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The Effect of Charter Schools on School Segregation

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Abstract

We conduct a comprehensive examination of the causal effect of charter schools on school segregation, using a triple differences design that utilizes between-grade differences in charter expansion within school systems, and an instrumental variable approach that leverages charter school opening event variation. Charter schools increase school segregation for Black, Hispanic, White, and Asian students. The effect is of modest magnitude; segregation would fall 6 percent were charter schools eliminated from the average district. Analysis across varied geographies reveals countervailing forces. In metropolitan areas, charters improve integration between districts, especially in areas with intense school district fragmentation.

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1 Introduction

The expansion of school choice programs in US public education has been constant 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 improved matches between students and schools (Friedman 1962, Coons and Sugarman 1977, Chubb and Moe 1990, Hoxby 2003, Betts 2005). Indeed, multiple studies show that charter schools are effective at improving student test scores in many settings (Dobbie and Fryer 2011, Angrist, Dynarski, Kane, Pathak and Walters 2012, Angrist, Cohodes, Dynarski, Pathak and Walters 2016). 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, Pathak, Schellenberg and Walters 2020, Walters 2018).

At the same time, one of the most pernicious and persistent issues in US education policy over the last century is racial segregation. More than sixty five years after the pivotal *Brown* Supreme Court decision, segregation by race is still pervasive and enduring across public schools (Figure 2). The negative impact of racial segregation on students has been documented in a large literature spanning across the social sciences (Welch 1987, Guryan 2004, Card and Rothstein 2007, Clotfelter 2011, Billings, Deming and Rockoff 2014, Reardon and Owens 2014, Johnson 2015). Given the sorting patterns documented for charter schools, a key remaining question is the impact that charter school expansion has had on the racial stratification of school systems.

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

We address this gap by providing definitive evidence on the effect that charter schools have had on school segregation by race and ethnicity. We construct a 1998-2018 panel on the universe of school systems by grade level based on an annual census of US public schools, and document impacts across the four largest racial groups using four common definitions of school systems. Our research design is based on a triple differences model, leveraging between-grade comparisons in changes in charter penetration within system and year. We also exploit the arguably cleaner source of variation generated by charter school openings in an instrumental variables framework, confirming the internal validity of our findings. The results show that the expansion of charter schools has led to increased levels of segregation for every major racial group across school districts, cities, and counties, especially in urban school systems with large populations of underserved minorities. For the largest geographies, metropolitan areas, we uncover countervailing forces - while charters worsen segregation between the schools in a metropolitan area, they improve integration between school districts, where much of the racial segregation exists.

Our data source is the Common Core of Data (CCD), the census of public schools conducted annually by the US Department of Education. We use all waves of the CCD data that feature a charter school indicator, 1998-2018. Apart from being comprehensive, the main advantage of the CCD data is that it reports school enrollment separately by grade and race, allowing us to distinguish racial stratification levels for individual grades. The key variation exploited in our research design is extracted from this feature of the data. The causal effect of charter percent of enrollment is identified by comparing the dynamics of segregation between grade levels that have experienced differing intensity in charter growth. We restrict comparisons within school systems in a given year (system-year fixed effects) to eliminate the influence of system-wide time-varying confounding factors that are correlated with charter enrollment. School systems are defined using four distinct geographies: school districts, municipalities, counties, and metropolitan areas. Furthermore, we flexibly control for fixed differences in segregation across the grade levels of each system in our sample and for cohort effects (grade-system and state-grade-year fixed effects).

We demonstrate that these flexible controls 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 pre-trends in outcomes. We also implement an IV framework leveraging the arguably cleaner variation that is driven by charter school opening events. We construct the instrumental variable using charter schools' opening date and age. First, we estimate school level event studies that track the typical enrollment growth pattern of an average charter school, by grade. We use the estimates from the event studies to compute the predicted share of charter enrollment in each school system, grade and year. We use these predicted shares as exogenous instruments in our triple difference models, generating 2SLS estimates of the impact of charters that rely on the assumption that charter opening events impact stratification only via the charter share margin (after partialing out the flexible controls of the baseline model).

Across multiple specifications, the OLS and IV estimates are remarkably similar, suggesting that with the presence of flexible controls the OLS estimates are unbiased for the effect of charter schools on segregation. Additionally, we establish that, were charter school enrollment drawn randomly from the non-charter school population, charters would have a small integrative effect, instead of the segregative effect identified using actual charter enrollment 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 are reliable average treatment effects of the charter school sector on school segregation.

We find that charter schools have countervailing effects on racial stratification, decreasing segregation across school district jurisdictions, but increasing segregation between schools. Increases in a public school system's charter share of enrollment cause increased levels of segregation for the four largest racial groups: Black, Hispanic, White, and Asian students. The magnitude of the impact is modest for the average district, but more worrying for systems with large charter shares. Our estimates suggest that a one percentage point (p.p.) increase in the fraction of enrollment going to charter schools causes approximately a 0.10 p.p. increase in the segregation of Black or Hispanic students, or an elasticity of 0.06 for the mean district. Impacts are larger for Black and White segregation than for Hispanic or Asian segregation. Moreover, we document substantial effect heterogeneity across states, with a potential mechanism related to state differences in charter schools' relative presence in urban districts and their target student population. Indeed, our findings indicate the segregation impact of charters is greater in urban districts whose student body is largely comprised of under-served minorities.

An exception to the general patterns in the data are metropolitan areas, which show more marked differences between the OLS and IV models and show little to no impacts when testing for pre-existing trends using distributed lags.[1] To explore this mixed finding further, we decompose metropolitan area segregation into a component due to within-district segregation and a component due to between-district segregation (Clotfelter 1999). We demonstrate that charter sector growth leads to decreases in the between-district component of segregation, and simultaneous increases in the within-district segregation component, suggesting that charter schools have led to diminished compositional imbalances across district boundaries that have not manifested into greater integration between schools. We provide additional evidence supporting this hypothesis by showing that the decrease in between-district racial imbalance caused by charter schools is concentrated in metropolitan areas with intense school district fragmentation. We also present models suggestive of a "reverse White flight" pattern between urban and suburban districts that is linked with charter growth.

In sum, our analysis constitutes a compelling case for the notion that charter schools have lead 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.[2] Under this lens, charters leading to heightened 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.[3] Because they serve

[1] We conduct a Hausman-type test of equality between the OLS and IV estimates. We are able to reject equality between OLS and IV coefficients on minority segregation in metropolitan areas, which show IV estimates that are considerably larger than OLS. In contrast, we fail to reject IV and OLS equality for districts and counties. In municipalities we also reject the null of exogeneity, but the point estimates for minority segregation are of similar magnitude.

[2] See, for example, Johnson (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 et al. 2014, Hanushek, Kain and Rivkin 2009, Card and Rothstein 2007).

[3] 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, Clark, Tuttle and Dwoyer 2015, Abdulkadiroglu, Angrist, Dynarski, Kane and Pathak 2011, Dobbie and Fryer 2011, Angrist et al. 2012, Angrist et al. 2016, Abdulkadiroğlu, Angrist, Narita and Pathak 2017, Walters 2018). See Epple, Romano, and Zimmer (2015) for a summary of the existing evidence

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 funneled fewer resources for the education of the Black population (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 with the same lens. More research is needed to understand whose choices drive charters' segregation effect, and the impact that choice-driven segregation has on the outcomes of school systems.

The rest of the paper proceeds as follows. Section 2 provides additional background on the history of school segregation and school choice, and the existing literature on these matters. Section 3 describes our data and estimation sample, and presents descriptive statistics. Section 4 develops our empirical framework to estimate the causal effect of charter schools on segregation. Section 5 presents the main results of the paper. Section 6 conducts a range of robustness checks and provides our analysis of the components of metropolitan area segregation. Section 7 concludes.

2 Background and Literature Review

2.1 History of School Segregation

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 Brown v. Board decision (Reardon and Owens 2014). In 1968, fourteen years after Brown, U.S. public schools were still intensely segregated, with an average within-district index of dissimilarity between black and white students of roughly 0.80 (Logan and Oakley 2004) and an average within-district variance ratio index of 0.63 (Coleman et al. 1975, Reardon and Owens 2014).

As a result of court-ordered desegregation plans in the mid-1970s, these rates fell substantially, with the largest declines in the south (Clotfelter 2011, 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 (Coleman et al. 1975). 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 inter-district desegregation plans. Nevertheless, within-district segregation decreased substantially throughout the 1970s and continued to fall during the 1980s, albeit more modestly. Additional examinations of trends during the 1990s and 2000s find that trends in segregation have been near flat or modestly decreased over the past 20 years (Logan 2004, Stroub and Richards 2013). Between-district segregation is now higher than within-district segregation (Reardon and Owens 2014). Moreover, where districts tend to be larger, such as in the South and West, between district segregation tends to be lower (Clotfelter 1999).

on charter schools.

2.2 The Effects of School Segregation

From a political perspective that values equality and diversity, integrated schools are inherently good. Moreover, there is a rich literature supporting the notion that exposure to diversity has positive political and sociological benefits for a pluralistic society. In terms of measurable educational outcomes, an 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 consequences 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 et al. 2014, Vigdor 2011). Experimental lottery data show that a desegregation plan allowing minority students to transfer to higher-income schools with higher shares of white students increased college enrollment (Bergman 2018). Other research has documented the effect of both neighborhood and school segregation on the black-white test score gap, which is systematically higher in more segregated cities (Card and Rothstein 2007).

Reardon and Owens (2014) suggest that there are two primary mechanisms by which integration might improve student outcomes: by ensuring educational resources are more equitably available to all students (e.g., school resources, teacher resources, peer resources, parent resources), and by increasing the total pool of available resources because, for example, the political capital of parents in an integrated system may be more directed at acquiring higher total resources for the school system rather than specific schools. Thus far, studies have tended to focus on the availability of resources, which vary greatly as a function of segregation and seem to be a driving mechanism of the benefits of integration (Bergman 2018, Johnson 2015, Reardon and Kalogrides 2019, Reber 2010).

2.3 Prior Research Findings on Charter Schools and Integration

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, Timpane, Ross, Brewer and Booker 2007). This gap in the research has been more glaring in recent years as Secretary of Education Betsy DeVos has made the expansion of school choice a centerpiece of her tenure. In 2017, for example, the Associated Press (AP) conducted an analysis that compared charter schools to traditional public schools and found that charter schools were more likely to demonstrate high levels of racial isolation, which was widely interpreted as more segregated (Moreno 2017). The reaction to the story exemplified the importance of measurement and the divisiveness of the issue. The president of the American Federation of Teachers, Randi Weingarten, called the data from the Associated

Press "damning," and argued that "America's children deserve better" (Fay 2017). The National Education Association announced "Racial Isolation of Charter School Students Exacerbating Resegregation" (Walker 2018). Charter proponents pushed back, calling the Associated Press analysis "irresponsible" and arguing that charter schools merely reflected the neighborhoods in which they locate and the students they predominantly serve, which in both cases tend to be students of color (Fay 2017, Lake 2017). Charter schools, they argued, were being unfairly criticized for doing exactly what they had set out to do—serve students who were most in need of better educational options.

Similar disagreements surfaced when UCLA's Civil Rights Project issued a study establishing that isolation of minority students is more severe in the charter school sector than in traditional public schools (Frankenberg, Siegel-Hawley and Wang 2010). A reanalysis using similar methods but limited to geographical areas where charters tended to locate—urban areas with high residential concentrations—found that both charter schools and traditional public schools were similarly isolated (Ritter, Jensen, Kisida and Bowen 2014). Importantly, such relationships do not tell us the causal effect of charter schools on systemwide segregation, as they do not identify the effect that charter growth has on the distribution of enrollment patterns in school systems.

Other studies dig deeper by following individual student transfers in choice systems and judging if the transfers harm or improve exposure or isolation. Comparing students' neighborhood-based school assignments to the actual schools students attended as a result of a public school choice program in, Durham, N.C., Bifulco et al. (2009) found that White parents tended to make moves that exacerbate isolation while Black parents made integrative moves. Also using North Carolina data, Bifulco and Ladd (2007) and Ladd and Turaeva (2020) found similar results when examining student transfers to charters. Similarly, Garcia (2008) used statewide Arizona data across a four-year period to track students who exited traditional public schools to attend charter schools and found that reductions in exposure were largely driven by White students transferring to whiter charter schools.

