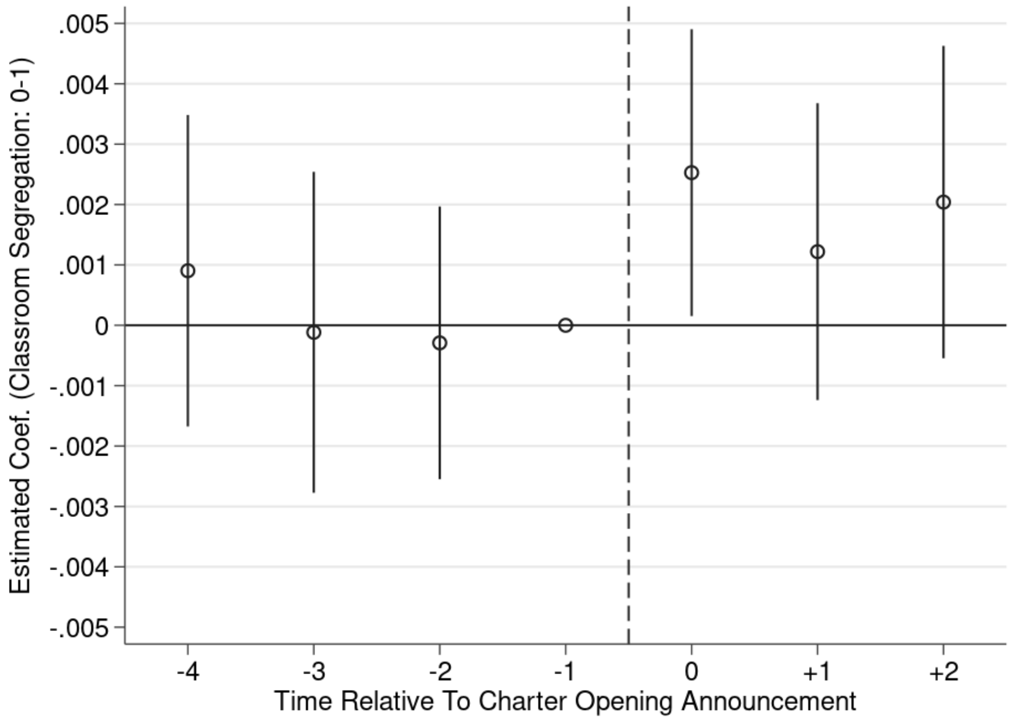
Notes: This Figure displays the estimates obtained from estimating the specification in (1), using the school-level TPS share of white students as the outcome variable. For each school and grade in a given year, the enrollment counts correspond to those used to compute the level of classroom segregation that serves as the outcome variable in Figure 3. The events are the 44 pre-announced charter openings occurred between 2003-2005 and 2013-2015. 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. Differently from Figure 3, all records for the school years 2008-2009 and 2009-2010 are excluded from the regression. As I include the observations for those years, the estimated coefficients at -4 and -3 become positive and statistically significant, while the pattern across the subsequent event times remains flat. While evidence of pre-trends at -4 and -3 does not necessarily invalidate the take-away that the shares do not change in the announcement year, or time event 0, the differences at -4 and -3 mask a significant deviation of the shares computed off classroom records from the Common Core Data totals, which I use as a "sanity benchmark", relatively to the other years in the sample. While I could not identify the reason for this difference, excluding 2008 and 2009 does not affect the results in Figure 3. Note that, given the timing of the events exploited in my analyses, 2008 and 2009 only contribute to estimating the coefficients at -4 and -3 for some of the second-wave entry cohorts, for which the main effect is relatively small anyways (see Table D.3.)

Figure C.5: Event Study Estimates: Fraction of Feasible Classroom Segregation Attained



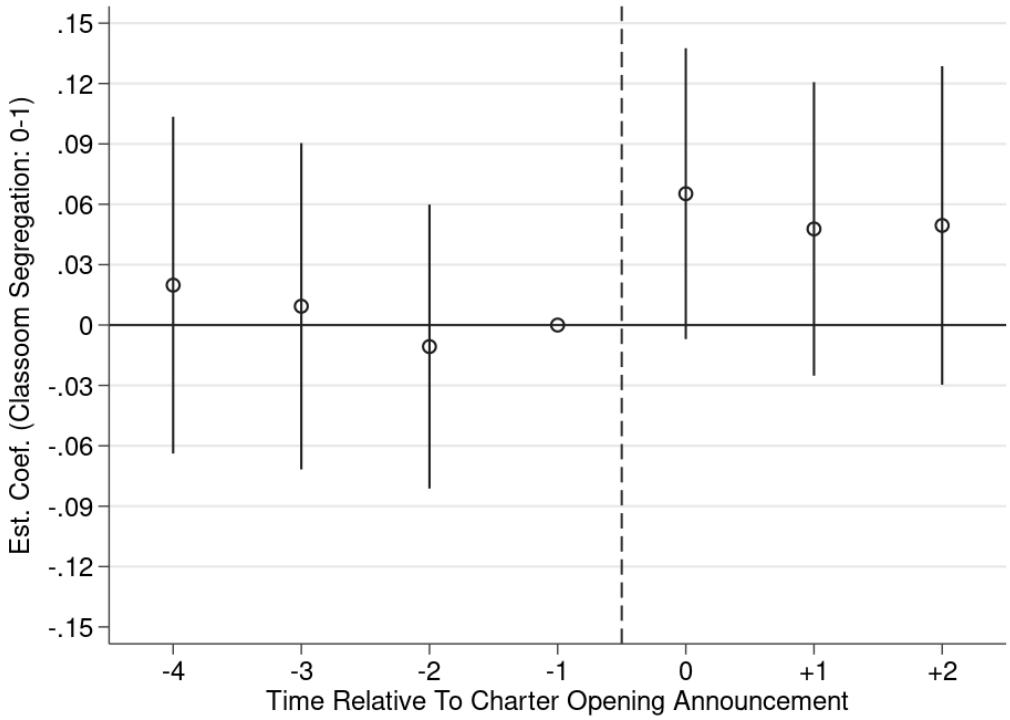
Notes: This Figure displays the estimates obtained from estimating the specification in equation (1), where the outcome variable is the fraction of feasible classroom segregation attained by the school. For a given year, the minimum and maximum feasible levels of classroom segregation are computed at the school-by-grade-by-term level, anchoring class sizes to those observed in the data, and then averaged at the school level. The events are 44 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 C.6: Robustness: Event Study Estimates with Continuous Distance



