American Economic Journal: Economic Policy 2022, 14(1): 301–340
https://doi.org/10.1257/pol.20190682

The Effect of Charter Schools on School Segregation†

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We examine the impact of the expansion of charter schools on racial segregation in public schools, defined using multiple measures of racial sorting and isolation. Our research design utilizes between-grade differences in charter expansion within school systems and an instrumental variables approach leveraging charter school openings. Charter schools modestly increase school segregation for Black, Hispanic, Asian, and White students. 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. For metropolitan areas, our analysis reveals countervailing forces, as charters reduce segregation between districts. (*JEL* I21, I24, J15)

Charter schools have steadily expanded in the United States over the last two decades. Proponents of the charter model argue that school choice generates education improvements via competitive pressure that rewards school effectiveness and improves matches between students and schools (Friedman 1962; Coons and Sugarman 1978; Chubb and Moe 1990; Hoxby 2003; Betts 2005). Indeed, multiple studies show that charter schools are effective at improving student test scores through both mechanisms (Dobbie and Fryer 2011; Angrist et al. 2012, 2016; Cordes 2018). But there is also evidence that parents don't necessarily select schools on the basis of effectiveness, dampening competitive incentives and potentially increasing disparities in public schooling through increased sorting on other dimensions (Rothstein 2006; Abdulkadiroğlu et al. 2020; Walters 2018).[1]

At the same time, one of the most pernicious and persistent issues in US education policy over the last century is racial segregation in public schools. More than 65 years after the pivotal *Brown v. Board of Education* Supreme Court decision, segregation by race is still pervasive across public school districts in the United

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† Go to https://doi.org/10.1257/pol.20190682 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

[1] Studies examining the alignment of parental choices and school effectiveness typically rely on test score value-added measures. A growing literature demonstrates that parents also value other school characteristics, and such characteristics are also predictive of important later life outcomes. See, for example, Beuermann et al. (2018).

States. The negative impact of racial segregation on students has been documented in a large literature spanning across the social sciences (Welch et al. 1987; Guryan 2004; Card and Rothstein 2007; Clotfelter 2011; Billings, Deming, and Rockoff 2014; Reardon and Owens 2014; Johnson 2015). Given the fact that the charter school model necessarily entails active choices made by families, a key remaining question is the impact that charter school expansion has had on the racial stratification of public school systems.

Because multiple potential mechanisms may drive a causal link between the growth of charter schools and segregation, theory is ambiguous on the net effect. On the one hand, choice entails decoupling school assignments from residential neighborhoods that tend to be segregated, which by itself may impact stratification. On the other, greater choice may lead to segregation if parents have strong peer preferences, lack equal access to resources, or confound school quality with school racial composition or other correlated observables (Abdulkadiroğlu et al. 2020). An additional complication is that many charter schools in urban areas specifically recruit historically underserved students (Angrist, Pathak, and Walters 2013). Estimating the causal effect of charter schools on school segregation is thus complicated by sorting dynamics that are endogenous to numerous underlying forces.

A summary of the research on charter schools and segregation described it as "regrettably weak" and noted that little is known about how charter schools affect the distribution of students in school systems (Gill et al. 2007). Descriptive studies have established that on average, minority students are more concentrated in the charter school sector than in traditional public schools (Frankenberg, Siegel-Hawley, and Wang 2010). However, similar studies focusing on urban areas have found that racial isolation of students in charters and district schools is similar (Ritter et al. 2010). Other case studies tracking the effect of student transfers between district and charter schools find mixed evidence, suggesting both positive and negative effects on racial isolation that vary among different localities (Bifulco, Ladd, and Ross 2009; Bifulco and Ladd 2007; Garcia 2008; Ladd and Turaeva 2020; Ritter et al. 2014, Zimmer et al. 2009).[2]

We contribute to the literature by providing the first comprehensive evidence on the causal effect of charter schools on racial segregation, using 1998–2018 panel data on the demographic breakdown of almost every public school in the country.[3] Our preferred index of segregation examines relative evenness in the distribution of different student groups across schools, but we also consider impacts using absolute measures of racial exposure and isolation. Intuitively, our preferred index measures the relative likelihood that a student has schoolmates from a different racial/ethnic group. We document impacts for the stratification of the four largest racial and ethnic groups and study effects at varying levels of geography. Our

[2] Studies from North Carolina found that White parents tended to make moves that exacerbate their own isolation, while Black parents made moves that increase their intergroup exposure. Zimmer et al. (2009) examined transfers to charters in seven cities and states and concluded that on average, student transfers between traditional public and charter schools were neutral, though there was considerable variation by region.

[3] In the pre-*Brown* era, the term segregation reflected intentional legally coerced segregation, as opposed to the de facto segregation we examine. Though we use the term segregation throughout this paper, this caveat should be kept in mind. For an additional discussion regarding how to define segregation in this context, see Monarrez, Kisida, and Chingos (2019).

identification strategy is based on a triple differences research design that leverages between-grade variation in the charter share of enrollment within a given school system and year. Our effect estimates are based on average differences in the dynamics of segregation between grade levels that have experienced differing intensity in charter growth. In addition, we exploit the arguably cleaner source of variation generated by charter school opening events in an instrumental variables (IV) framework, generating similar estimates to our baseline OLS models.

The results show that the expansion of charter schools has led to increased levels of segregation for every major racial and ethnic group across school districts, cities, and counties, especially in urban school systems with large populations of Black and Hispanic students. For the largest geographies, metropolitan areas, we uncover countervailing forces—while charters worsen segregation between schools in a metropolitan area, they improve integration between school district jurisdictions, where much of the racial segregation exists. The increase in segregation caused by charters in the average district is modest, but larger for systems with large charter shares. Our estimates suggest that a 1 percentage point (p.p.) increase in the fraction of public school enrollment going to charter schools causes about a 0.10 p.p. increase in segregation for Black or Hispanic students, or about 1 percent of the standard deviation of the 2018 segregation distribution. When looking at groups separately, segregation impacts are larger for Black and White students than for Hispanic or Asian students. We also document substantial effect heterogeneity across states, finding a potential mechanism related to state differences in charter schools' relative presence in urban districts and their target student population.

We demonstrate that the flexible controls in our models are effective at eliminating confounding components of the correlation between charter school presence and racial stratification. Using a distributed lags model, we show that future charter growth is not predictive of current segregation levels, suggesting a lack of preexisting trends in the outcomes of interest. Additionally, we establish that were charter school enrollment drawn randomly from the non-charter-school population, charters would have a small integration effect on public schools instead of the segregation effect identified in the data. Finally, we conduct a series of placebo tests asking whether changes in the charter share in one grade are spuriously linked to segregation in other grades, finding encouraging patterns. Altogether, the evidence indicates that our estimates constitute reliable average treatment effects on the treated.

Our analysis provides compelling evidence that charter schools have led to higher average racial and ethnic segregation in US public schools. However, a clear normative stance on the implications of these impacts is complicated by the voluntary nature of school choice. On the one hand, there is enormous evidence of the beneficial impacts of school integration on the educational and socioeconomic outcomes of racial and ethnic minorities.[4] Through this lens, charters leading to heightened

[4] See, for example, Johnson and Nazaryan (2019). Evidence on both the harm of segregation and the benefit of integration on a multiplicity of student outcomes has been documented in decades of research (Crain and Mahard 1978; Crain and Strauss 1985; Clotfelter 2011; Guryan 2004; Ashenfelter, Collins, and Yoon 2006; Jackson 2009; Reber 2010; Reardon and Owens 2014; Billings, Deming, and Rockoff 2014; Hanushek, Kain, and Rivkin 2009; Card and Rothstein 2007).

segregation is particularly worrisome. On the other hand, the stated mission of charter schools is often to serve students from underserved populations, and many have been shown to improve student outcomes.[5] Because they serve homogeneous student bodies, specialized charter schools are likely to cause increased segregation within school systems. Segregation in the charter school sector is thus fundamentally different from the de jure segregation of the pre–*Brown* era, which explicitly provided fewer resources for the education of Black students (Card, Domnisoru, and Taylor 2018). As such, we caution that school segregation caused by charter schools and segregation forced by government statute should not be interpreted through the same lens. More research is needed to understand whose choices drive charters' effect on segregation and the impact that choice-driven segregation has on educational outcomes.

The rest of the paper proceeds as follows. Section I provides additional background on the history of school segregation and school choice as well as the empirical and theoretical literature on the effects of segregation for student outcomes. Section II describes our data and estimation sample and presents descriptive statistics. Section III develops our empirical framework to estimate the causal effect of charter schools on segregation. Section IV presents the main results of the paper, conducts a range of robustness checks, and provides our analysis of effect heterogeneity. Section V concludes.

I. Background

The issue of segregation and school choice has particular significance in the United States, as historically it was used as a tool to maintain segregated schools in the South following the Supreme Court's 1954 *Brown v. Board of Education* decision (Reardon and Owens 2014). In 1968, 14 years after *Brown*, US public schools were still intensely segregated, with an average within-district variance ratio index of 0.63 in urban districts (Coleman, Kelly, and Moore 1975).[6] As a result of court-ordered desegregation plans in the mid-1970s, these rates fell substantially, with the largest declines in the South (Clotfelter 20111; Reber 2005). At the same time that within-district segregation was decreasing, however, between-district segregation increased, particularly in areas where school districts tended to be smaller and more numerous (Clotfelter 2011). This form of de facto segregation, facilitated through White flight and racist housing market practices, was more difficult to address after the Supreme Court's 1974 *Milliken v. Bradley* decision ruled against court-ordered interdistrict desegregation plans (Kruse 2013).

There is a rich literature supporting the notion that the court-ordered desegregation plans of the 1970s and 1980s caused large reductions in racial disparities in socioeconomic outcomes. In terms of measurable educational outcomes, an

[5] While evidence on the mean national impact of charter schools on student achievement is mixed, studies using school lotteries in urban settings find that charters are more effective than other public schools at raising student test scores (Gleason et al. 2015; Abdulkadiroğlu et al. 2011; Dobbie and Fryer 2011; Angrist et al. 2012; Angrist et al. 2016, Abdulkadiroğlu et al. 2017; Walters 2018). See Epple, Romano, and Zimmer (2015) for a summary of the existing evidence on charter schools.

[6] See Section IIIB for a detailed description of the variance ratio index of segregation.

expanding body of research has documented the benefits of school integration, yet the precise mechanisms are less clear (Reardon and Owens 2014). Analysis of the desegregation plans that followed the *Brown* ruling found reduced high school dropout rates for Black students (Guryan 2004; Reber 2010) as well as reductions in the probability of incarceration and increases in wages, employment, and health status (Johnson 2015). The eventual termination of desegregation orders subsequently led to short-term resegregation that resulted in higher dropout rates for Black and Hispanic students (Liebowitz 2017; Lutz 2011). Similarly, an examination of the impact of ending race-based bussing in Charlotte-Mecklenburg found that it increased racial inequality and led to negative effects on high school exams for White and minority students, lower graduation rates and college attendance for White students, and increases in crime for minority males (Billings, Deming, and Rockoff 2014; Vigdor 2011).

Economists have raised two primary mechanisms by which school integration might reduce racial disparities in student outcomes: by ensuring educational resources are more equitably available to all students and by reducing the direct negative impact of social isolation (Card and Rothstein 2007). Many recent empirical studies have raised the distribution of resources as a primary driver of the negative effect of segregation (Bergman 2018; Jackson, Johnson, and Persico 2016; Reardon, Kalogrides, and Shores 2019; Reber 2010). Additionally, an emerging empirical literature suggests that intergroup exposure has direct benefits, ranging from reducing the stigma associated from segregated schools to cultivating political tolerance (Billings, Chyn, and Haggag 2021). Indeed, the *Brown* decision rejected the notion that equalizing resources alone were the main impetus for integration when they rejected the "separate but equal" doctrine. For these reasons, we examine both relative and absolute measures of segregation in our empirical examination.

II. Data

The main data source in our study is the National Center of Education Statistics' (NCES) Common Core of Data (CCD), an enumeration of public school enrollment head counts by grade level and race/ethnicity, school type, and the latitude and longitude of schools' geographic locations (US Department of Education 2020).[7] We use geographic information system (GIS) procedures to match school locations to different geographies: geographic school districts, municipalities, counties, and metropolitan areas. We treat each of these geographies as distinct definitions of school systems. This step is important, as it allows us to geographically locate charter schools in the public school systems that they impact.[8] For school districts, we use the 2015 definition of school district boundary maps from NCES' Education and Geographic Estimates (EDGE) (US Department of Education 2015). For metropolitan areas, we use US Census Bureau Topologically Integrated Geographic

[7] To ensure the accuracy of the school location data, we conduct a geocoding procedure using school address data, which are more complete than location data in the CCD. This makes a difference, especially for early years (approximately 1998–2009) in which location data are not available, missing, or otherwise low quality.

[8] We investigate the potential for spillover effects across neighboring jurisdictions in Section IVB.

Encoding and Referencing (TIGER)/Line 2010 definitions of core-based statistical areas, focusing only on metropolitan statistical areas. We also match schools to census places, the census's definition of "municipalities"—incorporated cities and towns and unincorporated concentrations of population in the United States (US. Census Bureau 2010). Finally, we merge school location data with 2010 census tract demographics (Manson et al. 2021).