In contrast, Ritter et al. (2014) found that student transfers to charters improved the overall racial balance in the school system of Little, Rock, Arkansas. Zimmer et al. (2009) examined charters in 7 areas and concluded that, on average, students tended to transfer between traditional public and charter schools with similar racial compositions, though there was considerable variation by region. In Denver, San Diego, Ohio, and Texas, black students transferred to charters with higher concentrations of Black students, whereas in Chicago and Milwaukee, both Black and Hispanic students transferred to charter schools with lower concentrations of their own race/ethnicity. Similarly, in Denver, Texas, and Ohio, Hispanic students transferred to charters with lower proportions of similar students. In Philadelphia, White, Hispanic, and to a lesser extent Black students students transferred to charter schools with higher concentrations of similar students.

Such studies are informative, as they allow us to directly observe the movement of students by race/ethnicity and shed light on the mechanisms through which choice may impact segregation. At the same time, these studies are limited in scope and specific to local geographic conditions. Moreover, rather than looking at the net effect on the distribution of students in school systems, they rely on the amount of exposure or isolation experienced by students who transfer. We address this gap with a comprehensive analysis of the effects of charter schools on the distribution of students in school systems using the universe of public school systems containing charter schools from 1998-2018.

3 Data

The main data source in our study is the National Center of Education Statistics' (NCES) Common Core of Data (CCD), which includes school enrollment counts by grade level and race/ethnicity, school type, and the latitude and longitude of schools' geographic locations.[4] The data are accessed via Urban Institute's Education Data Portal. We use standard GIS procedures to match school locations to different geographical units: geographic school districts, municipalities, counties, and metropolitan areas. We treat each of these geographies as distinct definitions of school systems. This is particularly important as it allows us to geo-locate charter schools in the school systems that they affect. For school districts, we use the 2015 definition of school district boundary maps from NCES' Education and Geographic Estimates (EDGE). For metropolitan areas, we use U.S. Census Bureau TIGER/Line 2010 definitions of Core Based Statistical Areas, focusing only on Metropolitan Statistical Areas. We also geo-locate schools to Census Places, the census' formal definition of all "municipalities" – incorporated cities, towns, and unincorporated concentrations of population in the U.S. Finally, we merge school location data with 2010 U.S. Census Bureau tracts and blocks. From these data we acquire residential population counts by age/race/ethnicity, adult educational attainment, and median household income.

We structure the data as a stacked panel of school systems over years and grade levels—for each year in 1998-2018. We observe racial composition for each school grade level across grades K-12 (1998 is the first year the charter flag is available). In terms of school level sample restrictions, we drop closed or inactive schools, schools devoted to special programs, schools serving only kindergarten or lower, those providing only adult education, and schools not located in U.S. mainland states. We also drop schools with missing enrollment counts by race, an issue that is prevalent in some states during the early years of the data. With regard to system level sample restrictions, we drop 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 these earlier sample restrictions.[5]

Our final analytic sample includes four distinct stacked panels of school districts ($n = 5,325$), municipalities ($n = 5,610$), counties ($n = 2,741$), and metropolitan areas ($n = 330$), observed for grades K-12 across 1998-2018. The total number of observations in these panels approx-

[4]To ensure the accuracy of the school location data, we conduct a geocoding procedure using school address data, which is more complete than location data in the CCD. This makes a difference especially for early years (approx. 1998-2009) in which location data is not available, missing, or otherwise low quality. The school location data we use is publicly available via the Urban Institute's Education Data Portal.

[5]In our primary analysis, we treat virtual public schools like other public schools, since they account for student enrollments and from a practical perspective provide the fullest picture of how students are distributed in school systems. We have also run our results excluding virtual schools and our findings are consistent.

imately ranges between 85,000 and 800,000.[6] It is also worth noting that the CCD charter indicator is often missing or otherwise of low quality during the early years of our estimation sample (about 1998–2003). Our main results are insensitive to the removal of these early years of data.

3.1 Descriptive Statistics

Nationally, charter schools have increased their share of total enrollment. Figure 1 shows that, between 1998-2018, the average K-12 student attended a district-grade in which the charter percent of enrollment grew from 0.3% to 6.9%. This overall trend includes many districts that never have charter entry, so it understates the average charter share growth among districts that have seen charter entry, which rose from 0.49% to 11.6 % over the same period. It is important to keep in mind that these national trends mask considerable variation in charter growth between districts and across grade levels within districts.

Charter schools tend to serve a population that over represents Black and Hispanic students. Table 1 shows that, compared to traditional public schools, charters enroll higher proportions of Black students than White students in elementary and middle schools. Charter schools also tend to enroll higher proportions of Hispanic students in middle and high school. These student enrollment characteristics are largely reflective of charter school locations. Charter elementary and middle schools are more likely to be located in census tracts with higher proportions of Black residents, while charter middle and high schools are located in areas with higher proportions of Hispanic residents compared to White residents. This is consistent with research demonstrating that schools tend to closely reflect neighborhood compositions (Monarrez 2020, Whitehurst, Reeves, Joo and Rodriguez 2017). Charter schools also tend to be located in tracts with relatively lower median income and adult educational attainment.

3.2 Measuring Segregation

Determining the effect of charter school growth on school system segregation has proved vexing. A notable takeaway from related 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 involve absolute measures and relative measures (Clotfelter, Hemelt, Ladd and Turaeva 2018). Absolute measures describe the extent to which students from one demographic group are exposed to another group within individual schools. A common exposure measure is the isolation index, which measures the average school minority share experienced by minority students. Other approaches quite simply define segregated schools as those with high proportions of Black or Hispanic students. Some researchers have adopted the term "hy-

[6]These stacked panels are highly unbalanced. For example, in the school district sample only 17% 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 grade. For districts with a single high school, for instance, we drop high school grades but still keep their middle and elementary schools, if they meet the criteria.

persegregated" to describe schools whose enrollment is comprised of 90% or higher share of underrepresented minorities, showing that charter schools are more hypersegregated (Frankenberg et al. 2010, Orfield, Jongyeon, Frankenberg and Siegel-Hawley 2016).

While descriptively 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 underlying pool 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. 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, Caetano and Maheshri 2017, Harris and Curtis 2018). These issues complicate comparisons of absolute segregation across time and place.

Relative segregation measures focus on stratification by describing how evenly the population of minority students is distributed across schools, adjusting for the racial composition of the school system. This makes them 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:

$$Variance\ Ratio = \frac{E[q_{sj}|URM = 1] - Q_j}{1 - Q_j} = E[q_{sj}|URM = 1] - E[q_{sj}|URM = 0], \quad (1)$$

where q_{sj} is the fraction of students in school s in school system j that are an underrepresented minority (URM), i.e. Black or Hispanic.[7] The isolation index, which is the average of q_{sj} when restricting attention to the URM population, can be written as the conditional expectation $E[q_{sj}|URM = 1]$, where URM is an indicator for URM students. This term is adjusted by Q_j, the system-wide fraction of the URM population. The intuition of the adjustment is straightforward. In a perfectly integrated system all schools would have a composition equal to Q_j. On the other hand, in a perfectly segregated school system URM students are only exposed to themselves, so the isolation index would equal one. Noting that the denominator is equal to the maximum excess isolation level possible and the numerator is the level of excess isolation currently experienced in the school system, the variance ratio measures existing excess URM isolation, relative to a complete segregation benchmark.

It is a remarkable fact of algebra that the variance ratio also coincides with the second equality in (1), which is the difference in average school exposure to URMs between URM and non-URM students. Therefore, the variance ratio index can also be interpreted as a gap in URM exposure. It bears mentioning that this characterization of the variance ratio also coincides with the OLS slope coefficient of a student level regression of q_{sj} on the URM indicator. In other words, the variance ratio can also be interpreted as how predictive a student's race is of the race

[7]the index includes charter schools and every other type of public school that reports enrollment counts to the Common Core of Data. 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.

of her school peers.

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 (Card and Rothstein 2007, Reardon and Owens 2014, Graham 2018). As such, our main results report charter schools' impact on the variance ratio. But to be sure that our results are not driven by our choice of segregation index, we conduct a parallel analysis in the appendix using another common relative measure of segregation, the index of dissimilarity (Table A4 in the 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 2019). But in today's diverse student population this would seem more arbitrary. The recent economics literature on segregation measures the segregation of URM (Black and Hispanic) students from others (Card, Mas and Rothstein 2008, Caetano and Maheshri 2017).[8] Recent work also uses multi-group indices of segregation (called "entropy") to account for the country's growing diversity (Iceland 2004)[9]. We take a middle of the road approach, presenting results for the segregation of URM students for comparability to the recent literature, and also looking at the segregation of 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 assuming a random school sorting mechanism, a school system with more schools has a wider scope for segregation than one with fewer schools simply because of sampling error. In 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 smaller population. Thus, our models control for number of schools to pick up these mechanical impacts, so the concern that our impact estimates may be mechanical worries us little.[10]

[8]It should be noted that the segregation of URM students may be considerably different (usually higher) from the segregation of Black students and the segregation of Hispanic students. URM segregation is not a linear combination of the segregation of these groups measured separately.

[9]For reference, we provide baseline impact estimates of charter school share of enrollment on Theil's H index of multi-group segregation in appendix Table A7.

[10]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 non-charter schools (with replacement), but are otherwise equally numerous and populous as in the real data, see the bottom of section 4.1 and Table A1 in the appendix. We also perform tests of treatment effect heterogeneity across school districts with different population sizes, shown in Table A5 in the appendix. Our estimate of the impact of charters on segregation is remarkably similar across quartiles of the distribution of total district enrollment.

3.3 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 group one focuses on, national trends in school segregation have been essentially flat over the last twenty 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 2000's 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 they 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 (Fiel 2013, Logan 2002, Logan 2004, Reardon and Owens 2014, Whitehurst et al. 2017).

While trends are similar, segregation levels across geographies are considerably different. When we measure it at the school district level, average school segregation is in the order of 20 in most years of the data. School segregation levels for municipalities is similar to that of districts. In contrast, when measured at the metropolitan area level segregation jumps up to about 35. County level segregation is in the middle, about 28 throughout the sample. Schools are therefore more severely segregated across metropolitan areas than they are within school districts, as has been documented in the literature (Clotfelter 1999, Reardon and Owens 2014, Reardon and Kalogrides 2019, Reardon, Yun and McNulty 2000, Stroub and Richards 2013). These differences have important implications for our evaluation of the role of charter schools in determining racial stratification patterns.

4 Empirical Framework

Theory suggests that the expansion of charter schools can impact school segregation via at least three distinct mechanisms, generating ambiguous predictions. First, they provide an outside option from traditional district schools, expanding families' choice set and partly removing the deterministic link between schools and neighborhoods (Monarrez 2020).[11] Because US cities typically have segregated neighborhoods, this mechanism raises the scope for school integration,

[11]Studies examining school commuters demonstrate that charter schools weaken the link between residence and school assignment. Using data from Detroit, descriptive work has found that more than three quarters of students who left the city to attend a school did so to attend a charter school, nearly double the rate of charter attendance for those who remained (Cowen, Edwards, Sattin-Bajaj and Cosby 2018). Moreover, Black and Hispanic students tended to travel further because they lived further from high-quality school options, whereas White and Asian students were more likely to live in tracts that bordered suburbs. Nationally, while 22 percent of parents attending residentially assigned schools report moving to their neighborhood specifically to attend their school, only 11 percent of parents attending schools of choice report making similar moves. These data also reveal disparities along racial lines–though roughly a quarter of White or Asian parents reported moving to attend a residentially-assigned school, only 17 and 14 percent of Hispanic and Black parents did the same (U.S. Department of Education 2017).

but cannot by itself achieve it. 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. If charters are considered "high quality", they may attract parents of higher socioeconomic status, placing downward pressure on school segregation. Finally, a third channel is linked to secondary impacts on the composition of district schools resulting from the sorting dynamics initiated by families that leave neighborhood schools for charters. These dynamics could evolve further, perhaps triggering tipping point effects (Schelling 1972, Caetano and Maheshri 2017). In addition, there is evidence that charter school selection behavior varies considerably between urban and suburban areas. Studies of the Boston area charter sector have shown that, while urban charters enroll predominantly URM students, suburban charter enrollment demographics are similar to that of district schools (Angrist et al. 2013). This suggests that parental preferences over charters may vary by locality, by socioeconomic status, or both.