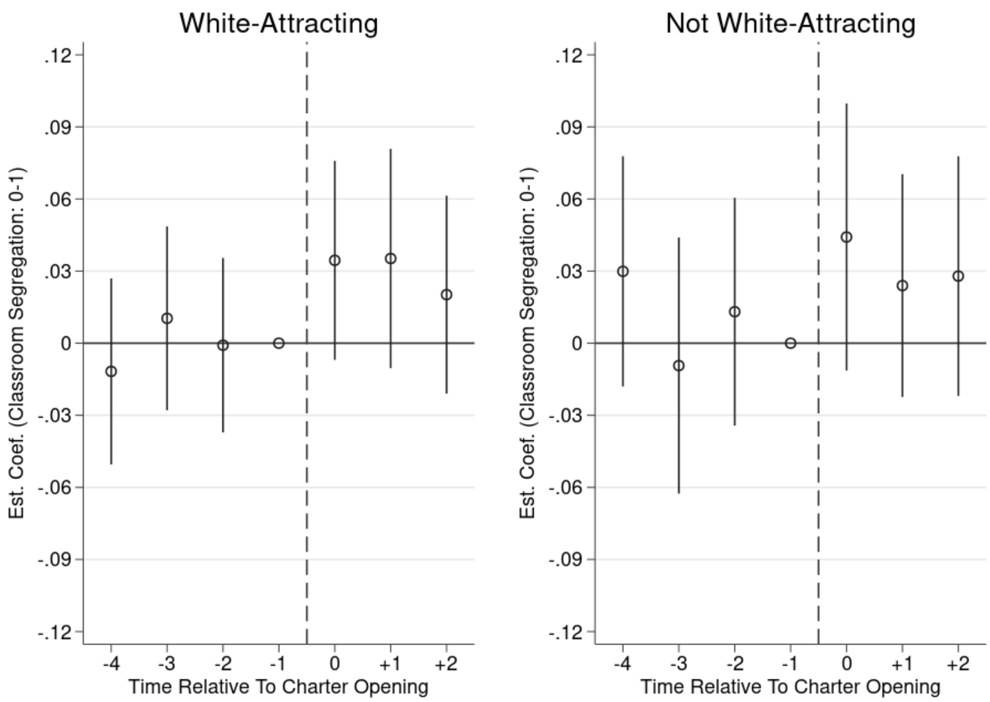
Notes: This Figure displays the results obtained from estimating a modified version of equation (1), where the outcome variable is the school-by-year dissimilarity index for Math courses, grades 1 to 5. I substitute the binary definition of treatment, based on the 5-mile distance cutoff, with a continuous 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 44 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 C.7: Robustness: Event Study Estimates with Transformed Dependent Variable



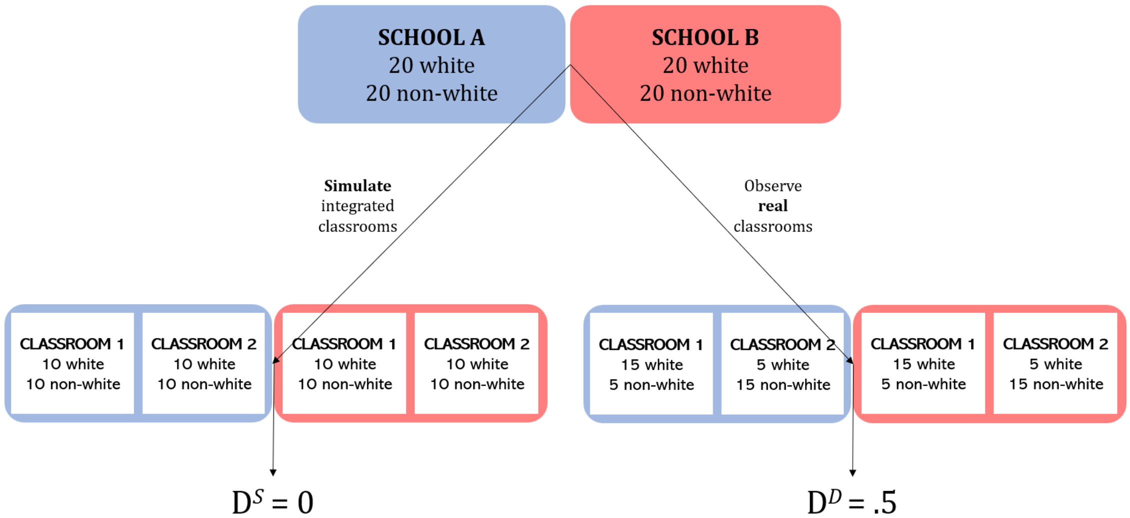
Notes: This Figure displays the estimates obtained from estimating equation (1), where the outcome variable is $log(D/(1-D))$, where D is the school-by-year dissimilarity index for Math courses, grades 1 to 5. The events are 44 elementary charter openings occurred between 2003 and 2005, and between 2013 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 C.8: Robustness: Event Study Estimates by Charter Type