We structure the data as a panel of school systems over the period 1998–2018, observed separately for each grade level between kindergarten and 12th grade (K-12).[9] We stack these grade-specific panels to obtain the dataset we use for estimation, which features observations at the system-grade-year level. We make some school-level restrictions to the analysis sample, dropping schools that are closed or inactive, devoted to special programs, serving only kindergarten or lower, providing only adult education, and not located in US mainland states.[10] We drop schools with missing enrollment counts by race, an issue that is more prevalent in some states than others during the early years of the data. We also make restrictions to the sample of school systems, dropping system-grades that have only one school at any point during the sample period, since segregation is not well defined in these cases. In addition, we drop systems that are observed only for a single grade or year after the baseline sample restrictions.

Our final analytic sample includes 4 distinct stacked panels distinguished by geographic scale: (1) school districts ($n = 5{,}325$), (2) municipalities ($n = 5{,}610$), (3) counties ($n = 2{,}741$), and (4) metropolitan areas ($n = 330$), observed for grades K-12 across 1998–2018. The total number of observations in these panels ranges approximately between 85,000 and 800,000.[11] It is also worth noting that the CCD charter indicator is often missing during the early years of our estimation sample (about 1998–2003). Our main results are insensitive to the removal of the early years of data.

A. *Descriptive Statistics*

Nationally, charter schools have increased their share of total enrollment in recent decades. Between 1998 and 2018, Figure 1 shows that on average, K-12 students attended a district in which the charter share of enrollment grew from 0.3 percent to 6.9 percent. This national trend includes many districts that never see charter school openings, so it understates the average charter share growth among districts with charter entry, which rose from 0.5 percent to 11.6 percent over the same period. For our research question and design, it is important to keep in mind that these

[9] The first year in our panel is 1998 because it is the first year that the charter school indicator is available in the data.

[10] We include virtual schools in our analysis. Our results are robust to excluding virtual schools from the analysis.

[11] The stacked panels are highly unbalanced. For example, in the school district sample, only 17 percent of districts have an observation for every single year in 1998–2018 and grade in K-12. The main reason is that most school districts have grade levels that don't meet our baseline sample criteria: at least two schools need to serve a given grade. For districts with a single high school, for instance, we drop high school grades but still keep their middle and elementary schools as long as they meet the other criteria.

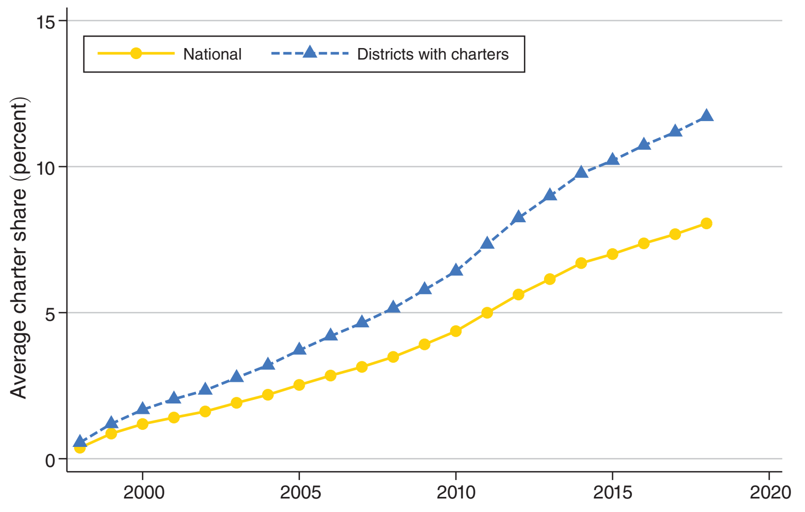

FIGURE 1. AVERAGE CHARTER SHARE OF SCHOOL DISTRICT ENROLLMENT 1998–2018

Notes: Observations are at the district-grade level and weighted by enrollment. Districts with charters are those that have consistent charter school enrollment during the entire sample period.

national trends mask considerable heterogeneity in charter growth between districts and across grade levels within districts.

Charter schools tend to serve student populations that overrepresent Black and Hispanic students. Compared to traditional public schools, Table 1 shows that in 2010, charters in all grade levels enrolled relatively higher proportions of Black students and lower proportions of White students. Charter schools also tend to enroll higher proportions of Hispanic students in middle and high school, but less so in elementary grades. The bottom panels in Table 1 show that these enrollment patterns are largely reflective of the neighborhoods in which charter schools open. At all grade levels, charter schools are located in census tracts with relatively higher shares of Black and Hispanic residents and lower shares of White residents. These patterns are consistent with research demonstrating that schools tend to closely reflect neighborhood compositions (Monarrez 2020; Whitehurst et al. 2017) and research showing that charter schools tend to be more racially isolated for Black and Hispanic students (Frankenberg, Siegle-Hawley, and Wang 2010). The bottom rows of Table 1 show that charter schools also tend to be located in tracts with relatively lower median household income (measured in 2010 USD) and adult educational attainment.

B. *Measuring Segregation*

A notable takeaway from the segregation literature is that different methods of measuring segregation can lead to different conclusions (Reardon and Owens 2014). The two most common approaches to segregation measurement are absolute measures and relative measures (Clotfelter et al. 2018). Absolute measures describe the extent to which students from one demographic group are exposed to another

Table 1—Characteristics of Charter Schools and Traditional Public Schools (TPS) in 2010

	Primary		Middle		High	
	Charter	TPS	Charter	TPS	Charter	TPS
School Characteristics						
Total enrollment	341.12	489.90	260.54	646.31	283.54	1,082.28
Percent Black	0.31	0.16	0.34	0.16	0.22	0.18
Percent Hispanic	0.24	0.23	0.33	0.20	0.30	0.17
Percent White	0.36	0.52	0.26	0.56	0.39	0.58
Census Tract Characteristics						
Percent Black	0.22	0.13	0.27	0.12	0.16	0.13
Percent Hispanic	0.24	0.17	0.28	0.15	0.25	0.14
Percent White	0.48	0.63	0.38	0.66	0.51	0.67
Percent adults with college	0.24	0.26	0.24	0.26	0.24	0.25
Median income (2010 USD)	47,322.67	57,382.39	44,772.23	57,185.64	46,025.97	54,428.29
Observations	2,113	42,249	521	13,288	958	11,259

Notes: Source of enrollment data is the 2010–2011 school year NCES CCD. 2010 census tract data for schools are obtained by matching school location to 2010 census tract geography and demographics using GIS software. Median income refers to median household income in 2010 US dollars.

group within individual schools. Common absolute measures are the isolation and exposure indices, which measure the average share of minority students' classmates that are from varying ethnic backgrounds. Other absolute approaches define segregated schools as those with high proportions of minority students. Some researchers have adopted the term "hypersegregated" to describe schools for which enrollment is comprised of a 90 percent or higher share of underrepresented minorities, showing that charter schools are more likely to be hypersegregated (Orfield et al. 2016).

While useful, a drawback of absolute measures is that they reflect both racial stratification across schools and the underlying racial composition of the school system. Schools in high-minority areas may be labeled hypersegregated simply for reflecting the neighborhood from which they draw students. Over time a school system may appear to be increasingly segregated simply because of increases in the local minority population (Caetano and Maheshri 2017). Recent claims in the media that schools have resegregated tend to rely on absolute measures, which do not account for the fact that White students make up an increasingly smaller share of all students in the United States (Fiel 2013; Harris and Curtis 2018). These issues complicate comparisons of absolute segregation across time and place.

Relative segregation measures focus on sorting by describing how evenly the population of a given group of students is distributed across schools, adjusting for system-wide demographic composition. This makes relative segregation measures more comparable across different locations and over time. We use the variance ratio index (also known as "eta-squared"), a relative measure that has been widely used by economists to characterize sorting (Kremer and Maskin 1996; Graham 2018). The variance ratio builds from the isolation index but includes a simple adjustment for system-wide composition, defined as

$$(1)\quad \textit{Variance Ratio} = \frac{E\left[q_{sj} \mid M = 1\right] - Q_j}{1 - Q_j} = E\left[q_{sj} \mid M = 1\right] - E\left[q_{sj} \mid M = 0\right],$$

where q_{sj} is the fraction of students in school s and school system j that are in the underrepresented minority (M) group.[12] The isolation index, the average minority share of peers q_{sj} experienced by the minority population, can be written as the conditional expectation $E\left[q_{sj} | M = 1\right]$, where M is an indicator for minority students and the expectation is taken over the distribution of students. Varying q_{sj} to be other groups' enrollment share defines a group-specific exposure index.

The intuition of the variance ratio as an adjusted isolation index is straightforward. In a perfectly integrated system, all schools would have a composition equal to Q_j, the system-wide minority share of the population. But in a perfectly segregated school system, minority students are only exposed to themselves, so the isolation index would equal one. Therefore, the denominator in (1) gives the total range of variation in isolation for district j, given its composition Q_j. The numerator is the system's racial isolation minus Q_j, interpreted as *excess isolation* relative to the perfect integration level of exposure to minorities. The variance ratio thus measures existing excess isolation expressed as a fraction of the excess isolation of a complete segregation benchmark. The index also coincides with the second equality in (1), the difference in mean exposure to minorities between minority and nonminority students. In other words, the variance ratio measures the relative likelihood that students from the minority group have minority schoolmates. Indeed, this expression is the OLS slope coefficient of a student-level regression of q_{sj} on a minority student indicator, telling us how predictive a student's race is of the race of her schoolmates.

Several studies have shown that the variance ratio index arises naturally in the econometric analysis of the racial achievement gap. In a linear model in which student outcomes are partly generated by school resources and school racial composition is correlated with school resources, the variance ratio is the natural metric linking inequity in schooling caused by segregation and mean racial gaps in outcomes (Rothstein 2007; Reardon and Owens 2014; Graham 2018). As such, our main results report charter schools' impact on racial sorting using the variance ratio. We also conduct a parallel analysis in the online Appendix using another common relative measure of segregation, the index of dissimilarity (Table A4 in the online Appendix).

An additional important consideration when measuring segregation is the "segregation of whom?" question. In the past, much of the literature focused on the separation of Black and White students, for good reason (Johnson and Nazaryan 2019). But in today's diverse student population, this may seem more arbitrary. The recent economics literature measures the segregation of underrepresented minority (Black and Hispanic) students from others (Card, Mas, and Rothstein 2008; Caetano and Maheshri 2017).[13] Recent work also uses indices of multigroup entropy, which measure sorting for multiple groups simultaneously to account for the country's growing diversity (Iceland 2004).[14] We take a comprehensive approach, focusing our main

[12] The index includes charter schools and every other type of public school that reports enrollment counts to the CCD. Our research question requires this, as we want to study impacts on imbalance across the public schools of the entire public school system in a given locality.

[13] Note that the segregation of of Black and Hispanic children may be considerably higher than both the segregation of Black students and the segregation of Hispanic students measured separately. By combining these two groups, we impose an assumption that greater exposure between these two groups does not amount to greater school integration.

[14] We provide baseline impact estimates on Theil's H index of multigroup entropy in online Appendix Table A7.

results on the relative segregation of Black and Hispanic students for comparability to the recent literature, but also looking at sorting and absolute intergroup exposure indices for Black, Hispanic, Asian, and White students separately.

A final relevant measurement issue is that segregation indices are mechanically sensitive to the number of schools per capita in the school system. Holding constant school system demographics and sorting dynamics, a school system with more schools has a wider scope for segregation than one with fewer schools. In online Appendix Figure A1, we carry out simulations to show the extent to which charter school expansion could impact segregation simply due to the fact that the same student population is sorting into a larger number of schools. The simulations demonstrate that the mechanical effect of number of schools on segregation is minimal in large districts but might be more worrisome in districts with a smaller population. Thus, our models control for number of schools to pick up these mechanical impacts.[15]

C. *National Trends in School Segregation*

Figure 2 reports trends in average school segregation nationally, defined across four geographies: school districts, municipalities, counties, and metropolitan areas. Across geographies, the dynamics of average school segregation tell a similar story. Regardless of which racial or ethnic grouping one focuses on, national trends in school segregation have been essentially flat over the last 20 years. An exception is the segregation of Black and White students in metropolitan areas, which has declined considerably during this period. While in the early 2000s, Black students were more segregated than Hispanic students in metropolitan areas, today segregation levels for these groups have converged. White segregation in metropolitan areas declined in a parallel manner to that of Black students, but White students continue to be the most segregated racial group. Asian students are the only group that has experienced increases in segregation. The relatively flat trend in average segregation is consistent with similar analyses using measures of unevenness, which show flat to declining trends in segregation over the past two decades (Reardon and Owens 2014; Whitehurst et al. 2017).

While trends are similar, segregation levels vary considerably with geographic scale. Measured at the school district level, the average variance ratio is slightly above 10 in most years of the data. School segregation levels for municipalities are similar to those of districts. In contrast, the mean variance ratio of metropolitan areas jumps up to about 35. County level segregation is in the middle, about 28 throughout the sample. These patterns establish that schools are more severely segregated across metropolitan areas than they are within school districts, as has been

[15] To be certain that mechanical effects do not drive our main estimates, we conducted falsification tests that estimate charter impacts on segregation assuming that charter schools randomly draw their enrollment from noncharter schools (with replacement) but are otherwise equally numerous and populous as in the real data; see the bottom of Section IVA and Table A1 in the online Appendix. We also perform tests of treatment effect heterogeneity across school districts with different population sizes, shown in Table A5 in the online Appendix. Our estimate of the impact of charters on segregation is remarkably similar across quartiles of the distribution of total district enrollment.