There is thus a multiplicity of countervailing mechanisms at play behind system-wide sorting dynamics driven by the expansion of the charter sector, making the effect of charter schools on segregation ambiguous. This theoretical ambiguity motivates the following reduced form empirical analysis. We begin by defining the endogenous variable of interest. We parametrize treatment dosage as the percent of total public school enrollment in a given geographic jurisdiction that goes to the charter school sector, making the treatment variable continuous and bounded between 0 and 100. We parametrize treatment in this way because 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 geographies of different population density.

The key intuition of our research design is that comparing grade differences in segregation by charter dosage within a system-year can rule out multiple competing explanations. In this sense, the best source of identifying variation in our data is the charter school opening (and perhaps closing). However our setting is not ideal for a standard event study design, since multiple charters can open and close in a given system-year, the level at which the outcome is measured.[12] Furthermore, when charter schools open, they do so gradually, meaning that the "event onset" of charter schools "treating" a district is not discrete. Thus, we first focus on models that parametrize treatment continuously while still retaining the key identification intuition, circumventing these issues. We then confirm our findings by leveraging the, arguably cleaner, charter opening event variation in an instrumental variables framework, outlined in section 4.1.

We begin by estimating the following econometric specification

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

[12]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.

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 an idiosyncratic error component that varies at the system-grade-year level and, if correlated with E_{igt} , may threaten the validity of the assumptions necessary to interpret β causally.

This specification can be interpreted as a triple differences strategy, with identification relying primarily on the inclusion of school system-year fixed effects, but also accounting for state-year-grade and system-grade variation.[13] 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 urban change and gentrification - which has happened sporadically across certain urban localities - may have on stratification patterns. Nonetheless, by themselves the system-year fixed effects cannot account for important between-grade 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, it gets 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. Second, system-grade effects difference out time-fixed variation in segregation across school grade levels, which have been documented empirically (Greenberg and Monarrez 2019). In addition, the state-grade-year effects $\gamma_{s(i)gt}$ ensure that we also flexibly account for differences in segregation by grade that vary by year and state, which could be driven by state-specific cohort effects, such as the secular growth of Hispanic enrollment during the last two decades in certain areas of the country.

Intuitively, our empirical strategy is to identify causal effects using variation in charter enrollment *dynamics* across grade levels within each school system. For example, if in 2010 the share of 9th grade charter school enrollment in Washington, D.C. grew more than in other grades and there was a corresponding increase in 2010 9th grade segregation changes, relative to other grade levels, our model would attribute this to charters having a causal association with increased segregation. Our national estimate β of this effect 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.[14]

[13]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).

[14]Since our strategy relies on the structure of the grade, year, and district effects, we present estimates of

4.1 Robustness

The triple differences 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 all of them out. One may worry that there could be unobserved factors driving both changes in charter school enrollment and in segregation at the system-grade-year level. Another potential confounder could arise if segregation dynamics themselves cause student flows in and out of existing charter schools, impacting the charter share of enrollment. This could happen if families' decision on charter schools is driven in part by within-grade segregation dynamics in the school system.

We carry out a series of tests to ensure that these potential explanations do not drive our main findings. First we conduct pre-trend tests using the following distributed lag specification:

$$Y_{igt} = \sum_{l=0}^{3} \beta_l E_{ig,t+l} + X'_{igt}\Gamma + \tau_{ig} + \delta_{it} + \gamma_{s(i)gt} + \epsilon_{igt}. \quad (3)$$

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 segregation, which would threaten our identification assumption.

We also carry out a two-stage estimation procedure that leverages variation generated by charter school opening events as the source of identification. The first step is to estimate the change in the charter share of enrollment that is attributable to the opening of charter schools, and not to the churn of student flows between existing schools in the district. 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, Coughlin, Crews and Ross 2019).[15] 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, then it 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.[16]

We compute the charter school fitted values $\hat{e}_{sgt}$ from the grade-specific school level event studies shown in Figure 3 (where k indexes schools). Next, we aggregate these fitted values to

models that vary fixed effect structure in Table A6, showing that our main conclusions do not rely heavily on the specific fixed effect structure of equation (2).

[15] We estimate grade-specific models of the following specification: $e_{sgt} = \sum_{k=1}^{10} \alpha_{kg} D^k_{sg} + \phi_{sg} + \psi_{tg} + \nu_{sgt}$, where e_{st} is the total enrollment of charter school s in year t; D^k_{sg} 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.

[16] 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.

the level of our outcome of interest, a system-grade panel:

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

where N_{igt} is total district enrollment by grade and year, $(\{\hat{\alpha}_{kg}\}_{k=1}^{10}, \hat{\phi}_{sg}, \hat{\psi}_{tg})$ 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 first is driven by charters' birth date, age, and the event study coefficients – the impact of the number of years charter schools have been open, given the typical growth of new charter schools. The second is the sum of school fixed effects, which capture differences in the overall size of charter schools that are opening. While potentially problematic, the flexible controls in equation (2) absorb variation in the charter share driven by these fixed differences between charter school sectors. The third is determined by the grade-specific year effects in the event study model, capturing the secular increase in charter enrollment over time, which is also absorbed by controls.

We leverage variation driven by charter school openings using the predicted share of charter enrollment defined in equation (4) as an exogenous instrument in 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 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, but we confirm these with a 2SLS specification and by conducting Hausman-type tests of equality between the OLS and IV impact estimates.

We conduct additional robustness tests which are informative in their own right. First, we carry out a randomization-based falsification test using a transformed outcome variable. For each charter school in the data we drew a binomial random variable with number of trials equal to the charter'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 non-charter 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 synthetic charter counts, and estimate our baseline triple differences model (equation (2)).

Because the variance ratio index considers a school that is racially representative of the district to be 'integrated' (equation (1)), the synthetic charters will tend to push segregation toward zero. In other words, if charter school enrollment is representative of district schools (up to randomization error), then the estimate of β in equation 2 should be negative when using counterfactual segregation as the outcome. The estimates in Table A1 in the appendix support this hypothesis. The coefficients on charter percent in these models tend to be small and negative, confirming the claim that, were charter school students randomly drawn from the district school population, they would have the impact of lowering segregation. We show in section 5 that this is not the case when using the real data on charter enrollment breakdowns.[17]

As a final robustness test, we estimate a battery of placebo tests, presented in detail in the appendix. Intuitively, the placebo tests ask whether charter growth in elementary school grades is predictive of contemporaneous changes in middle or high school segregation, and vice versa. Observing such a relationship in the data may be indicative of unobserved confounders that vary at the district-year-grade level and could threaten the causal interpretation of our main estimates. In practice, we test the cross-grade link hypothesis across all 13 school grade levels. The direction of the potential cross-grade link is interesting by its own right. One could imagine that off-diagonal coefficients with $g < g$ (the lower triangle in Table A.2) could capture preemptive behavior on the part of households. This could happen if student sorting in early grades reacts to the growth of charter schools in higher grades, as families choose the educational trajectory of their children. Under this view, significant patterns in the upper-triangle of the placebo test matrix are more worrisome in terms of threats to our research design, as they cannot be explained by preemptive behavior and are more likely to be an indication of endogeneity problems. See appendix A for details.

5 Results

Table 2 summarizes our estimation samples (defined at the system-grade level), 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 controlling 99 and 92 schools on average. Of these, about 10 to 11% are charter schools, although they tend to have lower enrollment than other public schools, enrolling about 8% of the student population. Our charter enrollment growth models (Figure 3), predict that the average charter share is slightly lower, about 7%.

Students attend districts and municipalities in which almost half (48%) of the student body is Black or Hispanic, and about 40% 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 student as an "underrepresented minority" (URM) group. This choice makes our work more comparable to the existing literature.

Patterns for larger geographies – counties and metropolitan areas – highlight that school segregation is more severe as one zooms out from school district and municipal boundaries.[18]

[17]As discussed in section 3.2, 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.

[18]In some instances, county and school district geographies coincide. This is common, but not ubiquitous, in the Southern US. For instance, counties and districts coincide in Florida, but not in Louisiana or Kentucky. But

Metropolitan areas do not fully cover the US, defined only in urban centers, although 91% of public school students nationwide attend school in a metro area. Students in metro areas have about 50 thousand peers in a given grade, attending about 870 schools. The average charter share of enrollment is similar to that of districts, about 7.4%. 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% larger than county segregation.

Having a sense of our estimation sample, the odd-numbered columns of Table 3 present baseline estimates of the impact of a one percent increase in the charter enrollment share on school segregation. These are OLS estimates of equation (2), which includes system-year, system-grade, and state-grade-year effects. Our preferred interpretation of these models is as a within-district-year estimator of the effect of the charter share. We estimate that a one percentage point increase in the charter share leads to between a 0.09 a 0.10 percentage point increase in the segregation of URM students in school districts, municipalities, and counties.[19] In contrast, for metropolitan areas the impact on URM segregation is lower and noisier, about 0.06 p.p. The discrepancy between metro areas and other geographies is a harbinger of much of the empirical results that follow.

The second and third panels of Table 3 show that impacts for Black student segregation are about 50% larger than impacts on Hispanic student segregation. We estimate that if charter schools increase their share of enrollment by 1 p.p. then Black segregation would go up by about .07 to .1 p.p., while Hispanic segregation would increase by .04 to .05 p.p. Impacts for Asian students are somewhat smaller than that for Hispanic students, in the range of .02-.04 p.p. For White students, the segregation impact of charter growth is roughly the same as the impact on URM segregation 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 URM segregation must be approximately equal by construction.

To get a better grip on the magnitude of the effect estimates, consider that in 2018 URM students experienced segregation levels of about 12.6 percentage points, and the mean charter percent of enrollment was 8.1%. The OLS fixed effect models thus suggest that, on average, the presence of charters has caused about 6% of current school segregation levels in districts. While the magnitude of this effect is modest, our models offer a cautionary tale for localities with a rapidly growing presence of charter schools. Extrapolating the charter share to 20% of public school enrollment, would lead to an increase in segregation of 15% relative to the nationwide average. This suggests that the elasticity between charter share and segregation is about .06 for the average district and closer to 0.15 for districts with a large charter share.

One may be worried that the effect estimates reported in Table 3 could be biased by district-

generally, district geographies tend to be smaller than counties.

[19] In Table A5 in the 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.

specific pre-trends in segregation leading the growth of the charter school sectors. Figure 4 presents our estimates of equation (3), which is a generalization of the models in Table 3 that includes leads and lags of the charter share of enrollment. Across geographic samples, our models estimate precise zeroes for leads of the charter share (negative event time). This suggests that past segregation dynamics that are system and grade specific 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 total enrollment, racial population shares, and the number of schools serving a grade. This evidence supports the claim that there are no pre-existing trends in segregation leading up to the growth of the charter school sector. Moreover, there is little indication of a lagged effect of charters on segregation, as noted by insignificant coefficients for positive event time in the plots. In sum, this evidence is consistent with charter growth causing contemporaneous 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 pre-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 5.1.

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 pre-existing 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 due to cohort demographic change - could themselves cause increases in charter enrollment growth, specially if households use segregation dynamics in their decision 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 fixed effects and time-varying controls).[20]

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 percentage points and up to 2.3 percentage points. Similar patterns hold

[20]In Figure A3 of the appendix we present tests for pre-existing trends in the instrumental variable, the predicted share of charter school enrollment given their opening dates and ages. The estimates confirm a lack of pre-trends in the instrument, similar to patterns in Figure 4 (the coefficients in Figure A3 are smaller, since they are reduced form estimates, see Table A3).