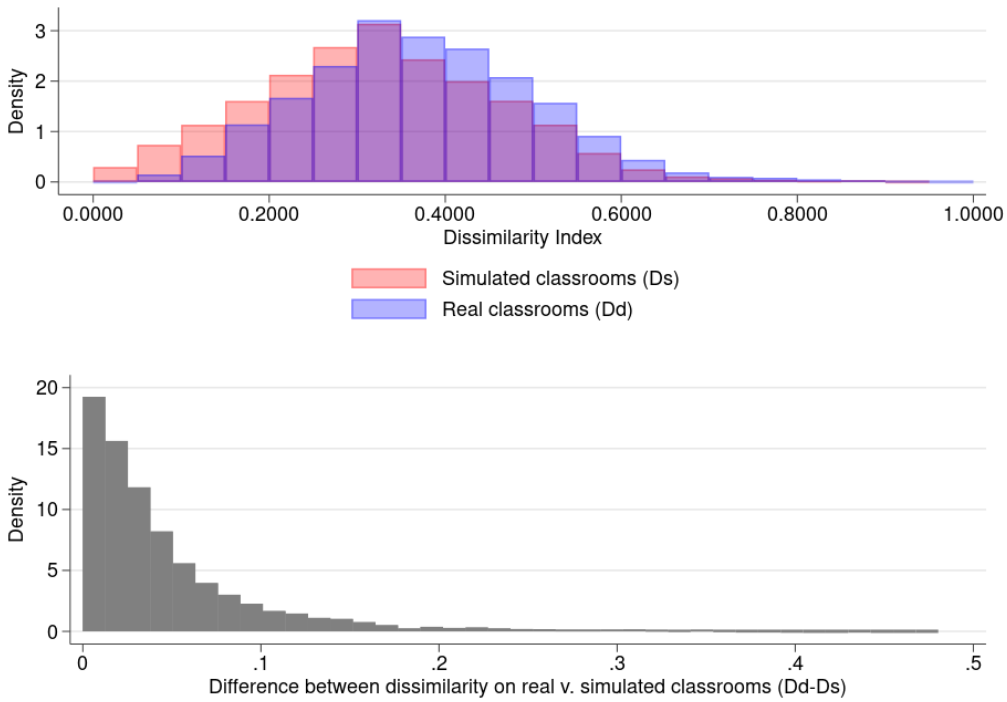
Notes: This Figure displays the estimates obtained from estimating equation (1), separately by white-attracting (i.e., Montessori, classical education, and college preparatory) vs. other charter openings. The events are the elementary charter openings that promise to add some grades one year after opening or later. 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 the opening announcement (i.e. the year before actual opening) for the other entry cohorts.

Figure C.9: Comparison Between District-Wide Segregation Measures: An Example



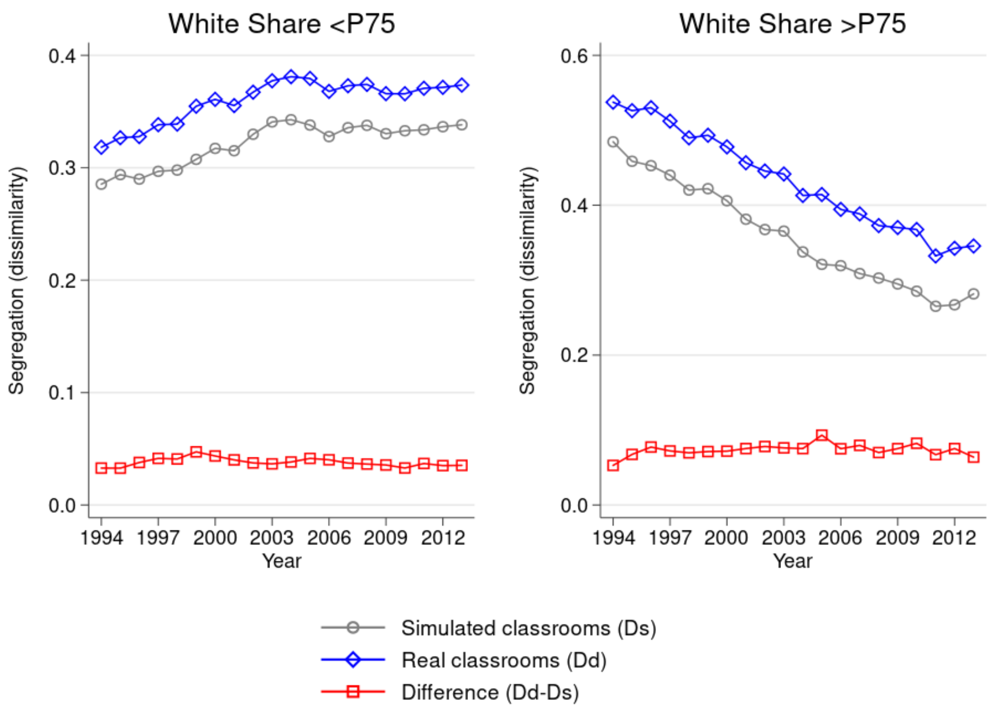
Notes: The indices are introduced in section 6.

Figure C.10: District-Level Racial Segregation
With and Without Accounting for Classroom Formation



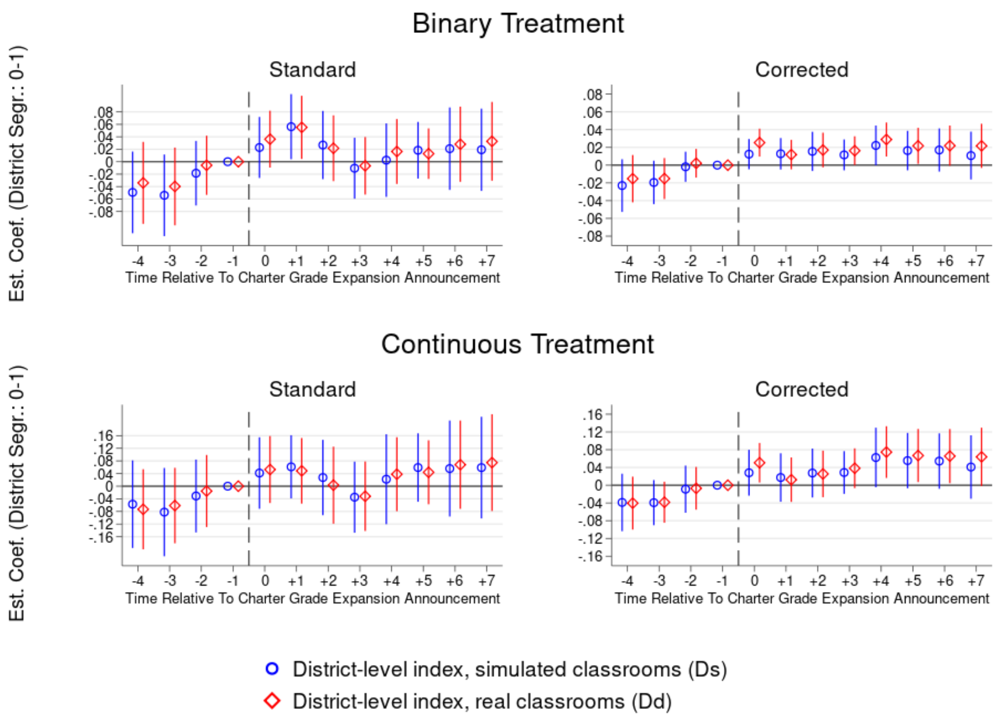
Notes: *Top panel:* distributions of the indices defined in equations 4 ($D^{\mathcal{D}}$) and 5 ($D^{\mathcal{S}}$). *Bottom panel:* distribution of $D^{\mathcal{D}} - D^{\mathcal{S}}$. In both panels, one observation is a district-grade-year combination. Grades are 1 to 4. In the rare instances (0.15% of the district-year-grade combinations in the sample) where the index calculated on simulated classrooms is larger than that computed on real classrooms, I set the simulated index equal to the real index. Districts are augmented to include nearby charters. For consistency with the main analysis, I exclude from the computation of the district-level indices the schools with one single classroom per grade-year-course-semester. The panel is balanced. These figures are produced before slicing and recomposing the sample by treatment cohort according to Cengiz et al. (2019)'s method for dealing with heterogeneous timing of treatment. See section 6.1 for details on how the indices are computed.