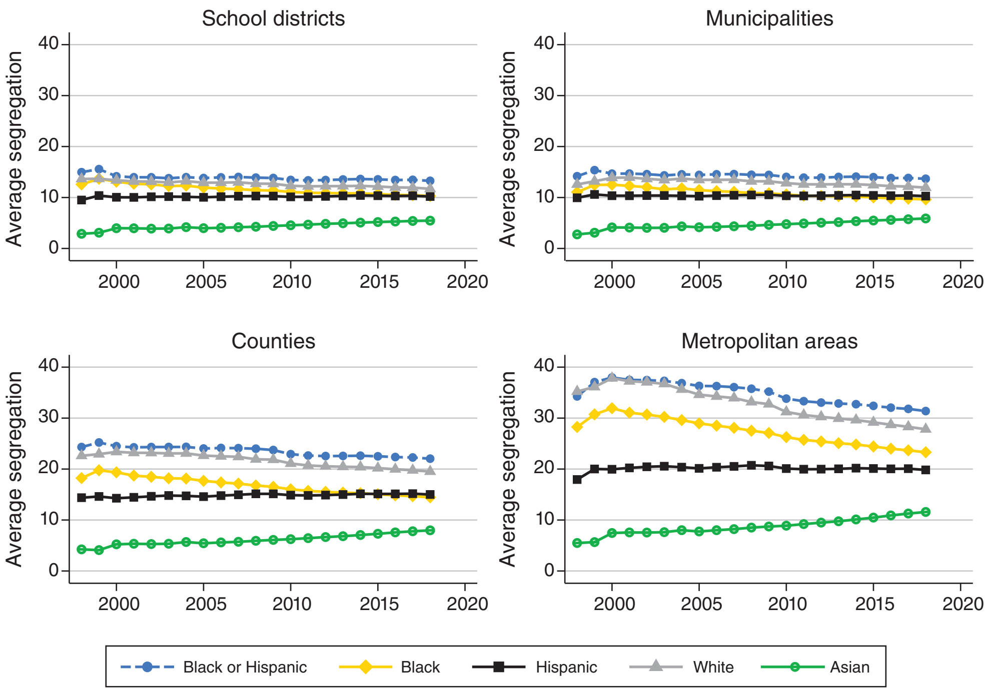

FIGURE 2. TRENDS IN SCHOOL SEGREGATION ACROSS GEOGRAPHIES AND RACIAL/ETHNIC GROUPS

Notes: Observations are at the school-system-by-grade level and weighted by enrollment. Segregation is defined with the variance ratio index (equation (1) in the text). See notes in Table 2 for geographic definitions.

documented in earlier research (Clotfelter 1999). Geographic differences will have important implications for our evaluation, as we uncover countervailing forces that hinge on the geographic level of aggregation in our analysis.

III. Empirical Framework

Theory suggests that the expansion of charter schools can impact school segregation via at least three distinct mechanisms. First, they provide an outside option from traditional public schools, expanding families' choice set and partly removing the deterministic link between schools and neighborhoods.[16] The second mechanism, perhaps the most important, is parents' school selection behavior, which is determined by charter schools' individual reputations (and missions) relative to district schools, as well as preferences over peers, commuting burdens, and other factors. Finally, a third channel is linked to dynamic sorting effects initiated by families that leave neighborhood schools for charters. These dynamics could evolve and reinforce themselves over time, triggering tipping point effects (Schelling 1972; Caetano and Maheshri 2017). In addition, there is evidence that charter school selection behavior

[16] Studies examining school commuters demonstrate that charter schools weaken the link between residence and school assignment (Cowen et al. 2018).

varies considerably between urban and suburban areas. Studies of the Boston area charter sector have shown that while urban charters enroll predominantly underrepresented minority students, suburban charter enrollment demographics are similar to those of traditional public schools (Angrist, Pathak, and Walters 2013). This suggests that parental preferences over charters may vary by place and socioeconomic status.

This theoretical ambiguity motivates our reduced form empirical analysis. We are interested in determining the causal impact of charter schools on system-wide student sorting, holding all else equal. We parametrize treatment dosage as the charter share of public school enrollment in a given geography, making the treatment variable continuous and bounded between 0 and 100. The charter share of enrollment is a natural and commonly used metric for the relative importance of the charter school sector, which is amenable to comparisons across place and time.

We identify the impact of the charter share of enrollment on segregation using the following econometric specification:

$$Y_{igt} = \beta E_{igt} + X'_{igt}\Gamma + \tau_{ig} + \delta_{it} + \gamma_{s(i)gt} + \epsilon_{igt}, \tag{2}$$

where Y_{igt} is the segregation of school system i in grade g for school year t; E_{igt} is the percent of school system enrollment going to charter schools in that grade and year; and X_{igt} is a vector of characteristics that vary at the system-grade-year level, including log of total enrollment, the fraction of students from a given racial group, and the number of schools serving a given system-grade-year. The model also includes system-by-grade fixed effects τ_{ig}, system-by-year fixed effects δ_{it}, and state-by-grade-by-year fixed effects $\gamma_{s(i)gt}$. Finally, ϵ_{igt} is a structural residual that may threaten the validity of the assumptions necessary to interpret β causally if correlated with E_{igt}.

This specification can be interpreted as a triple differences model, with identification relying primarily on the inclusion of system-year fixed effects but also accounting for state-year-grade and system-grade variation.[17] The system-year effects δ_{it} serve an important role because they account for unobserved time-varying shocks at the school system level that have equal impact on segregation across all grade levels. For instance, we can rule out that our estimates are driven by districts enacting a policy that applies to all grade levels and impacts segregation and whose timing coincides with the rise of the charter school sector in this locality. Additionally, system-year effects flexibly absorb the impact that uneven urban change and gentrification may have on stratification patterns. Nonetheless, by themselves, the system-year fixed effects cannot account for important between-grade differences in the determinants of school segregation.

The inclusion of system-grade fixed effects τ_{ig} restricts comparisons to the same grade level within a single school system, which has a twofold use in the case for causal identification. First, they difference out time-fixed variation in segregation

[17] The analytic sample drops school districts that administer a single school and those that are singletons with respect to the fixed effect structure of equation (2); see Table 2 for a summary.

across school grade levels, which has been documented empirically on a national scale but may vary by place (Greenberg and Monarrez 2019). Second, both the system-grade and system-year effects get rid of time-fixed confounding variation in segregation across the geography of the country. For instance, school segregation is higher in southern school systems than in western ones. Charter penetration also happens to be higher in the West than in the South, but we wouldn't want to attribute this correlation to the causal effect of the charter sector. Finally, the state-grade-year effects $\gamma_{s(i)gt}$ ensure that our estimates flexibly account for differences in segregation varying by grade, year, and state, which could be driven by state-specific cohort effects like the secular growth of Hispanic enrollment in certain areas of the country over recent decades.

Intuitively, equation (2) identifies average effects by aggregating variation in charter enrollment *dynamics* across grade levels within each school system and year. For a given school system, if in year t the charter share of grade g enrollment grew relatively more than in other grades and there was a corresponding relative increase in the change rate of segregation, our model would attribute this to a causal association between charter sector growth and increased segregation. Our national estimate of the average effect β can be interpreted as a weighted average of these types of adjusted comparisons within system-years across all school systems fitting our analysis requirements over the period 1998–2018.[18]

A. *Robustness Tests*

The empirical framework described above helps rule out a large number of confounding factors in our efforts to estimate the causal effect of charter schools on segregation. But it does not rule out all of them. One may worry that there could be unobserved factors varying at the system-grade-year level that drive changes in both charter school enrollment and segregation. Another potential threat is that segregation dynamics themselves may cause student flows in and out of existing charter schools, impacting the charter share of enrollment and generating a problem of reverse causality for our regression model. This could happen if families' decisions on charter schools are driven in part by within-grade segregation dynamics in the public school system.

We carry out a series of tests to ensure that these potential explanations do not drive our main findings. First, we assess the existence of preexisting trends in the outcome using the following distributed lags specification:

$$(3) \qquad Y_{igt} = \sum_{l=-5}^{5} \beta_l E_{ig,t+l} + X'_{igt}\Gamma + \tau_{ig} + \delta_{it} + \gamma_{s(i)gt} + \epsilon_{igt}.$$

Here, β_l captures the effect of l leads of charter percent of enrollment $E_{ig,t+l}$ on current segregation levels. In other words, this model tests whether future increases in the fraction of students enrolled at charters are predictive of current levels of district

[18] Since our strategy relies on the structure of the grade, year, and district effects, we present estimates of models that vary fixed effect structure in online Appendix Table A6, showing that our main conclusions do not rely heavily on the specific fixed effect structure of equation (2).

segregation, which would signify the presence of unobserved time-varying factors. These models also include lags of the charter share, allowing us to assess whether charters have a lagged effect on sorting, conditional on the contemporaneous charter share.

Our research design is based on the idea that outlining segregation dynamics by grade level and charter dosage in a given place and time can rule out the role of a multiplicity of confounders that are correlated with charter school presence at the national level. In this sense, the best source of identifying variation in our data is the charter school opening (and perhaps closing). Unfortunately, our setting is not ideal for a standard event study design. Multiple charters can open and close in a given system year, the level at which the outcome is measured, and the properties of event study models with multiple events per unit are not well understood.[19] Further, when charter schools open, they do so gradually, meaning that the "event onset" of charter schools "treating" a district is not discrete.

We circumvent these issues by developing a two-stage estimation procedure that leverages variation from charter school opening events as the source of identification. The first step in this procedure is to estimate the change in the charter share of enrollment that is attributable to the opening of charter schools and not the churn of student flows between existing schools in the district. To do so, we construct a school-by-year panel of charter schools and estimate standard event study models of total enrollment on years since school opening (Jacobson, LaLonde, and Sullivan 1993; Cohen et al. 2019). We estimate the following model separately by grade: $e_{sgt} = \sum_{k=1}^{10} \alpha_{kg} D_{sg}^{k} + \phi_{sg} + \psi_{tg} + \nu_{sgt}$, where e_{sgt} is the total grade g enrollment of charter school s in year t, D_{sg}^{k} are event study indicators for the number of years that have passed since the school's opening year, and ϕ_{sg} and ψ_{tg} are school and year effects. These models produce precise event study estimates of the average growth in enrollment associated with the opening of a charter, measured in number of students, presented in Figure 3. For the mean charter school, annual growth in enrollment is steep in the early years and then quickly levels off. Enrollment growth dynamics vary by grade in a predictable manner that is consistent with schools "rolling up" their enrollment with a gradual opening.[20]

We compute the charter enrollment fitted values $\hat{e}_{sgt}$ from the school-level event studies shown in Figure 3. Next, we aggregate these fitted values to the level of our outcome of interest, a system-grade-year panel:

$$(4) \qquad \hat{E}_{igt} = \frac{1}{N_{igt}} \sum_{s \in i} \hat{e}_{sgt} = \frac{1}{N_{igt}} \sum_{s \in i} \left(\sum_{k=1}^{10} \hat{\alpha}_{kg} D_{sg}^{k} + \hat{\phi}_{sg} + \hat{\psi}_{tg} \right),$$

[19] While different approaches have been proposed to handle multiple events in an event study framework, the literature on best practices in this realm is still inconclusive (Sandler and Sandler 2013). Because in many cases we would be dealing with dozens (if not hundreds) of events for a single treated unit, we opt for a different route.

[20] Online Appendix Figure A2 shows distributions summarizing the variation in opening and closure events that underlie the enrollment growth event study estimates. Charter openings range around 500 per year nationally; closures are growing over time from fewer than 100 to more than 400 annually.