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 URM segregation across most geographies. We conduct Durbin-Wu-Hausman (DWH) tests of equality between the IV and OLS coefficients on URM segregation, presenting the p-values for these at the bottom of Table 3. Assuming the validity of the IV coefficients, we fail to reject the assumption that charter percent is exogenous (after partialing controls out) 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%), but for metro areas the IV is consistently about 2-3 p.p. (about 33%) 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 5.1).

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. 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, we report the IV estimates as a useful check on the credibility of our claim to causality.[21]

As a final robustness check, we show in Table A2 of the 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 appendix A for a detailed description of the placebo tests.

Together, the absence of segregation pre-trends, the similarity of the OLS and IV point estimates, and the consistency of both the placebo tests and the randomization-based falsification tests (Table A1), provide convincing evidence that our models identify the causal effect of the charter share of enrollment on the racial segregation of public school systems. While the magnitude of this effect is modest, we can confidently 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 more odd patterns. While our baseline models still indicate that charters lead to 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 5.1); we then turn to an

[21]Table A3 in the 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.

analysis of effect heterogeneity (section 5.2).

5.1 Between-District Segregation in Metropolitan Areas

An additional consideration for the effect of school choice on segregation is its potential ability to diminish the role that the jurisdictional boundaries of school districts have in determining school segregation at the metropolitan area level. It is a known fact that racial composition differences between school districts drive about two thirds of metropolitan school segregation (Clotfelter 1999). But charter school enrollment normally takes place with little regard to district jurisdictional divisions.[22] Indeed, we are only able to measure charters' impact on school districts by linking them geographically to district boundaries and implicitly assuming that any school sorting takes place within the population encased within district jurisdictions. Due to the potential importance of this mechanism, we now develop a framework to test the impact of charters on racial stratification patterns across school district boundaries.

The introduction of school choice could have different simultaneous and counteracting effects on school segregation at the metropolitan level. On the one hand, we know from the analysis above that the presence of charter schools leads to increased racial stratification within school districts. On the other, if charters lead to increased between-district sorting patterns, this could potentially have an integrative effect. Such an effect would be especially likely if charters cause increased non-minority enrollment in high minority districts, or vice versa.

Using our grade-specific panel datasets on school segregation by metro and year, 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.[23]

Table 4 presents our estimates of the effect of charter percent of enrollment on each com-

[22]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. Colorado requires that a majority of students come from the district where the charter resides or from contiguous districts, which likely covers anyone within a reasonable commuting distance. At the other end of the spectrum, some states require that charters be open to all students, regardless of district. For example, Indiana has no geographic constraints, but like most states it allows preferences for siblings, children of employees, and socioeconomically disadvantaged students (Education Commission of the States 2018).

[23]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 measure.

ponent of metropolitan school segregation. Columns (1) and (2) replicate the OLS and IV results on total metro segregation in Table 3, which showed that the effect on metropolitan area segregation is lower than it is on smaller geographies like school districts, municipalities, and counties. Columns (3) and (4) report the impact of the metro charter share on the within-district component of metro segregation. The estimate magnitudes here are remarkably similar to the estimates for the school district panel in Table 3 (1) and (2), even though Table 4 uses a metropolitan level dataset. We interpret this as added evidence of the robustness of our results of the effect on school district segregation.

Columns (5) and (6) report OLS and IV estimates of the impact of charters on segregation between school districts, where we find *negative* point estimates. Interestingly, our estimates indicate growth in the charter share leads to lower levels of between-district segregation. While our point estimates are negative across all the major racial groups, we can reject that these impacts are zero for URM, Black and White stratification in the OLS models, and only for URM (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, the estimates suggest that, were charter schools abolished, between-district segregation would fall by about 1%. 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 4 establish that charter schools have counteracting effects on school segregation at the metropolitan area level. As we had shown before, charters increase the segregation taking place inside 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 1987). 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. Magnets were thus meant to 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, but this has not resulted in greater school integration.

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 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.[24]

[24]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,

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 medium to high school district fragmentation. In contrast, effects for the within-district component of segregation (reported in in the top right panel) are positive and at least marginally significant across the board, suggesting that the within-district segregative 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.

One possible explanation for these findings is that metropolitan areas that are fragmented are those in which white animosity toward school integration was historically more intense. This would be consistent with higher white flight to suburban districts and more district secessions (Reber 2005). If charter schools are bringing white students back to urban districts, but into white-isolated charter schools, this would lead to higher within-district segregation but lower between-district segregation. We can shed some light on these implications using our current empirical framework.

To do so, we first restrict our sample to school districts located in metropolitan areas and split it into large urban districts and other urban or suburban districts. We define large urban districts as those at or above the 75th percentile of total enrollment, in the distribution of districts within the metro. 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):

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

All variables are defined as in equation (2) and $E^{*}_{M(i)gt}$ is the charter percent of enrollment in certain other districts in the same metro area M as i.

We present the results in Table 5. The first three columns use the sample of large urban school districts, regressing their segregation level on their own charter percent of enrollment as well as the charter percent of enrollment at smaller districts in the same metropolitan area. For large urban districts, an increase in charter percent is associated with increases in the White share of total enrollment, and also higher White isolation and White segregation in these districts. Similar effects hold for the segregation of Black and Hispanic students, although we detect no effects on the total share of enrollment of these groups. The charter share of nearby suburban school districts is not associated with compositional or sorting dynamics in large urban districts.

Conversely, columns (4)-(6) of Table 5 present estimates for smaller school districts, modeling outcomes as a function of their own charter share and that of nearby large districts. Charter presence in large urban districts nearby is associated with a decrease in the overall White and Black enrollment shares of smaller districts. For the case of Whites, the sign of this effect

Dallas, and Houston.

corresponds symmetrically with the evidence of the increased large urban White share in column (1). Additionally, charter growth in urban districts is associated with increases in the Hispanic share of the smaller districts.

Altogether, Table 5 provides evidence indicative that charter school growth causes enrollment flows between districts, although our data precludes us from studying flows directly. Further, the evidence suggests one explanation: White students moving from smaller districts to large urban ones, a "reverse white flight" effect. Patterns for other groups are less consistent, although we also detect evidence of between district dynamics for them. Nonetheless, since smaller districts tend to have higher White enrollment shares than large urban districts, this evidence is suggestive that one of the mechanisms leading to higher between-district integration following charter growth is an increase in White student flows from suburban to urban districts. The evidence also indicates potential flows of Black families to urban schools, which may be resulting in a rising share of Hispanic students in suburban districts.

At the same time, there is little indication that between-district integration flows translate to lower within-district segregation. The estimates show that increases in the White share of enrollment are juxtaposed with more white-isolated schools in large urban school districts. This may 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 instead provide a first look into the complexity of stratification dynamics caused by the growth of the charter school sector.

5.2 Treatment Effect Heterogeneity

The evidence presented thus far establishes that charter schools increase the racial and ethnic segregation of school districts. The average national effect is small in magnitude, which is likely both a function of charters' relatively small share of total enrollment and of treatment effect heterogeneity. One potential mediator in effect heterogeneity for school districts is urban/suburban difference in charter sorting. There is evidence of differences in student selection across 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 et al. 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 shown to be effective at raising student outcomes (Angrist et al. 2013, pp. 4). Such variability between urban and nonurban charter school sorting suggest that the effect of charter schools on segregation would be larger in urban than in suburban districts.

We test this hypothesis in Table 6, 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, interacting the charter percent variable with indicators for district urbanicity.[25] In column 2, we test whether impacts vary significantly by baseline district composition (2010 URM share of total enrollment), defined using three quantiles of the nationwide district distribution.[26] Columns 3 through 4 look a 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 charter's 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 school's impact on segregation, while positive and significant across the board, is larger in districts with a high URM share of total enrollment. We split the sample to show that the patterns in columns 1 and 2 mask important interaction effects, most prominently for White stratification. Notably, charters don't have a main effect on the segregation of Whites in urban districts; the impact is present only in urban districts with a relatively high URM 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 URM 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 URM 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 than we saw for urban school districts. For Black students, the suburban main effect is similar to the pooled models, and the interaction with higher URM is positive, albeit noisy and indistinguishable from zero. Similarly, Hispanic students attending suburban districts see a charter segregation main effect of about .07 percentage points, 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 impact 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 telling of the complexity of the sorting patterns caused by the charter sector. Also telling of complexity is the fact that charters have small to no segregation impacts in low minority urban districts.

Our results show that effect heterogeneity varies by location and district demographics. One policy implication of these findings is that policymakers concerned about segregation should

[25] 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".

[26] The average URM share in the Low quantile was 7.4%, for Mid it was 25.4%, and for High it was 65%.

be more wary of a growing charter sector in high-minority urban districts and low minority suburban districts, than in low minority urban districts and high minority suburban districts.

5.2.1 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 economically disadvantaged 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, it could also be the case that parental preferences over school racial composition vary by state.

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:

$$Y_{igt} = \sum_{s} \beta_s D_{s(i)} \times E_{igt} + X_{igt}^{'}\Gamma + \tau_{ig} + \delta_{it} + \gamma_{gt} + \epsilon_{igt}. \quad (6)$$

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 effects in this equation (and their confidence intervals) are reported in Figure 5, separately by racial grouping. These models use a restricted sample of states that have at least a 1% charter school share total public school enrollment at some point during the time period 1998-2018.

In 32 of the 37 states with sizable presence of charters, our point estimate of the effect of the charter share on Black or Hispanic segregation is positive, and a majority of these (19) are positive and statistically different from zero. 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 percentage point increase in segregation leads to a 0.25 percentage point increase in the segregation of Black and Hispanic students. In 2018, school districts in our sample for these states had average segregation levels of about 20 percentage points (and a mean charter share of 14%). Our estimates thus suggest that on average at least 18% of current school segregation levels in these states was caused by the charter school sector.

The results in Figure 5 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, in some cases alarmingly so.[27] 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 URM segregation include: Arizona, Florida, Illinois, Oregon, and Pennsylvania. Finally, for a couple 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 URM students in Connecticut. However, we strongly advise against the over-interpretation 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 present suggestive evidence of this channel in Figure 7.[28] 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%. The positive relationship between urban share of the charter sectore 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 charter share of minorities, defined as the ratio of the minority share of charter enrollment over the minority share of total public school enrollment. The plots in the bottom panels of Figure 7 show that states in which charters are more likely to educate minority students than other public schools tend to see higher increases in segregation as the charter school sector grows. This evidence is consistent with a theory in which states with more charter schools that are geared toward serving underrepresented minority students seeing a larger impact of the charter sector on school segregation, echoing some of the takeaways of the district effect heterogeneity estimates reported above.

6 Conclusion

Employing a triple differences 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 is highly heterogenous across different types of school systems and by state, highlighting the importance of nuance in interpreting our main estimates. Moreover, we provide evidence that

[27] For succinctness, we omit results for Asian students here. There is limited state heterogeneity on Asian segregation impacts, and as in Table 3, the coefficients are relatively small.

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

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 battery of robustness tests and measurement choices.

But it is worth noting that the normative implications of this result are complicated by the voluntary nature of charter school enrollment. If a family chooses a charter school because it focuses on students of special 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 they are more likely to have missions targeted at underserved students.

Simply put, segregation that takes place under a choice environment is fundamentally different to the type of school segregation that took place during the pre-*Brown* era of *de jure* segregation. Moreover, while segregation by race is a highly salient and important topic in U.S. 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 charter schools may increase socioeconomic segregation at levels comparable to our findings (Marcotte and Dalane 2019). Future work should continue to explore the effects of charter schools on the distribution of historically underserved students in school systems.

The 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. Our simulations suggest 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 (Table A1). Though this effect is small, the potential is greater in areas with significantly larger charter shares. Such efforts 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, Hawkins 2018a).

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 from broader geographical areas, such designs have the promise of using charters as agents for integration. While little research has yet to evaluate 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 strategically locating new schools of choice and increased funding for transportation (Hawkins, 2018a, 2018b). With the right design features, it is possible that school choice could become an agent of integration.