Figure C.11: Time Trends in District-Level Segregation
By District Racial Makeup



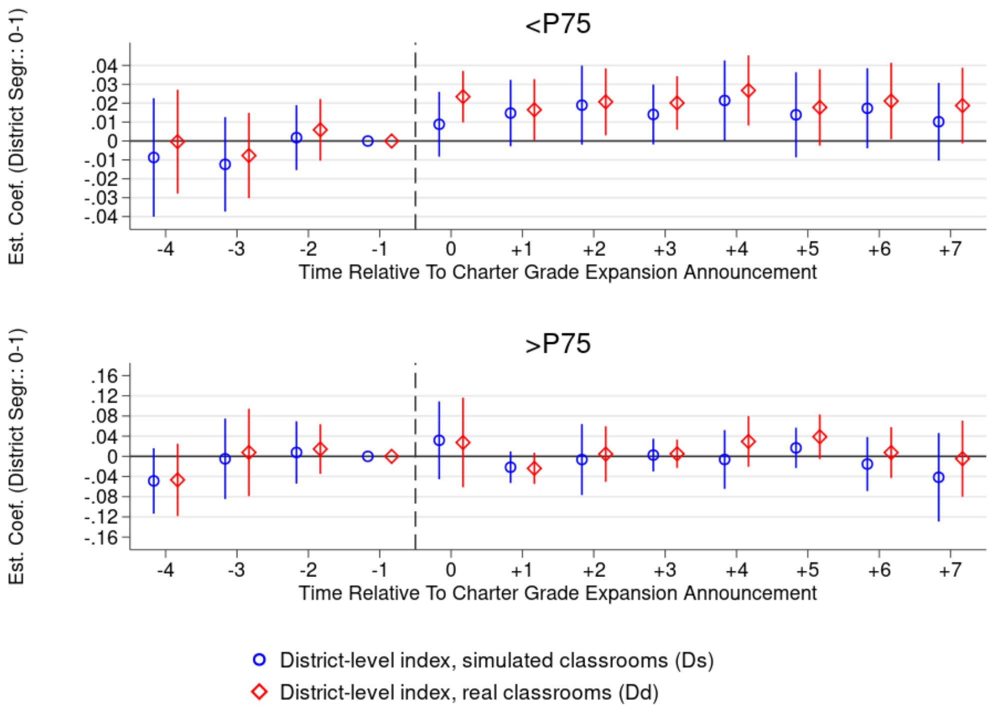
Notes: time trends in measures of segregation ($D^{\mathcal{S}}$, defined in equation (4), in gray; $D^{\mathcal{D}}$, defined in equation (5), in blue; $D^{\mathcal{D}} - D^{\mathcal{S}}$ in red) over the years incuded in the window of analysis. Districts are divided based on their share of white enrollment—below or above the third quartile of the sample distribution. Grades are 1 to 4. In the rare instances (0.15% of the district-year-grade combinations in the sample) where the index calculated on simulated classrooms is larger than that computed on real classrooms, I set the simulated index equal to the real index. Districts are augmented to include nearby charters. For consistency with the main analysis, I exclude from the computation of the district-level indices the schools with one single classroom per grade-year-course-semester. The panel is balanced. These figures are produced before slicing and recomposing the sample by treatment cohort according to Cengiz et al. (2019)'s method for dealing with heterogeneous timing of treatment. See section 6.1 for details on how the indices are computed.

Figure C.12: Charter Effects on District-Wide Segregation With and Without Accounting for Classroom Formation — Including Single-Classroom TPS