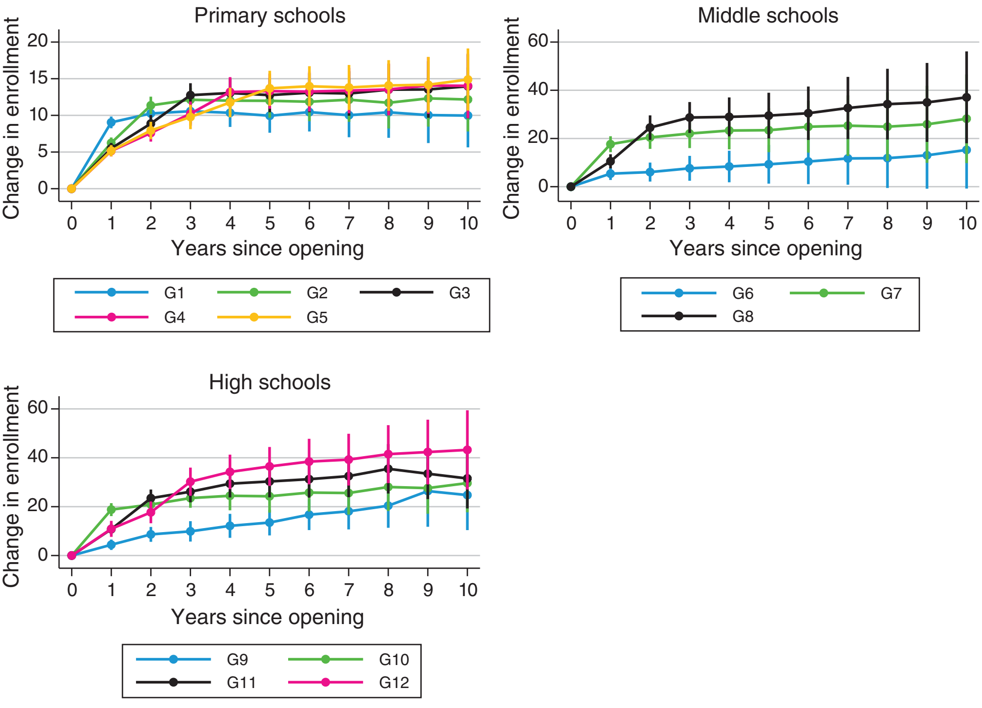

Figure 3. Event Study Estimates of Typical Enrollment Growth at Opening Charter Schools, by Grade

Notes: OLS coefficient estimates (and 95 percent confidence intervals) from an event study model of charter school enrollment as a function of years since opening, using a school-year panel of all charter schools in existence between 1998 and 2018. The model is estimated separately by grade, using the specification $e_{sgt} = \sum_{k=1}^{10} \alpha_k D_{sg}^k + \phi_{sg} + \varphi_{tg} + \nu_{sgt}$, where e_{sgt} is the total grade g enrollment of charter school s in year t, D_{sg}^k are event study indicators for the number of years that have passed since the school's opening year, and ϕ_{sg} and φ_{tg} are school and year effects. Standard errors are clustered at the school level.

where N_{igt} is total district enrollment by grade and year, $\left(\left\{\hat{\alpha}_{kg}\right\}_{k=1}^{10}, \hat{\phi}_{sg}, \hat{\psi}_{tg}\right)$ are the estimated event study coefficients, and D_{sg}^k are school-level indicators of the number of years the charter school has been open. The predicted charter share estimates capture variation in the charter share that is attributable to three sources: the aggregation of the event study coefficients given the school's opening date and age, the sum of the school-grade effects, and the sum of the grade-year effects.

The first component is determined by the impact of the number of years that charter schools have been open on charter enrollment, given the typical growth of new charter schools. The second is the sum of school-grade effects, capturing differences in the overall size of charter schools that are opening. One may be concerned that the actual system-by-grade-by-year charter school share could be endogenous to this component because parents can see their student's cohort and adjust, but the school-grade effects are constant over all years that the school is present, and the year variation only enters based on the presumably exogenous year of entry, which is unlikely to be affected by the specific cohort circumstances for the year that the school entered that grade. The third source of variation is determined by the sum of grade-year effects in the event study model, capturing the secular increase in charter enrollment over time. While the grade-year effects are effectively multiplied by the number of charter schools in the system in a given year, there is little reason to be concerned that system-by-year-by-grade variation would be correlated with the system-by-cohort circumstances for a given grade and year that might lead to endogeneity of the actual charter enrollment share.

We use the predicted share of charter enrollment defined in equation (4) as an exogenous instrument for the triple differences models (equation (2)). The logic of this instrument is that it is purged from "bad" variation in the charter share of enrollment that is driven by student flows in and out of charter schools that could be correlated with unobserved determinants of district segregation. The exclusion restriction assumption in the IV models requires that the charter school opening events impact segregation exclusively via the charter share of district-grade enrollment, after partialing out flexible fixed effects. With the presence of controls, the IV models rule out the vast majority of threats to the identification strategy. We first report the OLS estimates of (2), given the ease of their interpretation, when we confirm these with a two-stage least squares (2SLS) specification and conduct Hausman-type tests of equality between the OLS and IV impact estimates.

Additionally, we conduct ae conduct a randomization-based falsification test using a transformed outcome variable. For each charter school in the data, we draw a binomial random variable with number of trials equal to the school's actual total enrollment in a given grade-year. The likelihood of "success" in the trial is equal to a racial group's share of enrollment in the noncharter schools in the charter's school system. This is equivalent to randomly drawing students with replacement from the non-charter-school population. We compute counterfactual segregation indices using these simulated charter counts and estimate our baseline triple differences model (equation (2)). By construction, the charter share cannot have a segregating effect for these simulated outcomes.

Because the variance ratio index considers a school that is racially representative of the district to be "integrated" (equation (1)), the simulated charters in our randomized falsification tests will tend to push segregation toward zero. In other words, if charter school enrollment is perfectly representative of noncharter schools (up to randomization error), then the estimate of β in equation (2) should be negative. The estimates in Table A1 in the online Appendix support this conjecture. The coefficients on charter percent in these models tend to be small and negative, confirming that were charter school students randomly drawn from the district school population, they would have the impact of lowering segregation. In Section IV, we show that this is not the case when using actual data on charter enrollment breakdowns.[21]

As a final robustness test, we estimate a series of placebo tests, presented in detail in the online Appendix (Table A2). The placebo tests look for cross-grade spillover effects of charter sector growth, assessing whether charter presence in one grade is predictive of segregation dynamics in other grades. The presence of certain spillover effect patterns may be indicative of unobserved confounders that could threaten the causal interpretation of our main estimates. We should be suspicious of the presence

[21] As discussed in Section IIIB, this falsification test also ensures that our estimates of β are not positive simply due to the mechanical impact of adding more schools on the segregation index.

of correlated unobservables if charters in lower grades are linked to segregation in higher grades. But a converse pattern of spillover effects from higher to lower grades would not be as concerning, as it could be explained by preemptive behavior on the part of households. We test for cross-grade spillover effects across the matrix of possible interactions between the 13 school grade levels in K-12. Our estimates of cross-grade spillover effects are broadly consistent with a causal interpretation of our main estimates, and we find limited evidence of preemptive segregation dynamics.

IV. Results

Table 2 summarizes our estimation samples, showing the 2018 mean of key analysis variables across four geographic levels, weighted by total enrollment. The first four columns show that school districts and municipalities are of similar size and hold similar attributes, although they aren't identical. Students attend schools in districts and municipalities administering 98 and 92 schools on average. Of these, about 10 to 11 percent are charter schools, although they tend to have lower enrollment than other public schools, enrolling about 8 percent of the student population. Our charter enrollment growth models (Figure 3) predict that the average charter share is slightly lower, about 6 to 7 percent.

Students attend districts and municipalities in which almost half (48 percent) of the student body is Black or Hispanic and about 40 percent is White. School segregation is similar whether it is measured at the district or municipality level. White students are the most isolated individual group, second in isolation only to Black and Hispanic students grouped together. These patterns highlight the importance of the "segregated from whom?" question when measuring segregation and serve as a preview of our analysis, which is conducted separately across racial and ethnic groups. Our main estimates focus on the grouping of Black and Hispanic students as an underrepresented minority group. This choice makes our work more comparable to the existing literature.

As we showed in Figure 2, patterns for larger geographies—counties and metropolitan areas—highlight that school segregation is more severe as one increases the geographic scale.[22] Metropolitan areas do not fully cover the United States, defined only in urban centers, although 91 percent of public school students nationwide attend school in a metro area. Students in metro areas have about 50,000 peers in a given grade, attending about 880 schools. The average charter share of enrollment is similar to that of districts, 7.4 percent. Further, metro areas and smaller geographies are similar in terms of their student-weighted average racial composition. Nonetheless, they differ markedly in terms of segregation levels. Metro area school segregation, regardless of racial or ethnic group, tends to be twice as severe as district and municipal segregation and about 50 percent larger than county segregation.

[22] In some instances, county and school district geographies coincide. This is common, but not ubiquitous, in the southern United States. For instance, counties and districts coincide in Florida but not in Louisiana or Kentucky. Generally, district geographies tend to be smaller than counties.

Table 2—2018 Summary Statistics of Estimation Sample, by Geography

	School districts		Municipalities		Counties		Metropolitan areas	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Population								
Number of schools	97.8	(182.9)	92.2	(176.3)	252.1	(403.1)	878.3	(860.2)
Enrollment (1,000s)	6.3	(13.8)	5.9	(13.9)	13.8	(21.9)	50.0	(54.0)
Charter schools								
Number of charters	18.0	(46.6)	17.5	(43.4)	36.9	(78.5)	93.3	(104.5)
Percent of system	10.4	(12.1)	10.7	(12.8)	8.9	(9.4)	10.0	(7.4)
Enrollment (1,000s)	0.9	(2.3)	0.8	(2.2)	1.5	(3.3)	4.1	(5.0)
Percent of system	8.1	(11.6)	8.3	(12.4)	6.7	(8.2)	7.4	(5.9)
Predicted percent	6.3	(10.1)	6.5	(10.8)	5.3	(7.4)	5.7	(5.4)
Racial comp. (percent)								
Percent Black	17.3	(18.2)	17.1	(18.8)	15.0	(15.2)	15.9	(11.6)
Percent Hispanic	30.4	(24.4)	30.9	(24.5)	27.5	(21.7)	29.5	(20.6)
Percent Asian	6.0	(8.3)	6.4	(8.6)	5.5	(6.2)	6.1	(5.1)
Percent White	41.1	(26.0)	40.4	(26.3)	46.7	(24.4)	43.5	(18.8)
Segregation								
Black or Hispanic								
Variance ratio	13.3	(12.5)	13.7	(13.6)	22.0	(14.7)	31.4	(12.7)
Dissimilarity	31.6	(17.0)	31.7	(18.3)	42.5	(14.9)	50.8	(10.8)
Isolation	53.3	(27.2)	53.8	(27.6)	53.6	(24.7)	62.2	(16.9)
Black								
Variance ratio	10.3	(13.8)	9.6	(13.6)	14.5	(15.2)	23.3	(16.0)
Dissimilarity	34.0	(18.3)	33.1	(18.9)	43.7	(15.3)	50.6	(11.7)
Isolation	24.3	(23.0)	23.7	(23.0)	25.8	(21.9)	34.3	(20.3)
Hispanic								
Variance ratio	10.2	(11.6)	10.3	(12.1)	15.0	(12.2)	19.8	(11.2)
Dissimilarity	28.8	(15.7)	28.3	(16.4)	36.8	(13.1)	41.8	(9.8)
Isolation	36.5	(25.8)	37.0	(26.3)	36.9	(24.4)	42.4	(22.0)
Asian								
Variance ratio	5.5	(7.5)	5.9	(8.1)	8.0	(8.5)	11.6	(8.8)
Dissimilarity	38.5	(19.3)	37.7	(19.7)	45.4	(14.6)	49.1	(10.1)
Isolation	11.1	(12.2)	11.8	(12.6)	12.8	(12.1)	16.7	(11.7)
White								
Variance ratio	11.7	(10.8)	11.9	(11.5)	19.5	(12.2)	27.8	(10.5)
Dissimilarity	31.3	(18.0)	31.6	(19.1)	40.7	(15.0)	48.5	(10.0)
Isolation	49.0	(22.7)	48.5	(23.0)	58.3	(19.4)	59.3	(15.8)
Unique systems	5,325		5,610		2,741		330	
Total observations	42,921		44,576		28,599		4,290	

Notes: Observations are at the school-system-grade level and weighted by total enrollment. Charter schools are matched to geographies using GIS procedures. School districts are defined as local education agencies with geographically defined jurisdictions. Municipalities are defined using 2010 US census place geographies, including all incorporated cities and townships as well as unincorporated populated areas. Both school districts and counties generate a full cover of the country's geography and schools, but census places and metropolitan areas do not. Metropolitan areas are defined according to 2010 US census core-based statistical area definitions. School systems with a single school serving a given grade are not included in the sample. School systems that are singleton in terms of the fixed effects in the regression model in equation (2) are also not included in this sample.

The odd-numbered columns of Table 3 present baseline estimates of the impact of a 1 p.p. increase in the charter enrollment share on school segregation. These are OLS estimates of equation (2), including system-year, system-grade, and state-grade-year effects. We estimate that a 1 p.p. increase in the charter share leads to between a 0.09 and 0.10 p.p. increase in the segregation of minority students in

Table 3—The Effect of Charter Schools on Segregation by Race/Ethnicity and Geography

	School districts		Municipalities		Counties		Metro areas	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Black or Hispanic								
Charter percent	0.094	0.093	0.078	0.089	0.103	0.113	0.059	0.082
	(0.009)	(0.012)	(0.010)	(0.012)	(0.019)	(0.024)	(0.026)	(0.031)
Black								
Charter percent	0.073	0.088	0.059	0.070	0.088	0.107	0.045	0.064
	(0.009)	(0.012)	(0.007)	(0.009)	(0.021)	(0.027)	(0.026)	(0.032)
Hispanic								
Charter percent	0.044	0.036	0.041	0.049	0.045	0.048	0.041	0.049
	(0.005)	(0.007)	(0.007)	(0.009)	(0.013)	(0.016)	(0.019)	(0.023)
Asian								
Charter percent	0.034	0.038	0.023	0.023	0.023	0.015	0.032	0.030
	(0.006)	(0.007)	(0.005)	(0.005)	(0.007)	(0.008)	(0.014)	(0.017)
White								
Charter percent	0.085	0.085	0.071	0.083	0.099	0.108	0.062	0.092
	(0.009)	(0.012)	(0.009)	(0.012)	(0.020)	(0.023)	(0.026)	(0.034)
Dependent variable mean	12.64		12.97		21.61		32.68	
Hausman endog. test (*p*-value)		0.973		0.027		0.247		0.027
System-year fixed effects	X	X	X	X	X	X	X	X
System-grade fixed effects	X	X	X	X	X	X	X	X
State-grade-year fixed effects	X	X	X	X	X	X	X	X
Covariates	X	X	X	X	X	X	X	X
Observations	831,042	831,042	847,078	847,078	554,162	554,162	86,212	86,212

Notes: Standard errors are clustered at the school system level in all models. Dependent variable means correspond to White segregation levels (approximately equal to mean Black or Hispanic segregation). Covariates varying at the system-grade-year level include log total enrollment, group's share of total enrollment, and total number of schools serving grade. See notes in Table 2 for the geographic definition of estimation samples. IV models use the predicted charter share of enrollment as the instrument, defined using the typical growth estimates shown in Figure 3. See equation (4) in the main text. The *p*-value of the Hausman (DWH) test of endogeneity (equality between OLS and IV coefficients) for the minority (Black or Hispanic) segregation specifications is reported.

school districts, municipalities, and counties.[23] In contrast, for metropolitan areas, the impact on minority segregation is lower and noisier, about 0.06 p.p. The discrepancy between metro areas and other geographies is a harbinger of many of the empirical results that follow.