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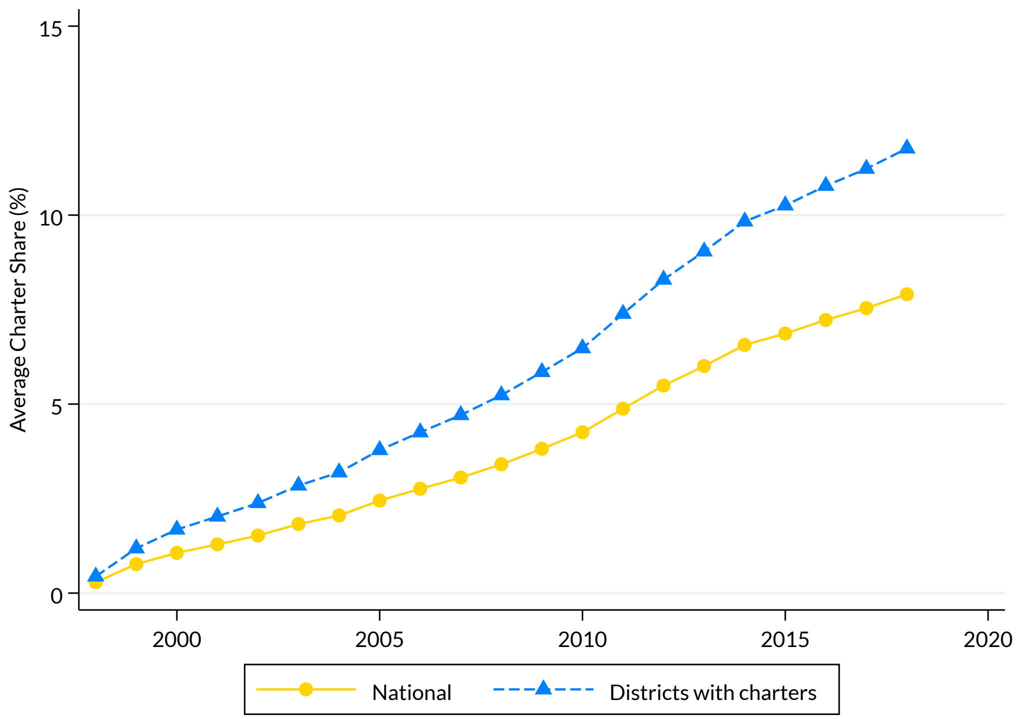
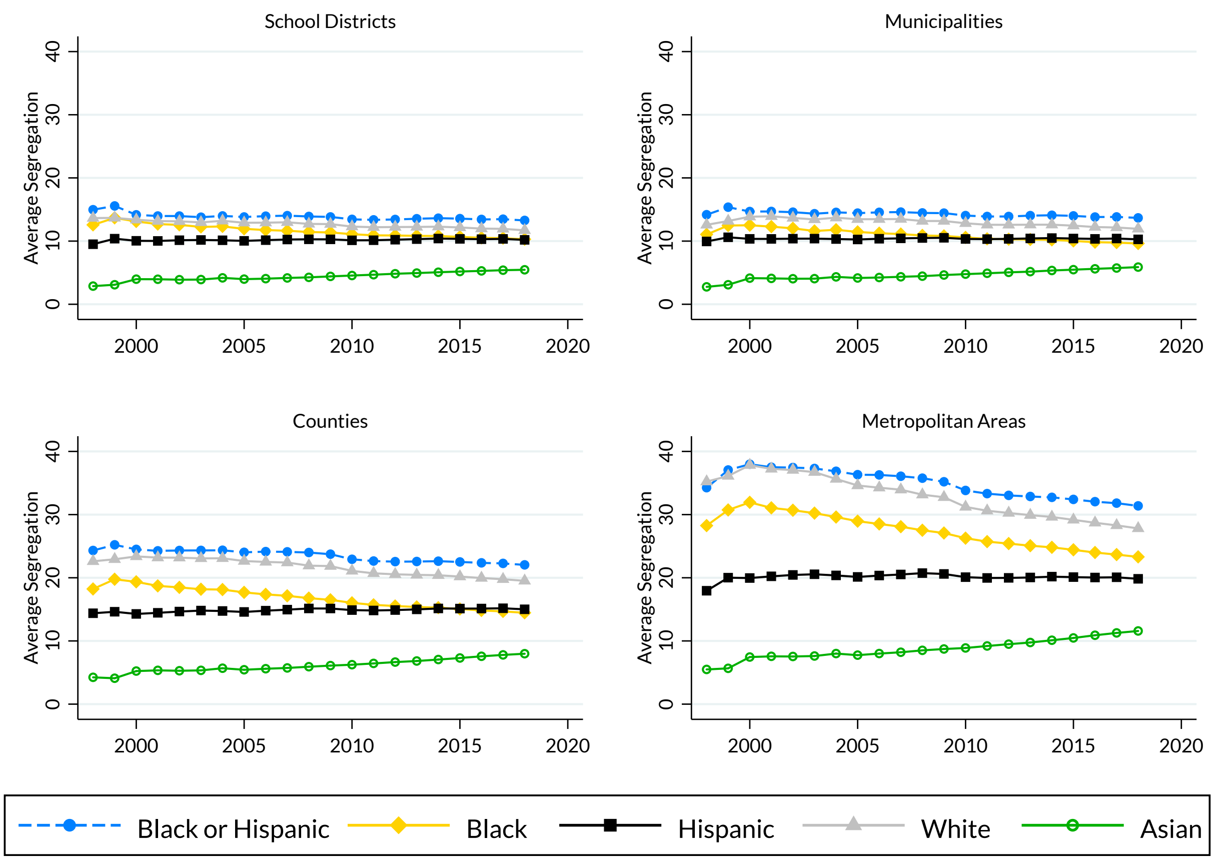
Figure 1: Mean Charter Share of Total Public School Enrollment 1998-2018



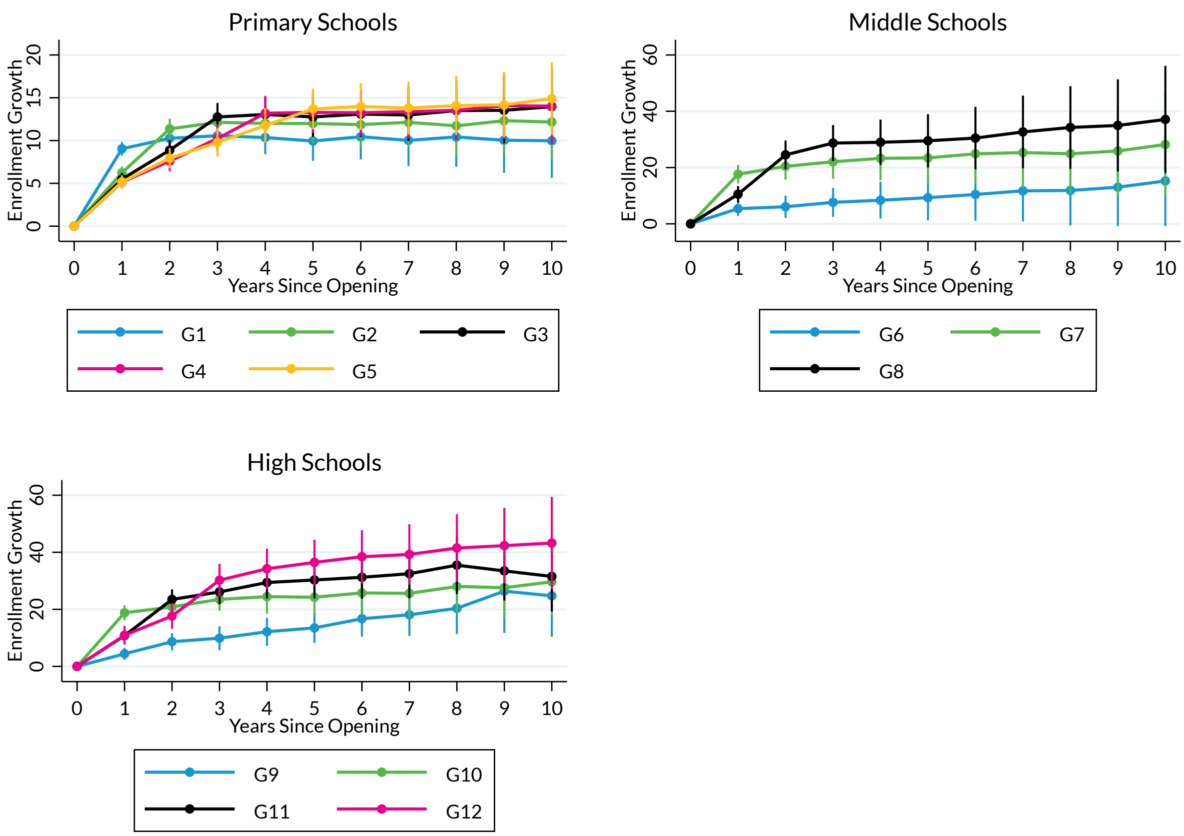
Note: Observations are weighted by enrollment. Districts with charters are those that have nonzero enrollment in a charter school during the entire sample period.

Figure 2: Trend in School Segregation Across Geographies and Racial Groups



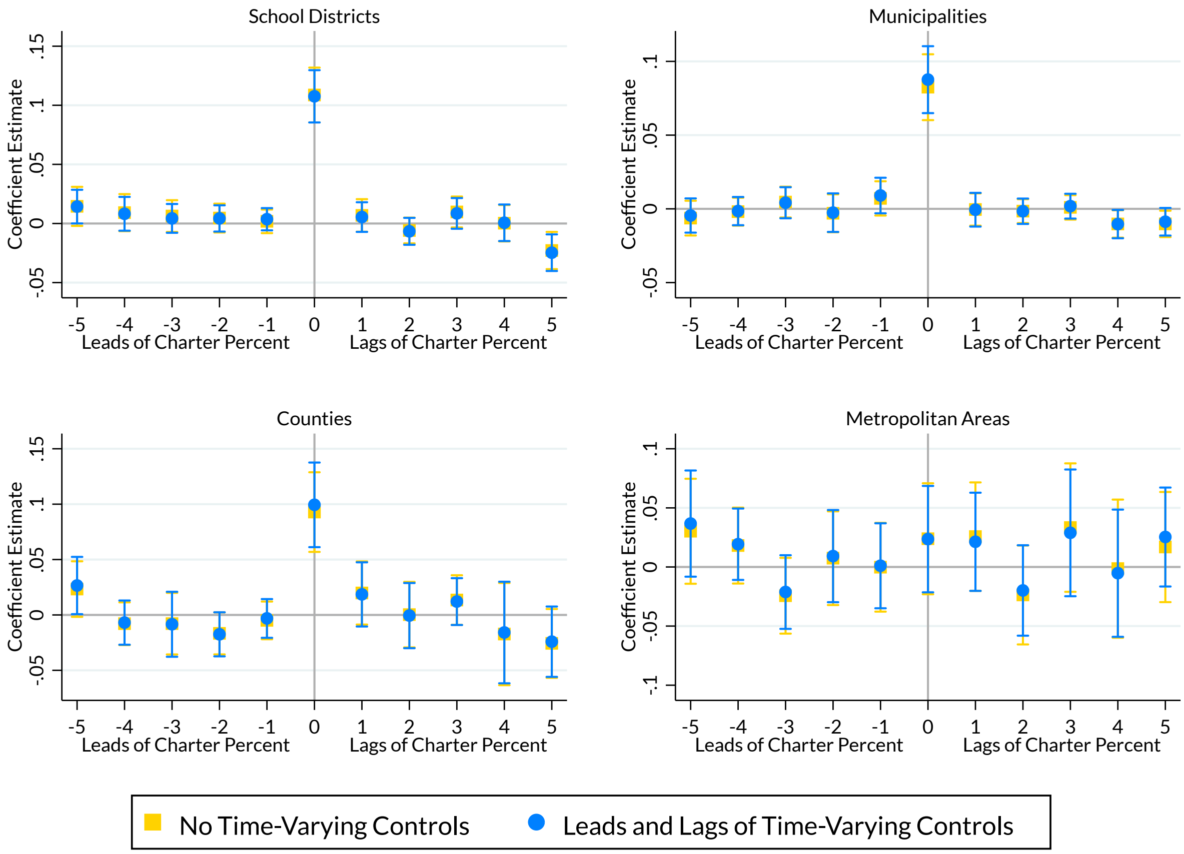
Note: Observations are weighted by enrollment. Segregation is defined with the variance ratio index (equation (1) in the text). See notes in Table 2 for geographic definitions.