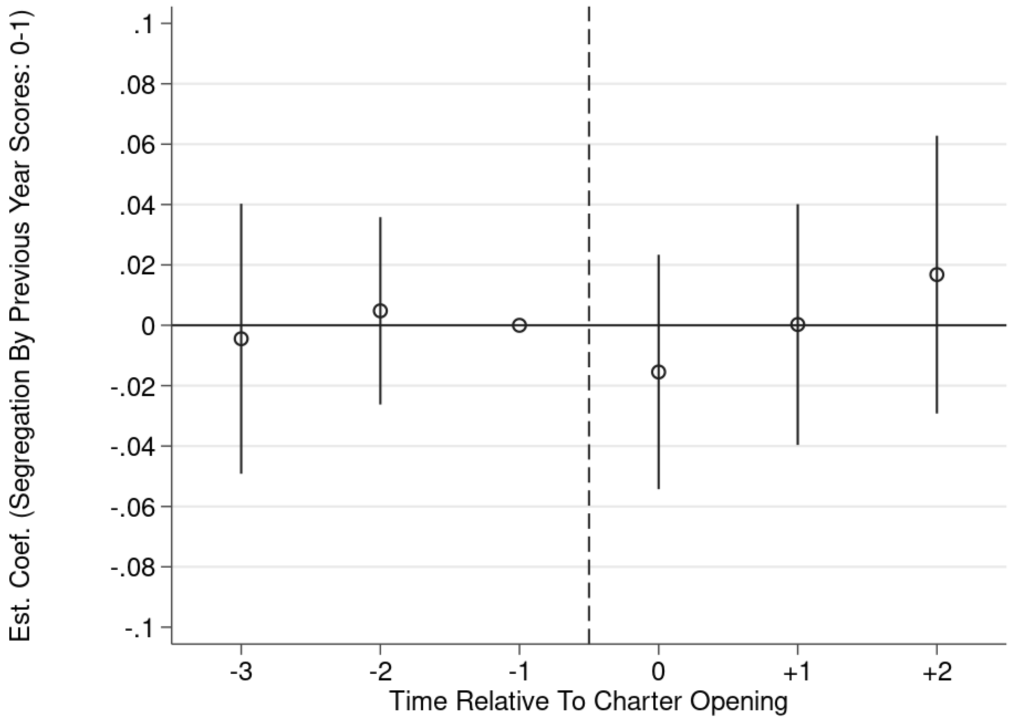
Notes: This Figure displays the estimates obtained from estimating equation (6), where the outcome variable is the district-by-grade-by-year dissimilarity index for Math and self-contained courses, grades 1 to 4, calculated with simulated classrooms (blue circles; equation (4)) vs. real classrooms (red diamonds; equation (5)) as the units of student assignments. For the latter index, the reported estimates are the linear combinations of the main and interaction effects in equation (6). In the rare instances (0.15% of the district-year-grade combinations in the sample) where the index calculated on simulated classrooms is larger than that computed on real classrooms, I set the simulated index equal to the real index. Districts are augmented to include nearby charters. Differently from the main analysis, I include from the computation of the district-level indices the schools with one single classroom per grade-year-course-semester. Standard errors are clustered at the district-by-grade-by-outcome level (i.e., the clusters, along with the fixed effects are saturated with the indicator variable — $\mathbb{1}(\mathcal{X} = \mathcal{D})$ in equation (6) — that distinguishes between different dependent variables, or records, for a given district-grade-year combination.) In all these specifications, the events are all the grade offer expansions implemented by elementary charters, as reported in the Common Core Data. The zero on the horizontal axis corresponds to the year before the expansion takes place, which I treat as the year of the expansion announcement. Control variables include the district-grade enrollment size and racial composition. The panel is balanced. *Top panel:* the treatment variable is binary and takes value equal to 1 if any strictly positive share of district-grade enrollment attends TPS within 5 miles of the expanding charter. *Bottom panel:* the treatment variable is continuous and equal to the share of district-grade enrollment attending TPS within 5 miles of the expanding charter. *Left column:* homogeneous treatment effects are assumed; Charter grade expansions occurred between 1997 and 2013 are used as events. *Right panel:* with Cengiz et al. 2019's correction; The treatment cohorts are 1997-2006. See section 6.1 for details.

Figure C.13: Charter Effects on District-Wide Segregation
With and Without Accounting for Classroom Formation
By District-Wide Share of White Enrollment



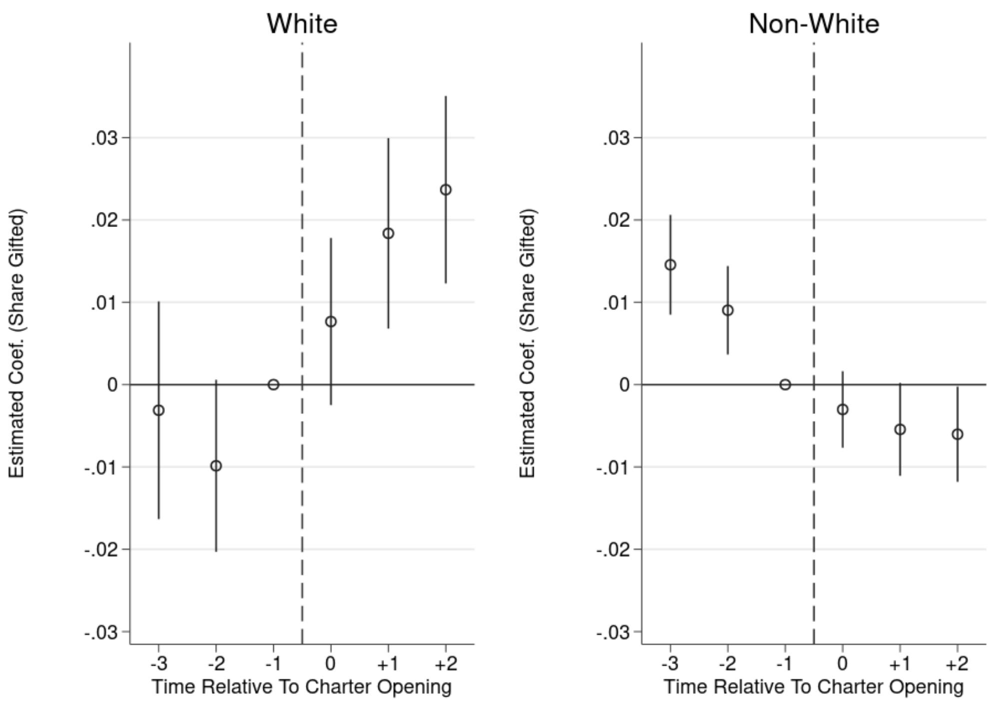
Notes: This Figure displays the estimates obtained from estimating equation (6), where the outcome variable is the district-by-grade-by-year dissimilarity index for Math and self-contained courses, grades 1 to 4, calculated with simulated classrooms (blue circles; equation (4)) vs. real classrooms (red diamonds; equation (5)) as the units of student assignments. For the latter index, the reported estimates are the linear combinations of the main and interaction effects in equation (6). In the rare instances (0.15% of the district-year-grade combinations in the sample) where the index calculated on simulated classrooms is larger than that computed on real classrooms, I set the simulated index equal to the real index. The specification is the one with binary treatment and Cengiz et al., 2019 correction. The analysis is conducted separately by racial make-up of the district: share of white enrollment below the third quartile (*top*) and above the third quartile (*bottom*). Districts are augmented to include nearby charters. For consistency with the main analysis, I exclude from the computation of the district-level indices the schools with one single classroom per grade-year-course-semester. Standard errors are clustered at the district-by-grade-by-outcome level (i.e., the clusters, along with the fixed effects are saturated with the indicator variable — $\mathbb{1}(\mathcal{X} = \mathcal{D})$ in equation (6) — that distinguishes between different dependent variables, or records, for a given district-grade-year combination.) The events are all the grade offer expansions implemented by elementary charters between 1997 and 2006, as reported in the Common Core Data. The zero on the horizontal axis corresponds to the year before the expansion takes place, which I treat as the year of the expansion announcement. Control variables include the district-grade enrollment size and racial composition. The panel is balanced. See section 6.1 for further details.