The second and third panels of Table 3 show that impacts for Black student segregation are about 50 percent larger than impacts on Hispanic student segregation in school districts and counties. We estimate that if charter schools increase their share of enrollment by 1 p.p., then Black segregation would go up by about 0.07 to 0.1 p.p., while Hispanic segregation would increase by 0.04 to 0.05 p.p. Impacts for Asian students are somewhat smaller than that for Hispanic students, in the range

[23] In Table A6 in the online Appendix, we present estimates that vary the structure of the fixed effects, gradually adding each of the control variables to our preferred model in equation (2). Notably, the coefficient on charter percent is sensitive to the addition of controls up until the inclusion of state-by-year effects, after which the inclusion of more controls changes the estimates little, even in the IV estimates.

of 0.02–0.04 p.p., but still statistically significant across all geographies. For White students, the segregation impact of charter growth is roughly the same as the impact on minority segregation (the first panel) across all geographies. This is not surprising, since in many systems, the share of Black, Hispanic and White enrollment shares is roughy equal to one; this means that White segregation and minority segregation will be approximately equal by construction, especially in districts in which the population share of other groups is small.

To put the magnitude of the effect estimates in context, consider that for districts in 2018, minority students experienced average sorting levels of about 13 percent according to the variance ratio, meaning that minority students are 13 p.p. more likely than others to have a minority schoolmate. The mean charter percent of enrollment in school districts was near 8 percent in 2018. Our estimates suggest that on average, the presence of charters has caused an $8 \times 0.09 = 0.72$ p.p. increase in segregation, about 6 percent of mean segregation levels in districts. While the magnitude of this effect is modest, the implications may be more salient in localities with a rapidly growing presence of charter schools. Extrapolating the charter share to 20 percent of public school enrollment would lead to an increase in segregation of 14 percent relative to the nationwide average.

Another way of understanding the nature of charter schools' impact on school segregation is by examining impacts on absolute measures of intergroup exposure among different racial and ethnic groups (see Section IIIB). We do so by estimating models of equation (2) using the average exposure rate of group A to group B as the outcome. When the two groups coincide, measuring exposure of a group to itself, we obtain the isolation index. Thus, Table 4 presents impacts on the "exposure matrix" in school districts, informing us of charters' impact on the degree of exposure between any two of the major racial and ethnic groups in the country. For brevity, we focus our analysis of intergroup exposure effects at the school district level and delegate estimates at other levels of geography to the online Appendix (Table A8).

The first panel of Table 4 shows that charter schools led to an increase in Black student isolation caused by decreasing levels of exposure to Asian and White students. Specifically, a 10 p.p. increase in the charter share of enrollment is linked to about a 0.5 p.p. increase in isolation and a corresponding 0.3 p.p. and 0.1 p.p. decrease in exposure to White and Asian students, respectively. For Hispanic students, charter school growth has resulted in increased isolation and exposure to Black students and a corresponding decrease in exposure to White and Asian peers. Asian students experienced increased isolation and decreased exposure to Hispanic students. Finally, White students also find themselves in schools that are more White isolated. The increase in isolation for White students is mainly caused by decreasing exposure to Hispanic and Asian students.[24] Together, the results in Table 4 tell a rich story of exactly which student groups become increasingly segregated from

[24]The asymmetry of the intergroup exposure effects is telling of an interesting pattern of treatment effect heterogeneity. For instance, in Table 4, the effect on Black exposure to White students is negative and significant, while the estimated impact on White exposure to Black students is negative but statistically negligible. The estimates differ because intergroup exposure indices naturally give some schools more weight than others. The Black exposure indices use a Black enrollment weight, highlighting schools with large Black populations. The White exposure

TABLE 4—THE EFFECT OF CHARTER SCHOOLS ON ABSOLUTE INTERGROUP EXPOSURE IN SCHOOL DISTRICTS

	Black		Hispanic		Asian		White	
	OLS (1)	IV (2)	OLS (3)	IV (4)	OLS (5)	IV (6)	OLS (7)	IV (8)
Black								
Charter percent	0.048	0.058	−0.006	−0.005	−0.012	−0.015	−0.033	−0.059
	(0.006)	(0.008)	(0.007)	(0.009)	(0.005)	(0.006)	(0.009)	(0.013)
Hispanic								
Charter percent	0.023	0.023	0.028	0.026	−0.011	−0.014	−0.039	−0.052
	(0.011)	(0.013)	(0.003)	(0.004)	(0.004)	(0.005)	(0.012)	(0.015)
Asian								
Charter percent	0.008	0.007	−0.027	−0.024	0.030	0.033	−0.002	−0.020
	(0.010)	(0.012)	(0.008)	(0.010)	(0.005)	(0.006)	(0.012)	(0.016)
White								
Charter percent	−0.007	−0.015	−0.029	−0.028	−0.010	−0.015	0.047	0.048
	(0.008)	(0.010)	(0.007)	(0.009)	(0.005)	(0.007)	(0.006)	(0.007)
Dependent variable mean	14.09		21.6		5.71		54.79	
System-year fixed effects	X	X	X	X	X	X	X	X
System-grade fixed effects	X	X	X	X	X	X	X	X
State-grade-year fixed effects	X	X	X	X	X	X	X	X
Covariates	X	X	X	X	X	X	X	X
Observations	831,042	831,042	831,042	831,042	831,042	831,042	831,042	831,042

Notes: Standard errors are clustered at the school district level in all models. Covariates are log total enrollment, number of schools, and the enrollment share of the group. Average exposure of group A students to group B in the schools i of a given district-grade-year is given by $Exp_B^A = (1/P^A)\sum_i p_i^A \times (p_i^B/p_i)$, where P^A is group A's total population in the district-grade-year; p_i^A and p_i^B are group A and B total enrollment, respectively; and p_i is total enrollment at school i. Reported dependent variable means correspond to White student exposure rates.

other groups when the charter school sector grows. A notable pattern is that charters lead to increased separation between historically underserved groups (Black and Hispanic) and groups that tend to be more advantaged (White and Asian).

A. *Robustness Tests Results*

One may be worried that the effect estimates reported in Tables 3 and 4 could be biased by district-specific pretrends in segregation preceding the growth of the charter school sector. Figure 4 presents our estimates of equation (3), a generalization of the models in Table 3 including leads and lags of the charter share of enrollment. Across geographic samples, our models estimate precise zeros for leads of the charter share (negative event time). This establishes that past segregation dynamics are not predictive of future growth in the charter sector. Further, the estimates are highly robust to the addition of leads and lags of time-varying controls that are system-grade specific, including the log of total enrollment, racial population shares, and the number of schools serving a grade. This evidence supports the claim that there are no preexisting trends in segregation leading up to the growth of the

indices weight by White student population, highlighting a different set of schools. We investigate the effect heterogeneity driving the differences in these impacts in Section IVC.

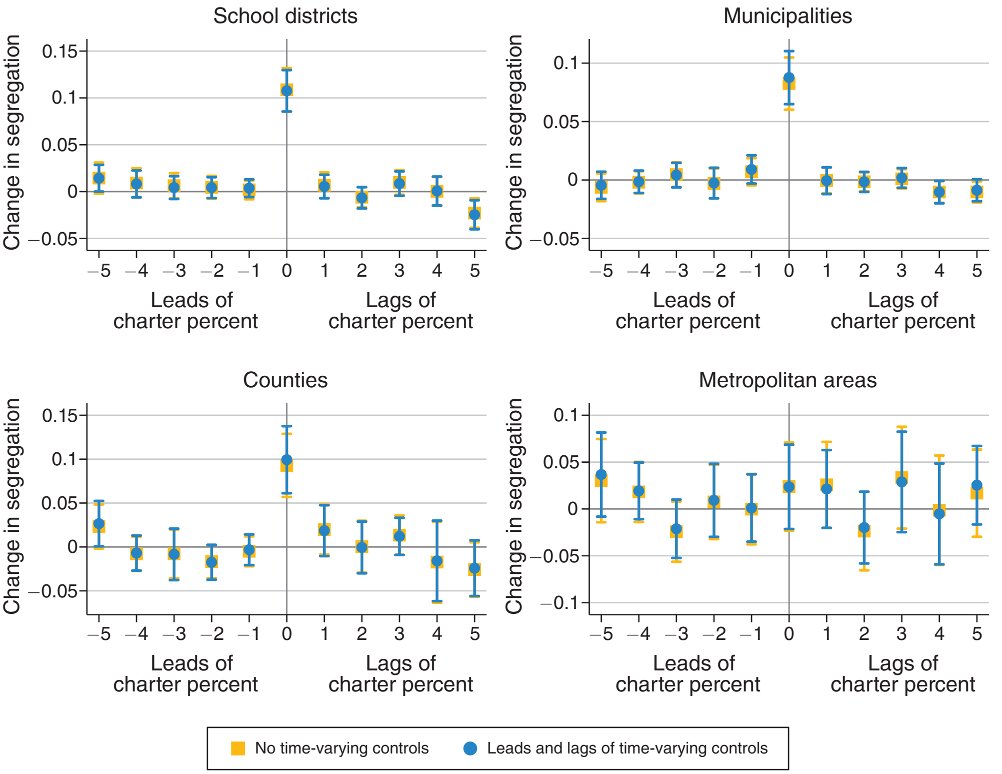

Figure 4. Distributed Lag Models of Black and Hispanic Segregation and Charter Percent of Enrollment, by Geography

Notes: Figure shows coefficient estimates from the distributed lag model defined in equation (3). Ninety-five percent confidence intervals are based on standard errors clustered at the school system level. Panels show estimates for different geographic samples; see Table 2 for detailed definitions and sample restrictions. Covariates varying at the system-grade-year level include log total enrollment, group's share of total enrollment, and total number of schools serving grade.

charter school sector. Moreover, there is little indication of a lagged effect of charters conditional on the contemporaneous share, as noted by insignificant coefficients for positive event time in the plots. In sum, the evidence in Figure 4 is consistent with charter growth causing immediate increases in school segregation.

The estimates reported in Figure 4 show consistent patterns for all geographic levels except for metropolitan areas. Our samples of school districts, municipalities, and counties all show precisely estimated contemporaneous effects and a lack of preexisting trends. Further, the estimates of the contemporaneous effects are of similar magnitude to the ones shown in Table 3, suggesting that omitting leads and lags of the charter share from the baseline models does not result in much omitted variable bias. In contrast, the metropolitan area models do not show significant contemporaneous effects, and the point estimate is attenuated (relative to Table 3, column 7) after the inclusion of leads and lags of the charter share. The exceptionality of metropolitan area effects in these models motivates an in-depth examination of charter effects in metro areas, presented in section IVB.

Thus far, the evidence suggests that charters have the effect of increasing the segregation of school systems and that this effect is not driven by preexisting trends in segregation. Nonetheless, there could be other identification threats. In particular, one may worry about the potential for reverse causality even conditional on our flexible controls. For instance, within a locality, grade, and year, it could be the case that segregation dynamics—say, increasing integration in district schools due to demographic change—could themselves cause increases in charter enrollment growth, especially if households use segregation dynamics in their decisions to enroll in charter schools. We remedy this concern by presenting IV estimates of equation (2) in the even-numbered columns of Table 3. The instrument in these models is the predicted share of charter school enrollment given the typical growth of charter schools after their opening (equation (4)). The exclusion restriction requires that the impact of charter opening events on segregation operates exclusively via the charter share of enrollment (conditional on flexible controls).[25]

The 2SLS models in Table 3 show that OLS and IV estimates of the effect of charter share on segregation are of similar magnitude across racial groups and geographies. Our baseline estimates for the segregation of Black and Hispanic students show that OLS and IV estimates differ by as little as 0.1 p.p. and up to 2.3 p.p. Similar patterns hold for the OLS and IV impacts on the segregation of individual racial groups. Taken as a whole, the evidence in Table 3 suggests that there is little difference between the OLS and IV estimates for minority segregation across most geographies. We formalize this claim with Durbin-Wu-Hausman (DWH) tests of equality between the IV and OLS coefficients for minority segregation (the outcome in the first panel of Table 3), presenting the *p*-values for these at the bottom of Table 3. Assuming the validity of the IV coefficients, we fail to reject the hypothesis that predicted charter percent is exogenous (after partialing out controls) for school district and county level segregation. However, the DWH test rejects equality between IV and OLS for segregation of municipalities and metropolitan areas. The IV coefficient is consistently larger than OLS in these models, with this difference being considerably more pronounced for metropolitan areas. For municipalities, the difference between OLS and IV is never larger than about 1 p.p. (12 percent of the OLS estimate), but for metro areas, the IV is consistently about 2–3 p.p. (about 33 percent) larger than OLS. Our interpretation of these patterns is that the rejection of the DWH test in metropolitan areas is more economically meaningful than for municipalities, motivating a more in-depth analysis of stratification dynamics at the metro area level (Section IVB).