Figure 3: Building a Charter Growth IV - Typical Enrollment Growth at Opening Charter Schools, by Grade



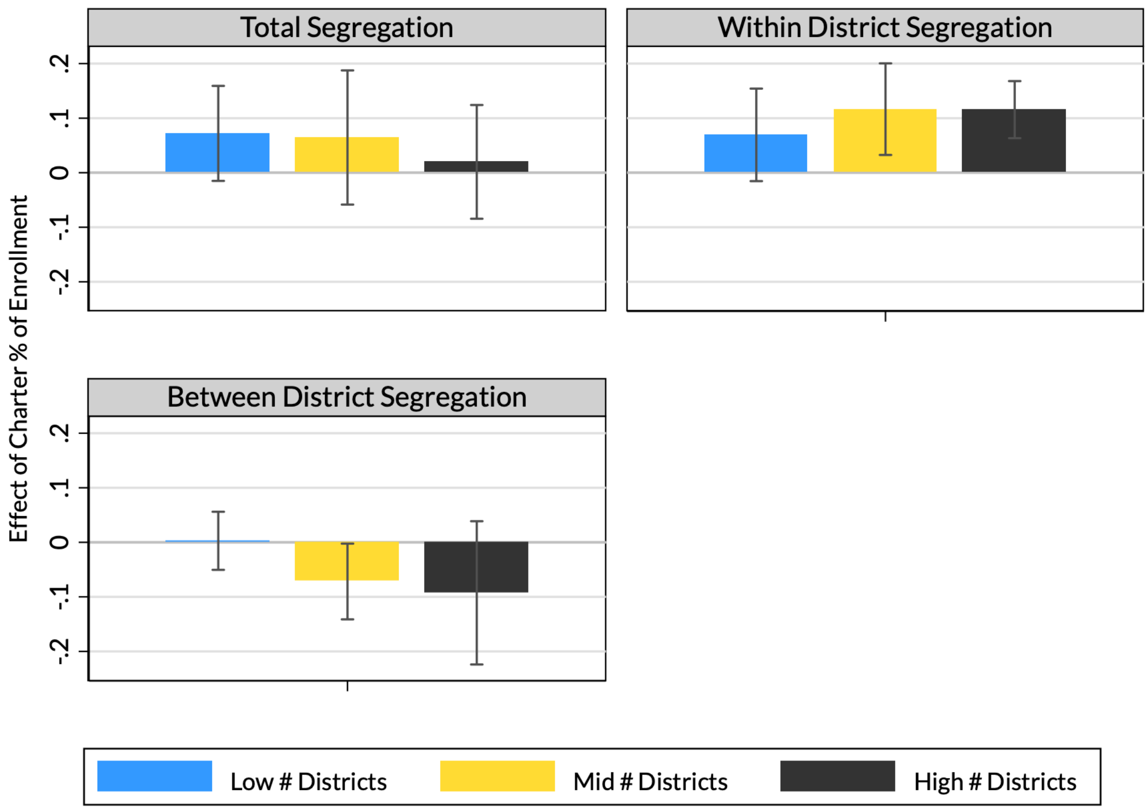
Note: Figure shows OLS coefficient estimates (and 95% confidence intervals) from an event study model of charter school enrollment as a function of years since opening, using a school-year panel of all charter schools in existence between 1998-2018. The model is estimated separately by grade, using the specification: $e_{sgt} = \sum_{k=1}^{10} \alpha_k D_{sg}^k + \phi_{sg} + \varphi_{tg} + \nu_{sgt}$, where e_{st} is the total 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.

Figure 4: Distributed Lag Models of Segregation



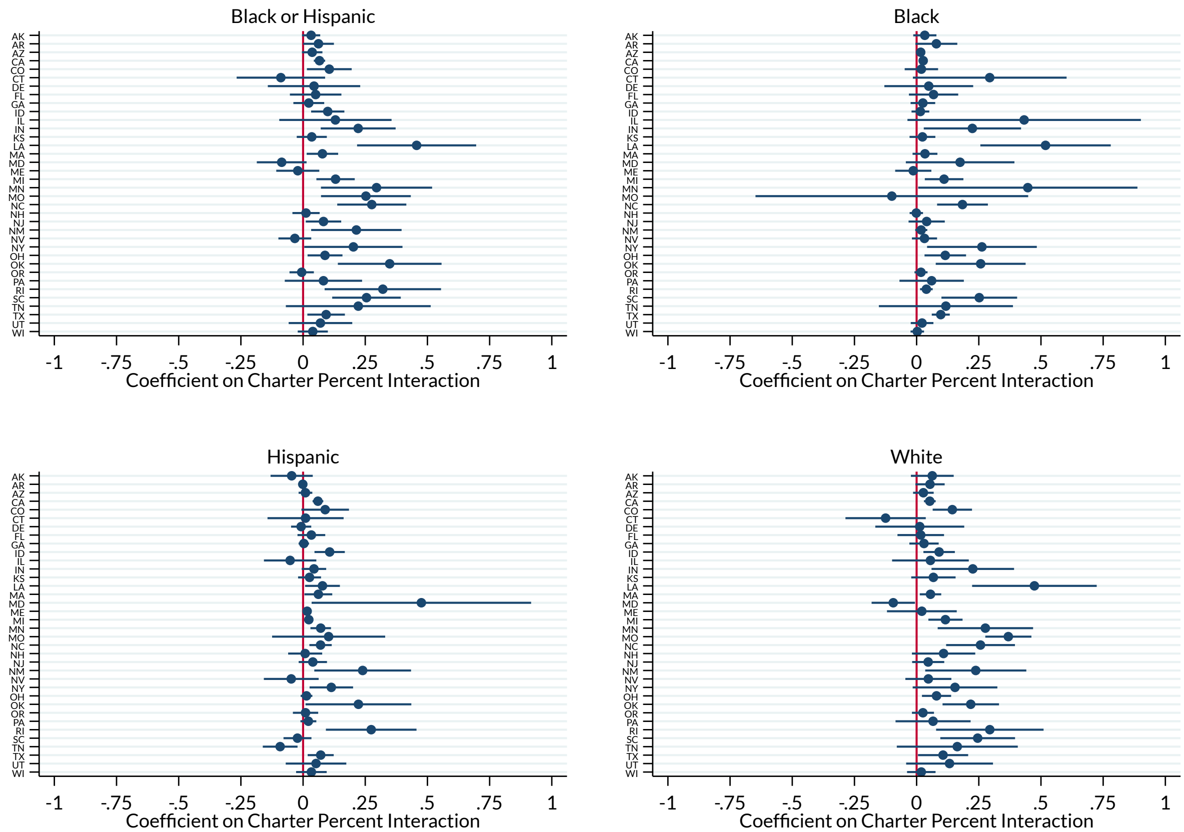
Note: Figure shows coefficient estimates from the distributed lag model defined in equation (3). 95% confidence intervals are based on standard errors clustered at the school system level. Panels show estimates for different geographic samples, see Table 2 for detailed definitions and sample restrictions. Covariates that vary at the system-grade-year level include: log total enrollment, group's share of total enrollment, and total number of schools serving grade.

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



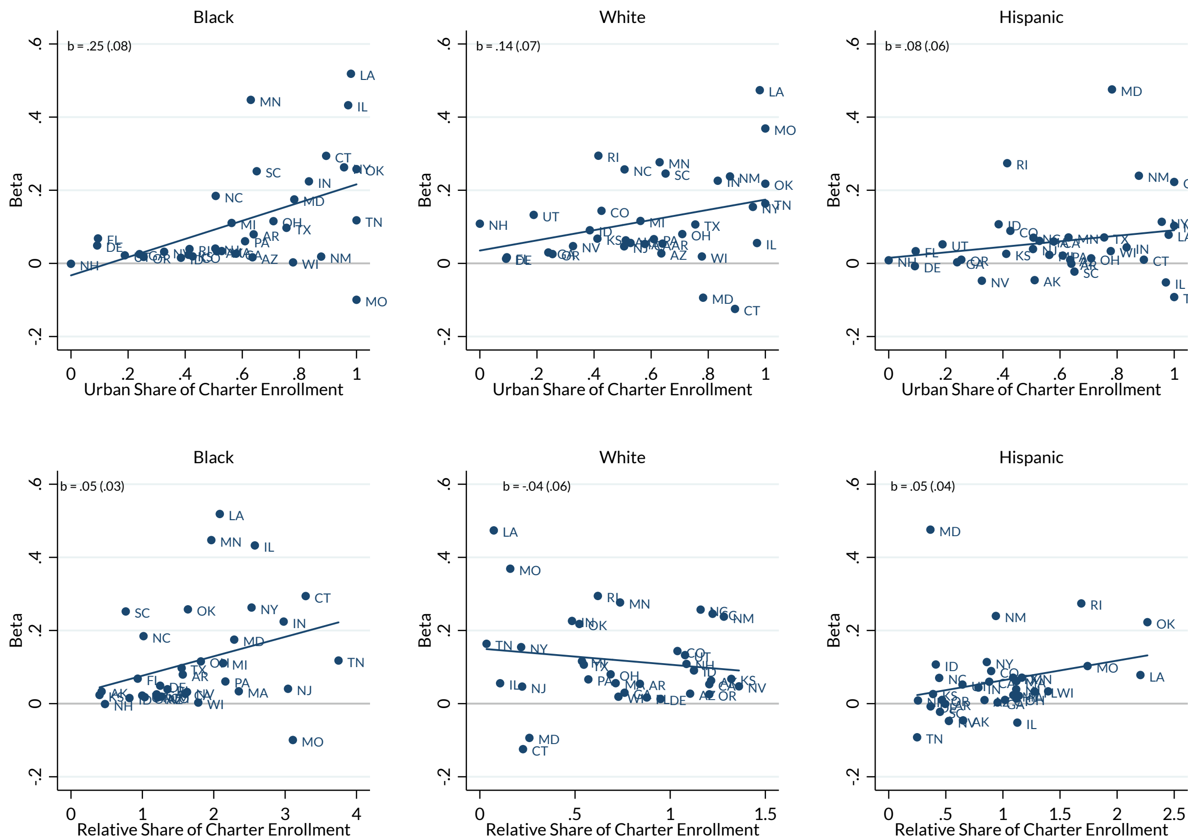
Note: Standard errors are clustered at the metropolitan area level in all models. Low fragmentation metropolitan areas have an average of 4.3 districts, mid fragmentation areas have 11.8, and high fragmentation areas have 53.9 school districts on average. Highly fragmented metropolitan areas encompass the most populous in the country, including New York, Los Angeles, Chicago, Dallas, and Houston. See Table 4 notes for estimation ans sample details.

Figure 6: The Effect of Charter Schools on Segregation, District Heterogeneity by State



Note: Standard errors are clustered at the school district level. Estimation sample is restricted to states that have at least 1% of total enrollment at charter schools at some point during the period 1998-2015. 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.

Figure 7: Potential Mediators of Effect Heterogeneity by State



Note: The vertical axis shows the state-specific estimate of the effect of charters on the segregation of a given group. The horizontal axis plots the 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.

Table 1: 2010 Census Tract Characteristics for Charter Schools and Traditional Public Schools (TPS)

	Primary		Middle		High	
	Charter	TPS	Charter	TPS	Charter	TPS
School Characteristics						
Total enrollment	353.94	552.00	262.16	769.58	312.02	1356.25
% black	0.35	0.23	0.36	0.23	0.26	0.28
% hispanic	0.25	0.32	0.34	0.30	0.31	0.26
% white	0.32	0.35	0.24	0.39	0.34	0.37
Census Tract Characteristics						
% black	0.25	0.19	0.28	0.17	0.18	0.20
% hispanic	0.24	0.24	0.28	0.23	0.27	0.21
% white	0.44	0.50	0.36	0.53	0.46	0.51
% adults with college	0.25	0.27	0.24	0.28	0.25	0.28
Median income	47558.17	55602.07	45053.10	56633.42	46319.76	54053.52
Observations	1958	19750	512	5174	809	4266

Note: SY 2010-11 NCES Common Core of Data geomatched to 2010 US census tract tabulations.