Figure C.14: Event Study Estimates: Ability Segregation



Notes: This Figure displays the estimates obtained from estimating equation (1), where the outcome variable is the difference between an index of ability segregation across classrooms computed on simulated data (or classroom configurations) and the same index computed off the real data (or classroom configurations). The simulated measure 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. Both the simulated and the actual measures are computed for math courses, and both are averaged within school and year across grades 4 and 5: Ability segregation in grade 4 is calculated based on grade 3 math test scores, while segregation in grade 5 is calculated based on grade 4 math test scores. The events are 43 elementary charter openings that occurred between 2012 and 2015. Zero is the normalized year of opening for entry cohort 2012, while it is the normalized year of the opening announcement (i.e. the year before the actual opening) for the other entry cohorts. The sample only contains data for schools that are used to estimate the specification in Table D.3, Column 3. The panel is balanced. Standard errors are clustered at the school-by-entry-cohort level.

Figure C.15: Event Study Estimates: Share of Students
In G&T Program, By Racial groups



Notes: This Figure displays the estimates obtained from estimating equation (1), where the outcome variable is the share of white (left panel) vis-à-vis 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.

D Tables

Table D.1: Comparison Between Switchers And Non-Switchers

Sample Averages	Switchers	Non-switchers	Significance
White			
2002-2003	.979	.523	***
2004-2005	.522	.502	-
2012-2013	.782	.550	***
2013-2014	.719	.516	***
2014-2015	.684	.521	***
2015-2016	.567	.441	***
Economically disadvantaged			
2002-2003	.096	.393	***
2004-2005	.315	.424	**
2012-2013	.320	.448	***
2013-2014	.281	.512	***
2014-2015	.201	.446	***
2015-2016	.325	.320	-
Math EOG score (May before opening)			
2002-2003	.338	-.021	***
2004-2005	.076	.081	-
2012-2013	.098	-.033	**
2013-2014	.399	.357	*
2014-2015	.448	.358	***
2015-2016	.371	.319	-
Reading EOG score (May before opening)			
2002-2003	.371	-.057	***
2004-2005	.220	.025	*
2012-2013	.036	-.082	*
2013-2014	.352	.332	-
2014-2015	.387	.343	***
2015-2016	.338	.311	-

Notes: This Table reports sample averages for switchers and non-switchers. Switchers are students who switch from a TPS to a charter school in the first year of operation. Non-switchers are defined as switchers' schoolmates who do not switch. The analysis is limited to TPS with at least one switcher. Due to data limitations, the analysis is restricted to openings occurred in the Fall of 2002, 2004, 2012, 2013, 2014 and 2015. The analysis is further restricted to 4th and 5th graders in the year of opening, and to schools in the main sample (i.e., TPS that experience at least one charter entry within 10 miles over the time window under analysis). Test scores are standardized at the State level. In column four, three stars (***) denote that the difference in means is significant at the 1% level; two stars (**) denote a significant difference at the 5% level; one star (*) indicates a significant difference at the 10% level; a dash (-) means that the difference is statistically indistinguishable from zero.

Table D.2: Event Study Estimates

VARIABLES	Pre-announced entries (1)	Promised grades (2)
Within 5 miles×(4 to event)	0.004	0.009
	(0.007)	(0.016)
Within 5 miles×(3 to event)	0.001	0.003
	(0.007)	(0.018)
Within 5 miles×(2 to event)	-0.002	0.008
	(0.006)	(0.016)
Within 5 miles×(0 to event)	**0.013****	**0.042****
	(0.006)	**(0.019)**
Within 5 miles×(1 after event)	0.010	0.028*
	(0.006)	(0.017)
Within 5 miles×(2 after event)	0.010	0.028
	(0.007)	(0.017)
School size	Y	Y
School white share	Y	Y
School-by-cohort FE	Y	Y
Year-by-cohort FE	Y	Y
Observations	11,270	5,629
R-squared	0.755	0.542
Mean dependent variable at -1	0.210	0.194

Notes: This Table reports point estimates for equation (1) in the paper. Column 1 relies on pre-announced entries only and corresponds to the top panel of Figure 3. Column 2 restricts the analysis to grades that the entrant charter schools promise they would start to offer in their second year of operation or later and corresponds to the bottom panel of Figure 3. 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 D.3: Difference-in-Differences Estimates by Entry Wave

VARIABLES	All entries; 7-year panel; All students (1)	1997-2005 entries; 7-year panel; All students (2)	2012-2015 entries; 7-year panel; All students (3)	2012-2015 entries; 3-year panel; All students (4)	2012-2015 entries; 3-year panel; Stayers (5)
Within 5 miles × $\mathbb{1}(Post)$	0.015***	0.019***	0.007	0.010	0.006
	(0.004)	(0.005)	(0.005)	(0.007)	(0.007)
School size	Y	Y	Y	Y	Y
School white share	Y	Y	Y	Y	Y
School-by-entry cohort FE	Y	Y	Y	Y	Y
Year-by-entry cohort FE	Y	Y	Y	Y	Y
Student cohort-by-entry cohort FE	N	N	N	Y	Y
Observations	26,068	19,621	6,447	8,169	8,169
R-squared	0.782	0.794	0.726	0.475	0.531
Mean dep. var.	0.204	0.198	0.223	0.203	0.256

Notes: *Columns 1-3* report estimates of equation (2) in the paper, separately by entry wave. The coefficient reported in Column 1 is the same as that reported in Table 2, Column 1. The panel is balanced. Standard errors are clustered at the school-by-entry-cohort level. *Columns 4-5* report estimates obtained from estimating equation (2) using only individual (course membership) data to compute the dependent variables. In column 4, the outcome variable is the index of dissimilarity computed on all students; in column 5, the outcome variable is the dissimilarity index computed only on students that, at the moment of any given charter opening (or announcement, depending on the entry cohort) have been enrolled in the same TPS for three consecutive years. The events are the charter openings occurred in the school years 2012-2013 through and 2015-16, as individual-level classroom assignments are not available to explore the effects of the first wave of charter entries. The post period is the opening year for the 2012 entry cohort, while it is the announcement year for the other entry cohorts. The panel is balanced, with three observations (years) per school and entry cohort. For each entry cohort (and three-year data set,) and for any given school, I identify and compute classroom segregation (on all vis-à-vis incumbent students) for three distinct student cohorts: those in grade 1 in the first year, grade 2 in the second year, grade 3 in the third year of the data set are cohort A; those in grade 2 in the first year, grade 3 in the second year, grade 4 in the third year are cohort B; those in grade 3 in the first year, grade 4 in the second year, grade 5 in the third year are cohort C. The regression is therefore run at the school by student cohort by entry cohort level, and student cohort fixed effects are saturated with the entry cohort indicators. Standard errors are clustered at the school-by-entry-cohort level. *** p<0.01, ** p<0.05, * p<0.1.