The IV results establish that there should be little worry of endogeneity in our OLS within-system-year estimates of the impact of the charter share on racial segregation of smaller geographies like school districts. We thus retain the OLS estimates of the triple differences model in equation (2) as our preferred estimates, since they have a more straightforward interpretation. Nonetheless, for the majority

[25] In Figure A3 of the online Appendix, we present tests for preexisting trends in the IV, the predicted share of charter school enrollment given their opening dates and ages. The estimates confirm a lack of pretrends in the instrument, similar to patterns in Figure 4 (the coefficients in online Appendix Figure A3 are smaller, since they are reduced form estimates; see online Appendix Table A3).

of our models, we report the IV estimates as a useful check on the credibility of our claim to causality.[26]

As a final robustness check, we show in Table A2 of the online Appendix that the placebo tests are largely consistent with a causal interpretation for our main estimates. Charters are most predictive of the segregation of the grade they serve, and their impact of segregation by grade is of similar magnitude to our main estimates, with a few exceptions. The placebo tests show little indication of the existence of unobserved confounders or preemptive household behavior. With the exception of small between-grade correlations across high school grades, charter school growth in grade k is not predictive of segregation in grade $k' \neq k$. We direct readers to the online Appendix for a detailed description of the placebo tests.

Together, the absence of preexisting trends in segregation, the similarity of the OLS and IV point estimates, and the consistency of both the randomization-based falsification tests (online Appendix Table A1) and the placebo tests (online Appendix Table A2) provide convincing evidence that our models identify the causal effect of charter schools leading to higher racial segregation of public school systems. While the magnitude of this effect is modest, we can reject the null that charters do not racially segregate schools for any of the four largest racial and ethnic groups in the country. However, results for segregation at the metropolitan area level show odd patterns. While our baseline models still indicate that charters caused increases in metropolitan segregation, the distributed lag models are inconsistent with meaningful effects, and the IV estimates are considerably larger than OLS. Therefore, we now turn to a decomposition of the metropolitan area effects and dig deeper into the underlying mechanisms (Section IVB); we then turn to an analysis of effect heterogeneity (Section IVB).

B. *Between-District Segregation in Metropolitan Areas*

Another important consideration for the effect of charter schools on school segregation is their impact on sorting patterns across school district jurisdictions, which drive about two-thirds of metropolitan segregation levels (Clotfelter 2011). Typically, charter school enrollment takes place with little regard to district jurisdictional divisions.[27] Indeed, we don't directly observe school district jurisdiction identifiers for charter schools and are only able to measure charters' impact on school districts by linking them geographically to district boundaries, implicitly assuming that any school sorting takes place within the population encased

[26] Table A3 in the online Appendix presents first-stage and reduced form model estimates corresponding to the IV models in Table 3. These models help establish why our OLS and IV estimated effects are so similar. For instance, they show that for school districts, the reduced form effect is slightly more than half of the OLS impact, but the first-stage effect is itself about 0.60, translating into IV impact estimates of a similar magnitude to OLS.

[27] Given the complexity of various state laws governing charter schools, it is difficult to summarily categorize state enrollment policies. Roughly a third require prioritization of students living in the district where the charters are located, with additional spots filled through open enrollment. South Carolina's policy, for example, is arguably one of the strictest, limiting out-of-district enrollment to 20 percent of total enrollment unless both the sending and receiving school boards approve. At the other end of the spectrum, some states require that charters be open to all students, regardless of district (Education Commission of the States 2018).

within a jurisdiction. Due to the potential importance of a between-district sorting mechanism, we now develop a framework to test the impact of charters on racial stratification patterns across school district boundaries.

Using our stacked panel of school segregation by metro area, year, and grade, we decompose metropolitan area segregation into within- and between-district components following the methodology introduced by Clotfelter (1999). We compute between-district segregation by assuming a counterfactual scenario in which school districts are perfectly integrated, such that every school in their jurisdiction has a racial composition equal to district-wide composition. Computing metropolitan segregation under this counterfactual focuses on differences in the composition of entire districts, giving us a measure of the extent of racial stratification between school districts in a metropolitan area. We measure the within-district component of metropolitan segregation by taking a population weighted average of the variance ratio index of the metropolitan area's school districts.[28]

Table 5 presents our estimates of the effect of charter percent of enrollment on each component of metropolitan segregation using the triple differences specification in equation (2). Columns 1 and 2 replicate the OLS and IV results on total metro segregation in Table 3, which showed that the effect for metropolitan area segregation is lower relative to smaller geographies. Columns 3 and 4 report the impact of the metro charter share on the within-district component of metro segregation. The estimates are positive and statistically significant, in line with our previous results on the effect on school district segregation (Table 3).

Columns 5 and 6 of Table 5 report OLS and IV estimates of the impact of charters on *segregation between school districts*, where we find *negative* point estimates. This indicates that growth in the charter share leads to lower levels of between-district segregation. While our point estimates are negative across every major racial group, we can reject that these impacts are zero for minority, Black, and White stratification in the OLS models, and only for minority (and marginally for Black) segregation in the IV specifications. These findings suggest that charters cause decreases in the between-district component of metropolitan segregation for certain groups—for the mean metropolitan area, between-district minority segregation would rise by about 1 percent were charter schools abolished. Noticeably, in the majority of specifications, the sum of the within- and between-district effect estimates is approximately equal to the total effect on metropolitan segregation. We interpret this as another encouraging pattern, suggesting that our empirical strategy is effective at disentangling effects component-wise.

Taken together, the results in Table 5 establish that charter schools have counteracting effects on school segregation at the metropolitan area level. Charters increase segregation within school districts, but they also tend to diminish compositional imbalances between districts in the same metropolitan area. The effect on within-district segregation is larger; thus, our estimate on total metro segregation is still positive. One interpretation of these results is that charter schools echo the role of magnet schools during the court desegregation order era (Welch et al. 1987).

[28] The difference between total metropolitan segregation and between-district segregation has also been used as a measure of within-district segregation. Our estimates are similar when using this alternative measure.

Table 5—Decomposition of Charter Schools' Effect on the Segregation of Metropolitan Areas, by Race/Ethnicity

	Total MA segregation		Within district segregation		Between district segregation	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Black or Hispanic						
Charter percent	0.059	0.082	0.108	0.113	−0.053	−0.042
	(0.026)	(0.031)	(0.023)	(0.027)	(0.019)	(0.018)
Black						
Charter percent	0.045	0.064	0.067	0.070	−0.037	−0.025
	(0.026)	(0.032)	(0.018)	(0.021)	(0.017)	(0.015)
Hispanic						
Charter percent	0.041	0.049	0.061	0.065	−0.015	−0.012
	(0.019)	(0.023)	(0.019)	(0.024)	(0.011)	(0.013)
Asian						
Charter percent	0.032	0.030	0.032	0.033	−0.004	−0.008
	(0.014)	(0.017)	(0.010)	(0.014)	(0.008)	(0.008)
White						
Charter percent	0.062	0.092	0.104	0.117	−0.040	−0.025
	(0.026)	(0.034)	(0.026)	(0.031)	(0.018)	(0.020)
Dependent variable mean	32.68		10.65		24.15	
District-year fixed effects	X	X	X	X	X	X
District-grade fixed effects	X	X	X	X	X	X
State-grade-year fixed effects	X	X	X	X	X	X
Covariates	X	X	X	X	X	X
Observations	86,212	86,212	86,212	86,212	86,212	86,212

Notes: MA = metropolitan area. Standard errors are clustered at the metropolitan area level in all models. Dependent variable means correspond to White segregation levels (approximately equal to mean Black and Hispanic segregation). Within-district segregation is defined as average school district variance ratio index within the metropolitan area. Between district segregation is the variance ratio of the metro area using districts as the social unit of observations (as opposed to schools). Covariates varying at the system-grade-year level include log total enrollment, group's share of total enrollment, and total number of schools serving grade. See notes in Table 2 for the definition of the metropolitan area estimation sample. IV models use the predicted charter share of enrollment as the instrument, defined using the typical growth event study estimates shown in Figure 3. See equation (4) in the main text.

Magnet schools were introduced as a way of attracting White families to urban school districts in the hope of limiting White flight to suburban school districts. As such, magnets were intended to partly sacrifice the within-district integration objective in order to limit the more severe problem of growing segregation between districts. Charter schools today appear to have this type of dual effect: they alleviate certain compositional imbalances across district lines while simultaneously increasing segregation between schools.

The evidence of counteracting effects of charters on the segregation of metropolitan areas motivates additional analyses investigating potential mechanisms. Prior research has found that White flight during the era of court-ordered desegregation was higher in areas that are fragmented into a large number of school districts (Reber 2005). If charters have the effect of facilitating and enhancing between-district enrollment flows, such dynamics are likely to play out to a greater degree in cities

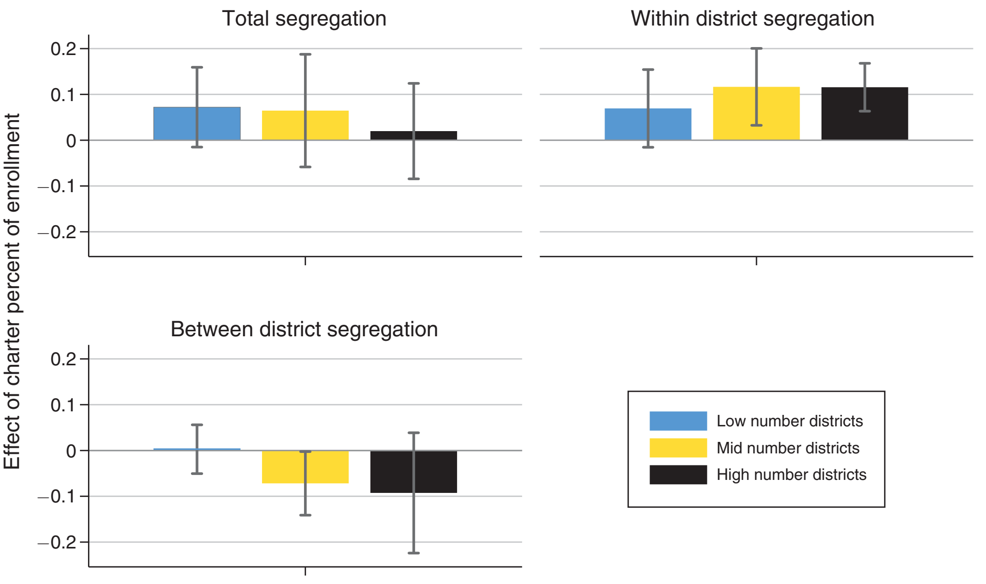

Figure 5. Within- and Between-District Charter Effects in Metro Areas, by Quantiles of School District Fragmentation

Notes: Standard errors are clustered at the metropolitan area level in all models. On average, metropolitan areas with low district fragmentation have 4.3 districts, midlevel fragmentation areas have 11.8, and high fragmentation areas have 53.9 school districts. Highly fragmented metropolitan areas include the most populous in the country, including New York, Los Angeles, Chicago, Dallas, and Houston. See Table 4 notes and Section IVB for model and estimation sample details.

with intense school district fragmentation. To examine this, we break our sample into three quantiles based on the total number of districts in a metro area.[29]

The first panel on the top left of Figure 5 shows that point estimates for total metropolitan segregation effects are positive in cities with fewer districts and close to zero in cities with high levels of school district fragmentation. In contrast, effects for the within-district component of segregation (reported in the top-right panel) are positive and at least marginally significant across the board, suggesting that the within-district segregation effect of charters is not particularly sensitive to school district fragmentation. On the other hand, the bottom-left panel reports effects on the between-district component of metropolitan segregation, showing that the between-district integration effect of charters, while imprecise, is increasingly more pronounced in areas that are fragmented into more local school districts. In areas with low levels of fragmentation, the between-district sorting impact is essentially zero, while in highly fragmented areas, it is close to -0.10, albeit noisily estimated.

A possible explanation for these findings is that highly fragmented metropolitan areas are those in which White animosity toward school integration was historically

[29] Using these categorizations, low-fragmentation metropolitan areas have an average of 4.3 districts, mid-fragmentation areas have 11.8, and high-fragmentation areas have 53.9 school districts. Highly fragmented metropolitan areas encompass the most populous in the country, including New York, Los Angeles, Chicago, Dallas, and Houston.

more intense. Following desegregation court orders, many cities saw increased White flight to suburban districts and more district secessions, leading to higher levels of fragmentation and between-district sorting (Reber 2005). If charter schools are bringing White students back to urban districts in these areas, but into White-isolated charter schools, this would explain the patterns observed in the data: higher within-district segregation and lower between-district segregation, especially in highly fragmented metro areas. We can shed some light on this hypothesis by adjusting our empirical framework to capture spillover effects between neighboring districts.