Table 2: 2018 Summary Statistics of System-Grade Estimation Samples

	School Districts		Municipalities		Counties		Metro 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 (1000's)	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)
% of system	10.4	(12.1)	10.7	(12.8)	8.9	(9.4)	10.0	(7.4)
Enrollment (thousands)	0.9	(2.3)	0.8	(2.2)	1.5	(3.3)	4.1	(5.0)
% of system	8.1	(11.6)	8.3	(12.4)	6.7	(8.2)	7.4	(5.9)
predicted %	6.3	(10.1)	6.5	(10.8)	5.3	(7.4)	5.7	(5.4)
Racial Composition (%)								
Black	17.3	(18.2)	17.1	(18.8)	15.0	(15.2)	15.9	(11.6)
Hispanic	30.4	(24.4)	30.9	(24.5)	27.5	(21.7)	29.5	(20.6)
Asian	6.0	(8.3)	6.4	(8.6)	5.5	(6.2)	6.1	(5.1)
White	41.1	(26.0)	40.4	(26.3)	46.7	(24.4)	43.5	(18.8)
Segregation								
Black or Hispanic (URM)								
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 Obs.	42,921		44,576		28,599		4,290	

Note: Observations are weighted by total enrollment. School district identifiers correspond to NCES geographic local education agencies. Charter schools are geo-matched to agency jurisdiction GIS data using their geo-located street address. Municipalities are defined using 2010 U.S. Census Place geographies, which include 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 are not. Metropolitan areas are defined according to 2010 U.S. Census CBSA definitions; they do not include micropolitan areas, and are not a full cover of the US. 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 defined in equation (2) are also not included in this sample.

Table 3: The Effect of Charter Schools on Segregation by Geography and Racial Group

Black or Hispanic	School Districts		Municipalities		Counties		Metro Areas	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
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	School Districts		Municipalities		Counties		Metro Areas	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
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	School Districts		Municipalities		Counties		Metro Areas	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
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	School Districts		Municipalities		Counties		Metro Areas	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
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	School Districts		Municipalities		Counties		Metro Areas	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
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)
Dep. Var. Mean	12.64		12.97		21.61		32.68	
DWH Endog. Test (p-val)		.973		.027		.247		.027
System-Year FE	X	X	X	X	X	X	X	X
System-Grade FE	X	X	X	X	X	X	X	X
State-Grade-Year FE	X	X	X	X	X	X	X	X
Covariates	X	X	X	X	X	X	X	X
N	831,042	831,042	847,078	847,078	554,162	554,162	86,212	86,212

Note: 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 that vary 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 Durbin-Wu-Hausman (DWH) test of endogeneity (equality between OLS and IV coefficients) for the URM (Black or Hispanic) segregation specifications is reported.

Table 4: Decomposition of Charter Schools' Effect on Metropolitan Area Segregation

Black or Hispanic	Total MA Segregation		Within District Segregation		Between District Segregation	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV
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	Total MA Segregation		Within District Segregation		Between District Segregation	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV
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	Total MA Segregation		Within District Segregation		Between District Segregation	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV
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	Total MA Segregation		Within District Segregation		Between District Segregation	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV
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	Total MA Segregation		Within District Segregation		Between District Segregation	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV
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)
Dep. Var. Mean	32.68		10.65		24.15	
District-Year FE	X	X	X	X	X	X
District-Grade FE	X	X	X	X	X	X
State-Grade-Year FE	X	X	X	X	X	X
Covariates	X	X	X	X	X	X
N	86,212	86,212	86,212	86,212	86,212	86,212

Note: 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 or Hispanic segregation). Within district segregation is defined as average school district segregation within the metropolitan area. Between district segregation is defined by assuming schools are perfectly representative of school district composition, but not necessarily metro composition. Covariates that vary 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.

Table 5: Mechanisms of Between-District Metropolitan Area Effects

White	Large Urban Districts			Other Urban/Suburban Districts		
	(1) Share	(2) Isolation	(3) Var. Ratio	(4) Share	(5) Isolation	(6) Var. Ratio
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)
Other 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 obs.	71,746	71,746	71,746	479,529	479,529	479,529

Black	Large Urban Districts			Other Urban/Suburban Districts		
	(1) Share	(2) Isolation	(3) Var. Ratio	(4) Share	(5) Isolation	(6) Var. Ratio
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)
Other 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 obs.	71,746	71,746	71,746	479,529	479,529	479,529

Hispanic	Large Urban Districts			Other Urban/Suburban Districts		
	(1) Share	(2) Isolation	(3) Var. Ratio	(4) Share	(5) Isolation	(6) Var. Ratio
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)
Other 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 obs.	71,746	71,746	71,746	479,529	479,529	479,529

Note: 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 75th percentile of the within-metro distribution of total district enrollment in 2010. Estimation panel samples in columns (4) through (6) include all suburban district in metropolitan areas, as well as the complement of large urban districts ('non-large' urban districts). Standard errors are clustered at the school district level in all models.

Table 6: Effect Heterogeneity by Baseline District Characteristics

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

Note: 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 URM total enrollment share categories are based on three quantiles of the 2010 distribution of total district enrollment.

A Placebo Test

Our main identification strategy relies on a generalized differences-in-differences design linking changes in charter share of district-grade enrollment to changes in district-grade segregation. This method identifies the effect of charters using between-grade comparisons within districts that see differing levels of charter penetration at different grade levels over time. However, it is possible that unobserved factors are simultaneously associated with charter school growth in particular grade levels and with corresponding increases in segregation. Though our method inherently addresses many potential confounding explanations, we test the validity of these assumptions with an informative robustness check.

We conduct a placebo test exploiting the structure of our district-by-grade-by-year level dataset. The placebo test is based on the notion that increases in charter percent of enrollment in, say, primary schools should not have a direct contemporaneous impact on the racial segregation of high schools. We conduct this test by restructuring our data wide by grade, resulting in a district-year panel with segregation and charter enrollment measured for different grade levels. We then estimate the following class of models:

$$Y_{it}^{g} = \sum_{g=0}^{12} \beta_g E_{it}^{g} + X_{it}^{'}\Gamma + \tau_i + \delta_{s(i)t} + \epsilon_{it} \tag{7}$$

where E_{it}^{g} is charter percent of enrollment for groups of school grades $g = 0, 1, 2, ..., 12$, and grade 0 corresponds to kindergarten (KG). The models control for district fixed effects, state-by-year effects, and separately for each grade: number of schools, log total enrollment, and share Black or Hispanic.

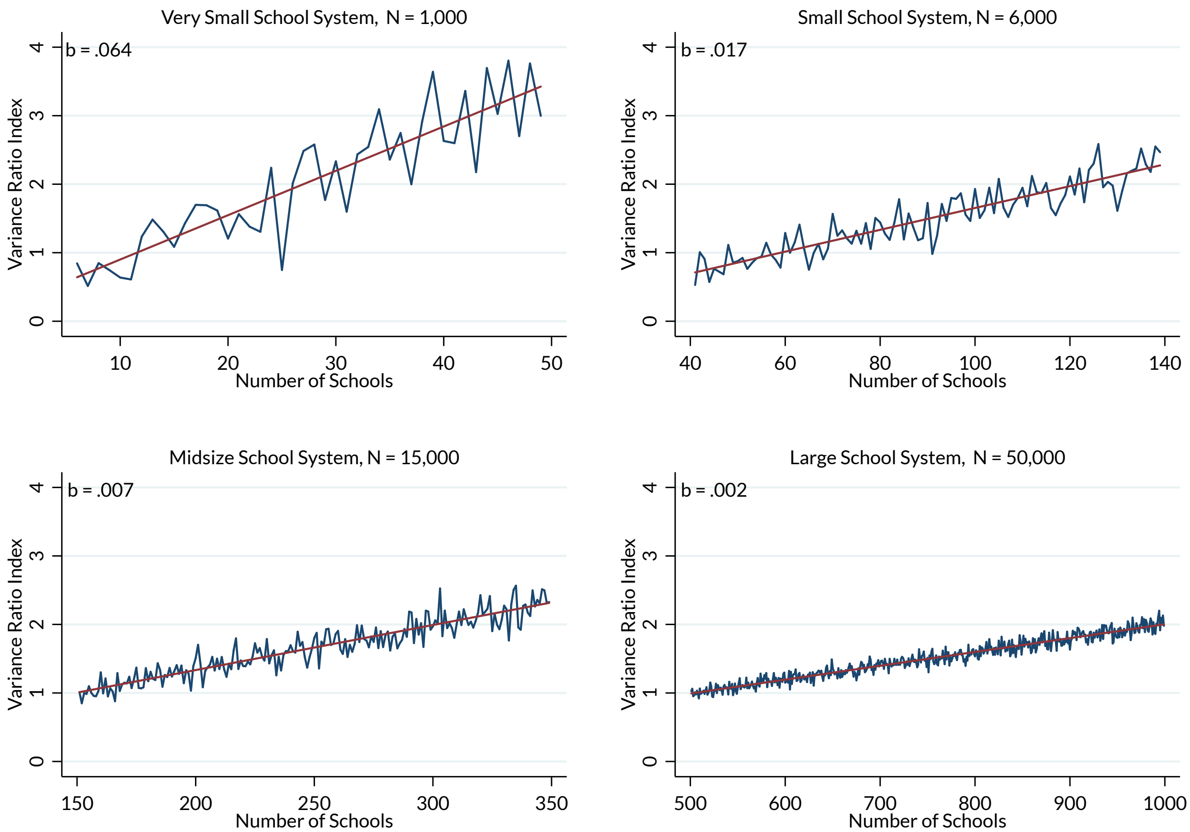
When estimating models of segregation in grade g, the test requires estimates of β_g similar to our main estimates (assuming between-grade effect heterogeneity is minimal) and null effects for any $\beta_{g'}$ with $g \neq g'$. However, one could also imagine that off-diagonal coefficients with $g' < g$ (the lower triangle in table A2) could capture "preemptive effects", where student sorting patterns react to the growth of charter schools in higher grades as families choose the educational trajectory of their children. Under this view, significant patterns in the upper-triangle of the placebo test matrix are more worrisome in terms of threats to our research design, as they cannot be explained by preemptive behavior and are more likely to be an indication of endogeneity of the treatment variable.

The results of the placebo test in Table A2 provide strong evidence that the relationships we have identified in our main models are well-identified causal relationships. Across 169 coefficients in 13 separate models, the results are generally significant where one would expect them to be if the effect was strictly contemporaneous, unbiased, and under minimal preemptive effects. For grades 4 through 12, the estimated effect of the own-grade (the diagonal of the matrix) are positive, of similar magnitude to our main estimates, and statistically significant. This is not the case for earlier grades, suggesting that there are smaller effects at the earliest elementary school grades. In contrast, the off-diagonal elements are of smaller magnitude and not statistically different from zero in the vast majority of cases. In the lower-triangle, only 3 coefficients are significant at the 5% level; In the upper-triangle, 4 coefficients are.

We formalize the placebo test by computing an F-statistic for the joint significance of the upper- and lower-triangle coefficients, separately. We report the p-values for these tests at the bottom of Table A2. Encouragingly, a large majority of these tests come back insignificant at conventional levels of statistical confidence. For about 3 or 4 of these models, we find that the lower triangle is jointly significant, depending on the level of confidence. These are concentrated at the high school level, consistent with preemptive sorting effects for charter high schools. On the other hand, none of the F-tests are significant for the upper triangle.