Table D.4: Charter Effects on District-Wide Segregation
With and Without Accounting for Classroom Formation:
Event Study Estimates—Without Cengiz et al. (2019) correction

VARIABLES	(1) Binary treatment	(2) Continuous treatment
4 to event x Treated	-0.044** (0.022)	-0.035 (0.057)
3 to event x Treated	-0.033* (0.019)	-0.056 (0.042)
2 to event x Treated	-0.007 (0.018)	-0.049 (0.037)
0 to event x Treated	0.025 (0.018)	0.043 (0.041)
1 from event x Treated	0.046*** (0.017)	0.056 (0.041)
2 from event x Treated	0.027 (0.018)	0.034 (0.042)
3 from event x Treated	-0.006 (0.014)	-0.026 (0.035)
4 from event x Treated	0.004 (0.020)	0.014 (0.051)
5 from event x Treated	0.022 (0.019)	0.076* (0.040)
6 from event x Treated	0.028 (0.028)	0.066 (0.058)
7 from event x Treated	0.027 (0.027)	0.072 (0.061)
4 to event x Treated x Classroom	0.011 (0.032)	-0.035 (0.075)
3 to event x Treated x Classroom	0.003 (0.026)	-0.019 (0.056)
2 to event x Treated x Classroom	0.002 (0.024)	-0.004 (0.049)
0 to event x Treated x Classroom	0.015 (0.024)	0.011 (0.056)
1 from event x Treated x Classroom	-0.001 (0.023)	-0.013 (0.059)
2 from event x Treated x Classroom	-0.005 (0.025)	-0.028 (0.061)
3 from event x Treated x Classroom	0.004 (0.019)	-0.001 (0.050)
4 from event x Treated x Classroom	0.012 (0.027)	0.013 (0.068)
5 from event x Treated x Classroom	-0.004 (0.025)	-0.018 (0.056)
6 from event x Treated x Classroom	0.008 (0.038)	0.014 (0.083)
7 from event x Treated x Classroom	0.013 (0.037)	0.012 (0.085)
District-grade FEs	Y	Y
Year FEs	Y	Y
Observations	7,536	7,536
R-squared	0.671	0.670

Notes: This Table displays the estimates obtained from estimating equation (6), where the outcome variable is the district-by-grade-by-year dissimilarity index for Math and self-contained courses, grades 1 to 4, calculated with simulated classrooms (equation (4)) vs. real classrooms (equation (5)) as the units of student assignments. For the latter index, the reported estimates are the linear combinations of the main and interaction effects in equation (6). In the rare instances (0.15% of the district-year-grade combinations in the sample) where the index calculated on simulated classrooms is larger than that computed on real classrooms, I set the simulated index equal to the real index. Districts are augmented to include nearby charters. For consistency with the main analysis, I exclude from the computation of the district-level indices the schools with one single classroom per grade-year-course-semester. Standard errors are clustered at the district-by-grade-by-outcome level (i.e., the clusters, along with the fixed effects are saturated with the indicator variable — $\mathbb{1}(\mathcal{X} = \mathcal{D})$ in equation (6) — that distinguishes between different dependent variables, or records, for a given district-grade-year combination.) In all these specifications, the events are all the grade offer expansions implemented by charter elementary charters between 1997 and 2013, as reported in the Common Core Data. Control variables include the district-grade enrollment size and racial composition. The panel is balanced. Specification without Cengiz et al., 2019 correction. See section 6.1 for further details.

Table D.5: Charter Effects on District-Wide Segregation
With and Without Accounting for Classroom Formation:
Event Study Estimates—With Cengiz et al. (2019) correction

VARIABLES	(1) Binary treatment	(2) Continuous treatment
4 to event x Treated	-0.023 (0.015)	-0.039 (0.033)
3 to event x Treated	-0.020 (0.012)	-0.039 (0.026)
2 to event x Treated	-0.002 (0.009)	-0.009 (0.027)
0 to event x Treated	0.012 (0.009)	0.028 (0.026)
1 from event x Treated	0.013 (0.009)	0.017 (0.028)
2 from event x Treated	0.016 (0.011)	0.027 (0.028)
3 from event x Treated	0.012 (0.009)	0.029 (0.025)
4 from event x Treated	0.022* (0.011)	0.062* (0.034)
5 from event x Treated	0.016 (0.011)	0.056* (0.032)
6 from event x Treated	0.017 (0.012)	0.054* (0.032)
7 from event x Treated	0.011 (0.014)	0.041 (0.036)
4 to event x Treated x Classroom	0.008 (0.020)	-0.002 (0.045)
3 to event x Treated x Classroom	0.004 (0.017)	0.001 (0.035)
2 to event x Treated x Classroom	0.004 (0.012)	0.002 (0.037)
0 to event x Treated x Classroom	0.013 (0.012)	0.023 (0.035)
1 from event x Treated x Classroom	-0.001 (0.012)	-0.005 (0.038)
2 from event x Treated x Classroom	0.002 (0.015)	-0.002 (0.039)
3 from event x Treated x Classroom	0.005 (0.012)	0.010 (0.034)
4 from event x Treated x Classroom	0.007 (0.015)	0.012 (0.045)
5 from event x Treated x Classroom	0.005 (0.015)	0.011 (0.044)
6 from event x Treated x Classroom	0.005 (0.017)	0.011 (0.045)
7 from event x Treated x Classroom	0.011 (0.019)	0.023 (0.050)
District-grade FEs	Y	Y
Year FEs	Y	Y
Observations	63,696	63,696
R-squared	0.669	0.669