To do so, we first restrict our sample to school districts located in metropolitan areas and split it into large urban districts and smaller urban or suburban districts.[30] We define large urban districts as those at or above the seventy-fifth percentile of total enrollment in the within-metro distribution of districts. We then estimate the following econometric specification, aimed at capturing the effect of the presence of charter schools in large urban districts on the composition of smaller nearby school districts (and vice versa):

$$Y_{igt} = \beta E_{igt} + \psi E^{*}_{M(i)gt} + X'_{igt}\Gamma + \tau_{ig} + \delta_{it} + \gamma_{s(i)gt} + \epsilon_{igt}. \tag{5}$$

All variables are defined as in equation (2), and $E^{*}_{M(i)gt}$ is the charter share of enrollment of neighboring districts in the same metro area $M(i)$ of district i. When looking at outcomes for large urban districts, $E^{*}_{M(i)gt}$ is the charter share of smaller urban and suburban districts in the same metro. In our models of smaller urban and suburban districts, $E^{*}_{M(i)gt}$ is the charter share of the large urban district of the metro area. Therefore, ψ captures the spillover effect of the charter share of large urban districts on the demographic composition of smaller urban and suburban districts in the same metro (and vice versa).

We present the results in Table 6.[31] The first three columns use the sample of large urban school districts. For large urban districts, an increase in charter percent is associated with increases in the White share of total enrollment (column 1) as well as with higher White isolation and White segregation in these districts (columns 2 and 3). We detect no effects on the total share of enrollment for Black and Hispanic students, but we do confirm that they become more segregated. The charter share of nearby suburban school districts is not associated with compositional or sorting dynamics in large urban districts, as shown by the insignificant coefficients in columns 1–3. Conversely, columns 4–6 present estimates for smaller urban and suburban school districts, testing for spillover effects coming from charters in nearby large urban districts. Increased charter presence in large urban districts is associated with a decrease in the overall White and Black enrollment shares of smaller districts and a corresponding increase in the Hispanic enrollment share (column 4). We find no spillover effects of large urban charters on the stratification of smaller

[30] In metropolitan areas with intense school district fragmentation, it is common to find small districts surrounded by larger ones. For example, the Beverly Hills Unified School District in the Los Angeles area is completely surrounded by the massive jurisdiction of the Los Angeles Unified School District.

[31] For succinctness, we omit results for heterogeneity in Asian student segregation. There is limited heterogeneity on Asian segregation impacts, and as in Table 3, the coefficients are relatively small. Tables are available upon request.

Table 6—Urban-Suburban District Spillover Effects in Metropolitan Areas, by Race and Ethnicity

	Large urban districts			Small urban and suburban districts		
	Share	Isolation	Var. ratio	Share	Isolation	Var. ratio
	(1)	(2)	(3)	(4)	(5)	(6)
White						
Own charter percent	0.063	0.063	0.094	−0.017	0.044	0.082
	(0.020)	(0.025)	(0.030)	(0.018)	(0.007)	(0.012)
Small urban/suburban charter percent	0.024	−0.001	0.003			
	(0.016)	(0.019)	(0.024)			
Large urban charter percent				−0.013	0.001	0.005
				(0.006)	(0.004)	(0.008)
Total observations	71,746	71,746	71,746	479,529	479,529	479,529
Black						
Own charter percent	−0.026	0.045	0.071	0.050	0.048	0.073
	(0.017)	(0.012)	(0.027)	(0.016)	(0.008)	(0.012)
Small urban/suburban charter percent	0.016	0.004	0.007			
	(0.011)	(0.011)	(0.018)			
Large urban charter percent				−0.014	0.001	0.001
				(0.005)	(0.006)	(0.009)
Total observations	71,746	71,746	71,746	479,529	479,529	479,529
Hispanic						
Own charter percent	−0.005	0.028	0.044	−0.023	0.031	0.047
	(0.013)	(0.012)	(0.015)	(0.009)	(0.004)	(0.007)
Small urban/suburban charter percent	−0.021	−0.001	0.005			
	(0.013)	(0.012)	(0.018)			
Large urban charter percent				0.016	0.006	0.008
				(0.005)	(0.003)	(0.005)
Total observations	71,746	71,746	71,746	479,529	479,529	479,529

Notes: All models control for district-year, district-grade, and grade-year fixed effects as well as log total enrollment, group's share of enrollment, and total number of schools serving the grade. Estimation sample in columns 1 through 3 is a panel of district-grade-years in metropolitan areas, restricted to districts that are large and urban, which is defined as being in the seventy-fifth percentile of the within-metro distribution of total district enrollment in 2010. Estimation panel samples in columns 4 through 6 include all suburban districts in metropolitan areas as well as the complement of large urban districts ("small" urban districts). Standard errors are clustered at the school district level in all models.

districts, but we do confirm that the charters in these areas still cause direct segregation effects (columns 5–6).

Altogether, Table 6 provides evidence indicative that charter schools have spillover effects between districts, although our data preclude us from studying between-district student flows directly. The evidence suggests one key mechanism: some White students move to large urban districts when charter schools open in these jurisdictions, a "reverse White flight" effect from smaller urban/suburban to large urban districts. The evidence also indicates potential flows of Black families to large urban districts, resulting in a rising share of Hispanic students in smaller urban/suburban districts. At the same time, there is little indication that between-district integration translates to lower within-district segregation. The results show that increases in the White share of enrollment are juxtaposed with more White-isolated schools in large urban school districts. These patterns explain why the charter effect of higher between-district integration does not lead to higher school integration on net. Further assessing the mechanisms of these compositional dynamics in school enrollment is of great policy interest but beyond the scope of this study. Our results provide a first look into the complexity of stratification dynamics caused by the growth of the charter school sector.

C. *Treatment Effect Heterogeneity*

The evidence presented thus far establishes that charter schools increase the racial and ethnic segregation of schools. The average national effect is small in magnitude, which is likely a function of both charters' relatively small share of total enrollment and treatment effect heterogeneity. Our previous results suggest that a potential mediator in effect heterogeneity for school districts is urban/suburban differences in charter selection patterns. There is also research showing differences in sorting between urban and suburban charter schools. For the case of Boston, for example, urban charter schools tend to enroll disproportionately high shares of Black students (relative to traditional district schools), unlike nonurban charters whose Black shares are more representative (Angrist, Pathak, and Walters 2013). This difference is partly driven by charter philosophies; urban charters often feature the "no excuses" teaching model, which is targeted for at-risk students and has been shown to be effective at raising student outcomes (Angrist, Pathak, and Walters 2013; Cheng et al. 2017). Such variability between urban and nonurban charter school sorting suggests that the effect of charter schools on segregation would be larger in urban than in suburban districts.

We test this hypothesis in Table 7, which shows that charter sorting heterogeneity by urbanicity is complex, as it interacts with baseline district demographic composition. Column 1 tests this using our preferred triple differences models of segregation (equation (2)), interacting the charter percent variable with indicators for district urbanicity.[32] In column 2, we test whether impacts vary significantly by baseline district composition (2010 minority share of total enrollment), defined using three quantiles of the nationwide district distribution.[33] Columns 3 through 4 look at the interaction of urbanicity and baseline composition by estimating models akin to column 2, separately for urban, suburban, and rural districts.

We do not find meaningful heterogeneity in charters' impact on segregation by urban and suburban status alone. Column 1 shows that the coefficients on urbanicity interactions tend to be small in magnitude and, in most cases, indistinguishable from zero (the omitted category are urban districts). Estimates in column 2 show that charter schools' impact on segregation, while positive and significant across the board, is larger in districts with a high minority share of total enrollment. We split the sample to show that the patterns in columns 1 and 2 mask important interaction

[32] We define urbanicity based on the "locale" variable in the CCD, using 2010 definitions. We combined the "town" and "rural" categories into one, "Town/rural."

[33] The average minority share in the Low quantile was 7.4 percent, for Mid it was 25.4 percent, and for High it was 65 percent.

Table 7—Heterogeneity in the Effect of Charters on School District Segregation, by Baseline District Characteristics and Race/Ethnicity

	All	All	Urban	Suburb	Town/rural
White	(1)	(2)	(3)	(4)	(5)
Charter percent	0.096	0.055	0.003	0.144	0.038
	(0.019)	(0.013)	(0.017)	(0.042)	(0.012)
Charter percent × Suburb	−0.002				
	(0.025)				
Charter percent × Town/rural	−0.027				
	(0.022)				
Charter percent × Mid URM share		0.023	0.124	−0.050	0.002
		(0.018)	(0.032)	(0.048)	(0.017)
Charter percent × High URM share		0.047	0.098	−0.061	0.084
		(0.021)	(0.035)	(0.049)	(0.029)
Black					
Charter percent	0.066	0.041	0.017	0.051	0.043
	(0.015)	(0.014)	(0.011)	(0.028)	(0.021)
Charter percent × Suburb	0.029				
	(0.024)				
Charter percent × Town/rural	−0.010				
	(0.018)				
Charter percent × Mid URM share		0.036	0.062	0.083	−0.030
		(0.022)	(0.027)	(0.047)	(0.023)
Charter percent × High URM share		0.042	0.052	0.021	0.066
		(0.020)	(0.023)	(0.038)	(0.032)
Hispanic					
Charter percent	0.053	0.028	0.015	0.070	0.011
	(0.009)	(0.009)	(0.010)	(0.030)	(0.008)
Charter percent × Suburb	−0.001				
	(0.013)				
Charter percent × Town/rural	−0.025				
	(0.011)				
Charter percent × Mid URM share		0.007	0.046	−0.049	0.020
		(0.011)	(0.018)	(0.032)	(0.012)
Charter percent × High URM share		0.029	0.039	0.002	0.019
		(0.012)	(0.017)	(0.034)	(0.013)
District-year fixed effects	X	X	X	X	X
District-grade fixed effects	X	X	X	X	X
State-grade-year fixed effects	X	X	X	X	X
Covariates	X	X	X	X	X
R^2	0.911	0.911	0.951	0.906	0.830
Observations	831,042	831,042	154,980	310,922	363,309

Notes: Standard errors are clustered at the school district level in all models. Covariates include log total enrollment, number of schools, and group's share of grade enrollment. Baseline district characteristics are based on the 2010 NCES CCD. Underrepresented minority (Black or Hispanic) (URM) share of enrollment categories (low, mid, and high) are defined using three quantiles of the district distribution.

effects, most prominently for White stratification. Notably, charters don't have a main effect on the segregation of White students in urban districts; the impact is present only in urban districts with a relatively high minority share. For Hispanic and Black students, the main effect in urban districts is there, but it is attenuated relative to the pooled models, and the interaction with the high-minority indicators is positive, relatively large, and precisely estimated.

Heterogeneity patterns in suburban districts are considerably different and appear more complex across racial groups. Charters have a large main effect on the segregation of White students in suburbs. Further, the sign of the interaction with high-minority shares is negative (albeit imprecise), suggesting that charters cause more White segregation in a low-minority suburban district than in a high-minority one, exactly the opposite pattern of what we saw for urban school districts. For Black students, the suburban main effect is similar to the pooled models, and the interaction with higher-minority shares is positive, albeit noisy and indistinguishable from zero. Similarly, Hispanic students attending suburban districts see a charter segregation main effect of about 0.07 p.p. and imprecisely estimated negative point estimates in districts in which minorities are a larger share of the population.

We hypothesize that these heterogeneity patterns may be due to differences in sorting akin to the urban/nonurban differences discussed by Angrist et al. (2013) for the case of Massachusetts. The results are consistent with the urban charter hypothesis: charters in high-minority urban districts cause a relatively large segregation increase because they are more likely to have missions targeted at helping disadvantaged students. For Black and Hispanic students, charters lead to a larger rise in segregation in higher-minority urban districts than in higher-minority suburban ones. However, suburban charters located in low-minority districts have a large impact on the stratification of White students, which is suggestive that the type of charter schools opening in predominantly White suburbs are particularly attractive to White families. Also telling of complexity in impacts is the fact that charters have small to no segregation impacts for any group in low-minority urban districts.

Our results show that the effect of charter schools on segregation is heterogeneous by urbanicity and baseline demographics. Coupled with our earlier evidence of between-district spillover effects, we can conclude that the impact of the charter sector on student sorting is complex and heterogeneous. This reinforces the logic behind our reduced form empirical approach, but it complicates policy takeaways. One policy implication of these findings is that policymakers concerned about segregation should pay particular attention to the growing charter sector in high-minority urban districts and low-minority suburban districts.

D. *State Heterogeneity*

A number of previous studies that have analyzed the effects of charters on school segregation in particular cities or states have found mixed results (Ritter et al. 2014; Clotfelter et al. 2018). We hypothesize that the discrepancy in the evidence is likely due to charter schools impacting segregation differently in different parts of the country. This could happen because of several reasons. First, there could be variability in the target student population of charter schools' missions by state. For instance, in Texas, charter schools tend to serve historically underserved Hispanic and Black students in urban settings, while in North Carolina, charter schools are more likely to open near White suburban communities. Second, states vary in their chartering procedures, which could affect the composition of their charter sector.

For example, some states require local school districts to approve charters, which could allow local authorities to determine whether and how charters open. Finally, parental preferences over charters or school racial composition may vary by state, even when focusing on districts of similar demographics and urbanicity.

We provide a comprehensive test of the extent of heterogeneity in the segregation effect of charters by state. To do so, we estimate models akin to the main econometric model in equation (2), with the addition of interactions of state indicators with the charter enrollment share:

$$(6) \qquad Y_{igt} = \sum_{s} \beta_s D_{s(i)} \times E_{igt} + X'_{igt}\Gamma + \tau_{ig} + \delta_{it} + \gamma_{gt} + \epsilon_{igt},$$

where all variables are defined as in equation (2), i indexes school districts, and $D_{s(i)}$ is an indicator of the state the district is located in, with state-specific average treatment effects β_s indexed by s. Because they are interacted with the continuous treatment variable E_{igt}, state effects are interpreted directly, not relative to an omitted state. Our estimates of the state-specific effects (and their confidence intervals) are reported in Figure 6, separately by racial grouping. These models use a restricted sample of states that have at least a 1 percent charter school share of total public school enrollment at some point during the time period 1998–2018.

In 32 of the 37 states with a sizable presence of charters, our point estimate of the effect of the charter share on Black or Hispanic school district segregation is positive, and a majority of these (19) are positive and statistically different from 0. States in which the effect of charters on segregation is above 0.25 (i.e., more than double the national average) include Louisiana, Minnesota, Missouri, North Carolina, Oklahoma, Rhode Island, and South Carolina. For these states, a 1 p.p. increase in segregation leads to at least a 0.25 p.p. increase in the segregation of Black and Hispanic students. In 2018, school districts in our sample for these states had average within-district segregation levels of about 20 p.p. (and a mean charter share of 14 percent). This implies that on average, at least 18 percent of current school segregation levels in these states was caused by the charter school sector, a considerably larger share than the national average impact.

The results in Figure 6 also demonstrate substantial heterogeneity in state impacts across groups. As in the baseline estimates in Table 3, coefficients tend to be larger for Black and White students than for Hispanic students. But while there is variability in the state interaction point estimates, they also vary considerably in precision. Some states for which we cannot reject that charters have no effect on minority segregation include Arizona, Florida, Illinois, Oregon, and Pennsylvania. Finally, for a couple of states, our point estimates suggest that charter schools led to increases in integration. Although none of these are statistically significant, charters seem to have led to greater school district integration for minority students in Connecticut. However, we strongly advise against the overinterpretation of the outlier patterns in these estimates.

We hypothesize that state heterogeneity in the effect of charter schools is likely mediated by state variation in the composition of charter schools' target student population and mission. While we cannot observe charter school type in our data, we

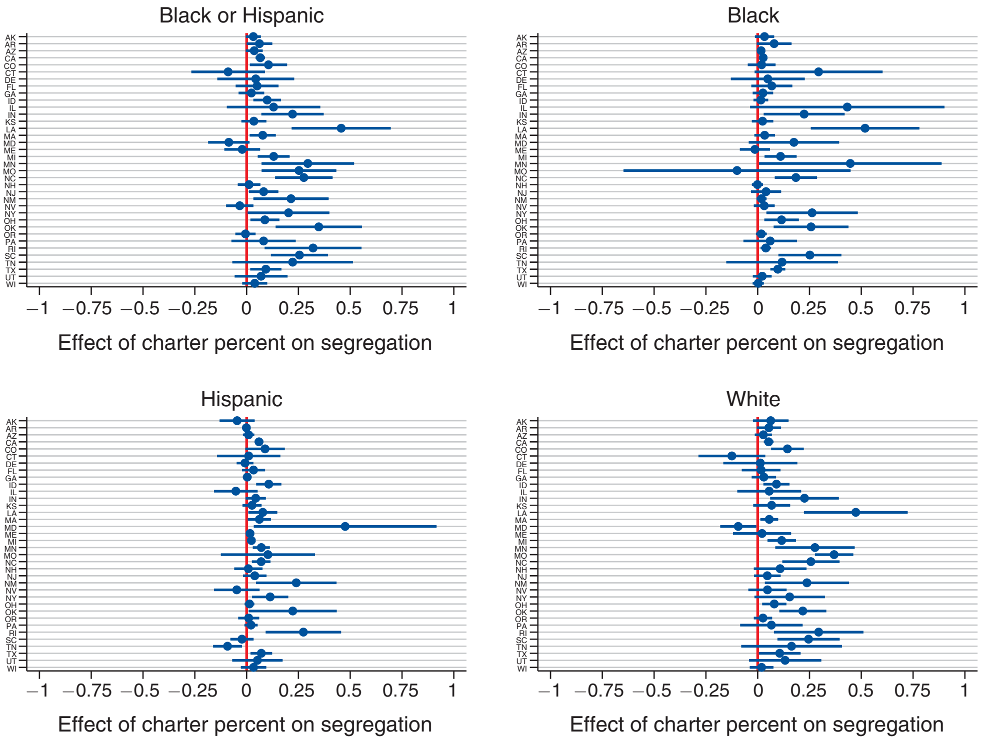

Figure 6. State Heterogeneity in Charter Schools' Effect on School District Segregation, by Race/Ethnicity

Notes: Standard errors are clustered at the school district level. Estimation sample is restricted to states that have at least 1 percent of total enrollment at charter schools at some point during the period 1998–2018. All models control for system-year, system-grade, and grade-year fixed effects as well as log enrollment, group share of enrollment, and number of schools.

present suggestive evidence of this channel in Figure 7.[34] The plots in the top panels show that the impact of charter schools in a given state is positively correlated with the urban share of charter enrollment, echoing the interaction estimates shown in Table 6 above. They also highlight some of the differences in state-specific impacts by race. Charter schools have quite a large impact on the segregation of Black students in Louisiana, Illinois, and Minnesota, all states in which the urban share of charter enrollment is close to 100 percent. The positive relationship between the estimated impacts and the urban share of the charter sector is also present for White students and, to a lesser extent, for Hispanic students.

Another potential correlated mediator of state heterogeneity in charter segregation impacts is the relative share of charter enrollment for a given racial group, defined as the ratio of their share of charter enrollment over their share of total public school enrollment. The plots in the bottom panels of Figure 7 show that

[34] In recent releases, the CCD has added a variable capturing information on the agencies in charge of certain charter school groups (e.g., the Knowledge is Power Program). Unfortunately, we do not have this information for much of the panel, which limits our ability to use this variable to investigate effect heterogeneity.

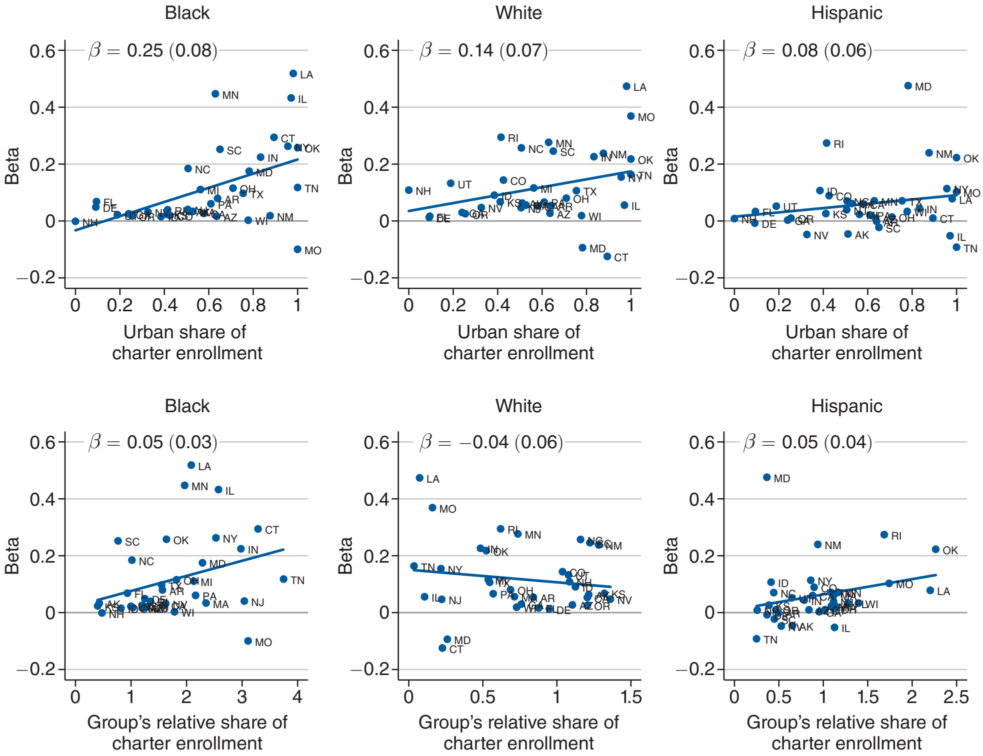

Figure 7. Candidate Mediators of State Heterogeneity in Charters' Effect on School District Segregation

Notes: The vertical axis shows the state-specific estimate of the effect of charters on the segregation of a given group. In the upper panels, the horizontal axis shows the urban share of the state's total charter school enrollment. In the bottom panels, the horizontal axis plots the racial/ethnic group's relative share of charter enrollment, defined as the ratio of the group share of total charter enrollment to their share of total public school population. The figure also reports the OLS coefficient of each these plots and its robust standard error.

the relative share is positively correlated with heterogeneity in states' impacts for Black and Hispanic students. This suggests that states in which charters educate higher shares of minority students than other public schools do tend to see higher increases in segregation as the charter sector grows. This evidence is consistent with the theory that states with charter schools geared toward serving underrepresented minority students will see a larger impact of the charter sector on school segregation, echoing some of the takeaways of our earlier results.

V. Conclusion

Employing a flexible identification strategy that controls for a wide range of observable and unobservable school system characteristics, we demonstrate that charter school growth over a period of 20 years has led to increased racial and ethnic segregation in US public schools. Our main estimates suggest that this effect is of modest magnitude. We present evidence that this effect varies widely across different types of school systems and by state. Moreover, we provide evidence that charters sometimes help improve compositional imbalances between school districts in the same metropolitan area. However, the slight rebalancing of district demographics has not translated into gains in the integration of schools. These takeaways are robust to a range of sensitivity tests and measurement choices.

However, the normative implications of these findings are complicated by the voluntary nature of charter school enrollment. If a family chooses a charter school because it meets their particular needs and this leads to higher segregation levels, it is difficult to say whether such an increase in segregation is detrimental to social welfare. Our examination of the heterogeneity of impacts suggests that charters in high-minority districts may cause a relatively large impact on segregation, which may be explained by the fact that they are more likely to have missions targeted at underserved students. In many such instances, charter schools have been found to increase student achievement for participants (Angrist et al. 2012; Angrist et al. 2016; Cordes 2018). Against this backdrop, it is possible that any negative effects produced by charter schools through increased stratification may be offset by charter schools' effects on student and school performance.

Moreover, while segregation by race is a highly salient and important topic in US public education, it is not the only type of segregation that is important. Policymakers and stakeholders are also rightly concerned about other forms of segregation, including segregation by income, disability, or English-language learner status. Recent work suggests that charter schools may increase socioeconomic segregation at levels comparable to our findings (Marcotte and Dalane 2019). Additionally, from the rationale of intergroup exposure theory, an ideal measure of integration would illuminate student contacts within schools. For example, while large comprehensive high schools may appear more integrated at the school level, students themselves may have little exposure to differences in their classrooms as a result of tracking (Dalane and Marcotte 2020). Future work should continue to explore the effects of charter schools on the distribution of historically underserved students in school systems and within schools.

Our findings suggest that policymakers should be attentive to the potential problems introduced when families are able to compete for a public good and how relative advantages across families may manifest in increased stratification. At the same time, a simulation exercise suggests that charter schools have the potential to generate integrative effects, amounting to as much as a 2 percent reduction in the segregation of minority students for the average district (online Appendix Table A1). Though this effect is small, the potential is greater in areas with significantly larger charter shares. Choice policies with the explicit goal of school integration seem worth considering given the political barriers that tend to thwart integration efforts in traditional public school districts.

One promising strategy comes from controlled choice policies that centralize school choice options into common enrollment systems. Research suggests that areas that adopt common enrollment systems reduce the burden of choosing a school and increase the proportion of disadvantaged students entering charter schools (Winters 2015). To the extent that the effect of charters on segregation is related to differential abilities of parents to navigate charter school options, common enrollment systems may ameliorate the problem. Related strategies include incorporating weights in common enrollment systems that increase diversity (Hawkins 2018b, a).

Other potential solutions involve so-called diverse-by-design charter schools. Though currently only a small fraction of charters fall into this category, they represent a growing trend (Potter and Quick 2018). Because charter schools have broad freedom to target their recruitment strategies, such designs have the promise of using charters as agents for integration. While little research has yet evaluated the effectiveness of such policies, strategies to encourage diversity, such as weighted admission lotteries and targeted recruitment efforts, show promise. In some areas, such as San Antonio, a holistic approach that includes charter schools and traditional public schools is being pursued that not only incorporates common enrollment systems and weighted admission lotteries but also strategically locates new schools of choice and provides increased funding for transportation (Hawkins 2018a, b). With the right design features, it is possible that charter schools could foster integration rather than segregation.

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