Notes: This Table displays the estimates obtained from estimating equation (6), where the outcome variable is the district-by-grade-by-year dissimilarity index for Math and self-contained courses, grades 1 to 4, calculated with simulated classrooms (equation (4)) vs. real classrooms (equation (5)) as the units of student assignments. For the latter index, the reported estimates are the linear combinations of the main and interaction effects in equation (6). In the rare instances (0.15% of the district-year-grade combinations in the sample) where the index calculated on simulated classrooms is larger than that computed on real classrooms, I set the simulated index equal to the real index. Districts are augmented to include nearby charters. For consistency with the main analysis, I exclude from the computation of the district-level indices the schools with one single classroom per grade-year-course-semester. Standard errors are clustered at the district-by-grade-by-outcome level (i.e., the clusters, along with the fixed effects are saturated with the indicator variable — $\mathbb{1}(\mathcal{X} = \mathcal{D})$ in equation (6) — that distinguishes between different dependent variables, or records, for a given district-grade-year combination.) In all these specifications, the events are all the grade offer expansions implemented by charter elementary charters between 1997 and 2006, as reported in the Common Core Data. Control variables include the district-grade enrollment size and racial composition. The panel is balanced. Specification with Cengiz et al., 2019 correction. See section 6.1 for further details.

Table D.6: Charter Effects on District-Wide Segregation
With and Without Accounting for Classroom Formation:
Diff-in-Diff Estimates (Binary Treatment, Cengiz et al. 2019 Correction)

VARIABLES	(1) All districts	(2) White share <Q3	(3) White share >Q3
$\mathbb{1}$(Post)	0.026***	0.020***	0.007
	(0.008)	(0.007)	(0.008)
$\mathbb{1}$(Post) x $\mathbb{1}$(Classroom)	0.002	0.001	0.010
	(0.011)	(0.009)	(0.013)
District-grade FEs	Y	Y	Y
Year FEs	Y	Y	Y
Observations	63,696	46,368	16,464
R-squared	0.669	0.731	0.586
Linear combination	0.028***	0.021***	0.017*
	(0.008)	(0.006)	(0.010)
Avg school-based, pre	0.345	0.319	0.414
Avg classroom-based, pre	0.392	0.357	0.486

Notes: This Table displays the estimates obtained from estimating equation (6), where the outcome variable is the district-by-grade-by-year dissimilarity index for Math and self-contained courses, grades 1 to 4, calculated with simulated classrooms (equation (4)) vs. real classrooms (equation (5)) as the units of student assignments. For the latter index, the reported estimates are the linear combinations of the main and interaction effects in equation (6). In the rare instances (0.15% of the district-year-grade combinations in the sample) where the index calculated on simulated classrooms is larger than that computed on real classrooms, I set the simulated index equal to the real index. The analysis is conducted separately by racial make-up of the district: share of white enrollment below the third quartile (*column 2*) vis-à-vis above the third quartile (*column 3*). Districts are augmented to include nearby charters. For consistency with the main analysis, I exclude from the computation of the district-level indices the schools with one single classroom per grade-year-course-semester. Standard errors are clustered at the district-by-grade-by-outcome level (i.e., the clusters, along with the fixed effects are saturated with the indicator variable — $\mathbb{1}(\mathcal{X} = \mathcal{D})$ in equation (6) — that distinguishes between different dependent variables, or records, for a given district-grade-year combination.) In all these specifications, the events are all the grade offer expansions implemented by charter elementary charters between 1997 and 2006, as reported in the Common Core Data. Control variables include the district-grade enrollment size and racial composition. The panel is balanced. Specification with Cengiz et al., 2019 correction. See section 6.1 for further details.

Table D.7: Difference-in-Differences Estimates, Ability Segregation

VARIABLES	Simulated (1)	Actual (2)	Simulated - Actual (3)
Within 5 miles × Post	0.026**	0.026**	0.000
	(0.011)	(0.013)	(0.016)
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	1,554	1,554	1,554
R-squared	0.658	0.744	0.684
Mean dependent variable pre-entry	0.141	0.099	0.042

Notes: This Table displays the estimates obtained from equation (2) in the paper using the 43 charter openings occurred between 2012 and 2015. The dependent variable in Column 1 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 dependent variable in Column 2 is the actual index of ability segregation. The dependent variable in Column 3 is the difference between simulated and actual measures. Both the dependent variables in Columns 1 and 2 refer to math courses, and both are averaged within school and year across grades 4 and 5: Ability segregation in grade 4 is calculated based on grade 3 math test scores, while segregation in grade 5 is calculated based on grade 4 math test scores. In both analyses the panel is balanced. The sample only contains data for schools that are used to estimate the specification in Table D.3, Column 3. Standard errors are clustered at the school-by-entry-cohort level.*** p<0.01, ** p<0.05, * p<0.1.

Table D.8: Difference-in-Differences Estimates: Mechanisms

VARIABLES	Value Added White (1)	Value Added Non-White (2)	Class Size White (3)	Class Size Non-White (4)	Gifted White (5)	Gifted Non-White (6)
Within 5 miles	-0.002	-0.005	0.067	0.047	0.021***	-0.012***
× Post	(0.007)	(0.006)	(0.163)	(0.152)	(0.005)	(0.002)
Size	Y	Y	Y	Y	Y	Y
% White	Y	Y	Y	Y	Y	Y
School FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Observations	5,851	5,851	5,639	5,639	5,817	5,817
R-squared	0.404	0.415	0.333	0.340	0.866	0.583
Mean dep. var.	0.015	0.012	20.883	20.860	0.159	0.060

Notes: This Table displays the estimates obtained from estimating equation (2) in the paper, using the 43 charter openings that occurred between 2012 and 2015. The dependent variable in Columns (1) and (2) is the average teacher value-added experienced by students within a given school-grade in a given year (see section A.5 for estimation details). The dependent variable in Columns (3) and (4) is the average class size measured at the same level. The dependent variable in Columns (5) and (6) is the fraction of students labeled as Gifted and Talented in a given school-year-grade. Odd (even) numbered Columns display the estimated effect on white (non-white) students only. 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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