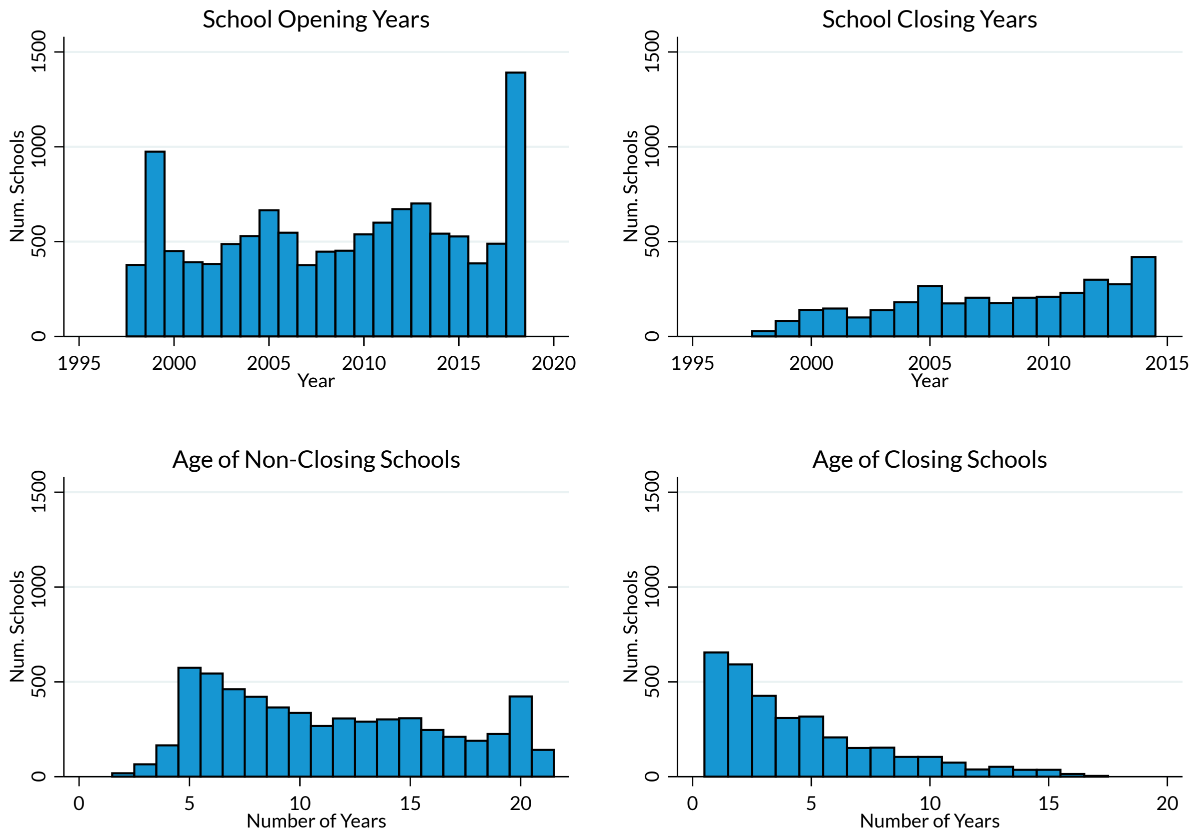
Altogether, the breadth of the evidence suggests that our models "pass the placebo" test, providing further evidence and confidence that our estimates can be interpreted causally without major reservations.

Figure A.1: Mechanical Effect of School Additions on Segregation Index



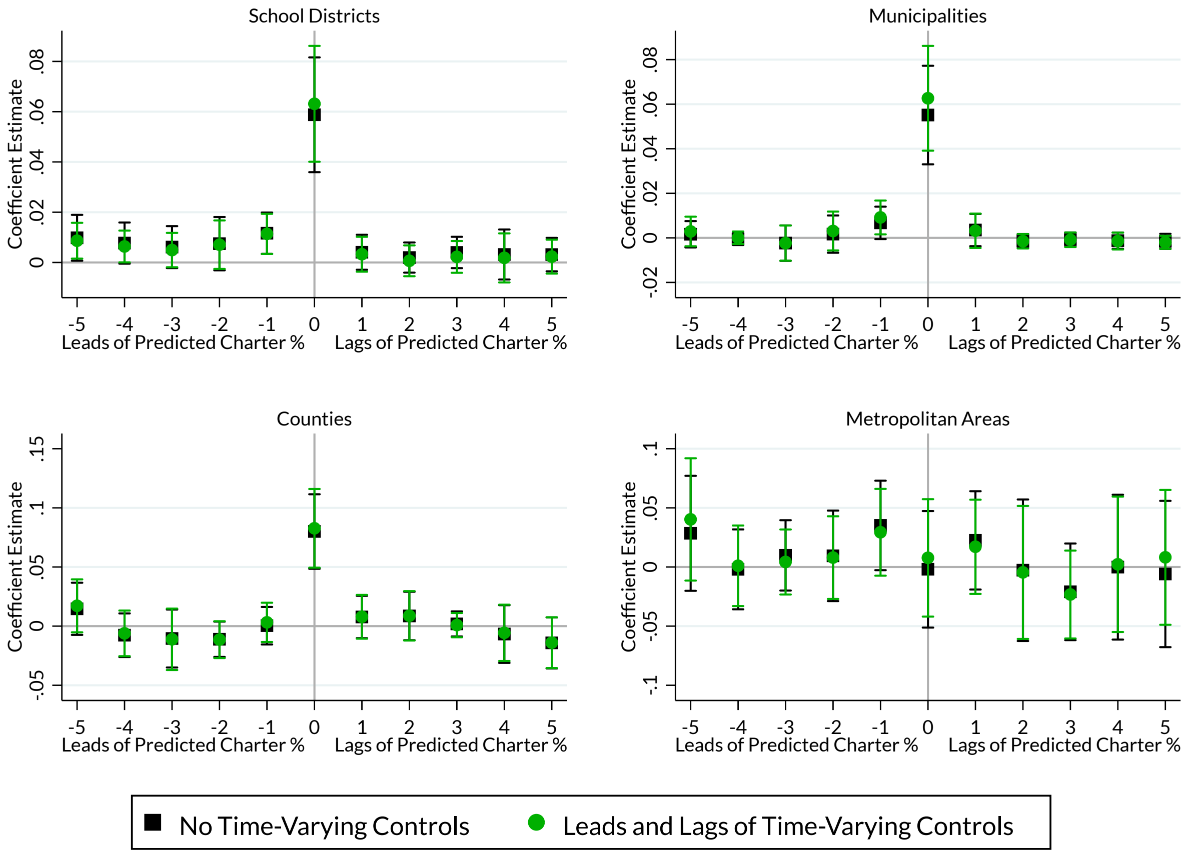
Note: Figure shows simulations of school segregation with random school assignment, for hypothetical school systems of different total population. It is assumed 50% of the population is from the "minority" group. Simulations gradually increase the number of schools in the system. Every time a school is added, random school assignment is conducted.

Figure A.2: Distribution of Charter School Opening and Closing Events



Note: School-level histograms of the cross-section of all charter schools in operation between 1998-2018. The top left panel shows the distribtuion of charter opening years, the first year they report enrollment to the Common Core of Data. The bottom left panel shows the distribution of current age of charter school that have not ceased operations. The right top panel shows the distribution of charter closure years. The bottom right panel shows the age of charter schools that close prior to 2018.

Figure A.3: Reduced Form Pre-Trend Test – Distributed Lag Models of Segregation and Predicted Charter Percent



Note: Distributed lag models. The independent variables are leads and lags of school systems' predicted charter percent of enrollment, which is computed using the fitted values from the enrollment growth models shown in Figure 3. Standard errors are clustered at the system level in all specifications.

Table A.1: Randomization-Based Falsification Test - What if charter enrollment was randomly drawn from district schools?

Black or Hispanic	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8) IV
Charter percent	-0.0111	-0.0394***	-0.0365***	-0.0325***	-0.0326***	-0.0326***	-0.0328***	-0.0394***
	(0.0102)	(0.0097)	(0.0084)	(0.0082)	(0.0082)	(0.0082)	(0.0082)	(0.0097)
Black	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8) IV
Charter percent	-0.0189**	-0.0347***	-0.0159**	-0.0160**	-0.0160**	-0.0160**	-0.0176**	-0.0125
	(0.0090)	(0.0087)	(0.0069)	(0.0067)	(0.0067)	(0.0067)	(0.0068)	(0.0084)
Hispanic	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8) IV
Charter percent	0.0390***	-0.0224***	-0.0224***	-0.0182***	-0.0178***	-0.0178***	-0.0171***	-0.0307***
	(0.0089)	(0.0052)	(0.0062)	(0.0061)	(0.0062)	(0.0062)	(0.0061)	(0.0073)
Asian	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8) IV
Charter percent	0.0037	-0.0044	0.0031	0.0042	0.0052	0.0052	0.0063	0.0019
	(0.0042)	(0.0036)	(0.0054)	(0.0056)	(0.0057)	(0.0057)	(0.0062)	(0.0067)
White	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8) IV
Charter percent	-0.0338***	-0.0539***	-0.0361***	-0.0318***	-0.0322***	-0.0322***	-0.0334***	-0.0412***
	(0.0090)	(0.0093)	(0.0093)	(0.0094)	(0.0094)	(0.0094)	(0.0089)	(0.0116)
Dep. Var. Mean	11.07							
Year FE		X						
District FE		X						
Grade FE		X						
District-Year FE			X	X	X	X	X	X
District-Grade FE			X	X	X	X	X	X
State-Grade-Year FE				X	X	X	X	X
Population Ctrl.					X	X	X	X
Num. Schools Ctrl.						X	X	X
Composition Ctrl.							X	X
R^2	0.001	0.668	0.884	0.894	0.894	0.894	0.895	0.014
N	834,555	834,527	831,059	831,042	831,042	831,042	831,042	831,042

Note: Standard errors are clustered at the school district level in all models. The dependent variable is a transformed segregation index, estimated assuming a counterfactual in which charter school enrollment is a random draw (with replacement) from the non-charter school student population in a given school system, grade and year.

Table A.2: Placebo Test – Effect of Charters on Segregation Across Different School Grade Levels

	(1) KG	(2) G1	(3) G2	(4) G3	(5) G4	(6) G5	(7) G6	(8) G7	(9) G8	(10) G9	(11) G10	(12) G11	(13) G12
Charter % KG	-0.05	-0.01	-0.01	-0.01	-0.02	0.01	0.01	-0.01	-0.01	-0.01	-0.04**	-0.03	-0.03*
	(0.05)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Charter % G1	0.02	0.05	-0.04	-0.03	-0.04	-0.04	-0.06*	-0.03	0.01	-0.01	0.04	-0.02	0.01
	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.02)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)
Charter % G2	0.13***	0.06*	0.14***	0.01	-0.01	-0.03	0.01	-0.01	-0.04	0.03	0.00	0.04	0.03
	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)
Charter % G3	0.02	0.02	0.02	0.06	0.05	0.03	0.01	0.02	0.01	-0.00	-0.04	-0.01	-0.02
	(0.03)	(0.03)	(0.03)	(0.05)	(0.04)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Charter % G4	-0.02	-0.02	-0.00	0.06**	0.08***	0.03	0.02	0.03	0.02	-0.00	0.06*	0.01	0.03
	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
Charter % G5	-0.04	-0.02	-0.04	-0.04	-0.01	0.08**	0.01	-0.00	0.01	-0.03	-0.04	-0.01	-0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.04)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)
Charter % G6	0.04	0.01	0.02	0.01	0.01	-0.03	0.07***	-0.02	-0.04	0.00	-0.02	-0.03	-0.02
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
Charter % G7	-0.01	0.00	0.03	0.01	0.03	0.03	0.04	0.13***	0.03	-0.02	-0.00	-0.01	0.01
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Charter % G8	-0.02	-0.02	-0.04*	-0.03	-0.03	-0.01	0.01	0.02	0.13***	0.06**	0.05*	0.06**	0.03
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Charter % G9	0.00	-0.00	0.01	0.00	0.01	0.01	0.02	-0.01	0.01	0.13***	0.03*	0.04**	0.05**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Charter % G10	-0.01	0.01	-0.00	0.01	0.00	0.01	0.01	0.02	0.01	-0.02	0.13***	-0.01	-0.00
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Charter % G11	-0.02	-0.02	-0.01	-0.01	-0.00	0.00	-0.02	0.03	0.03	0.04*	-0.02	0.13***	-0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
Charter % G12	0.02	0.03*	0.01	0.01	0.00	-0.00	0.01	-0.01	-0.01	-0.03**	-0.03	-0.05**	0.07***
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)
State-by-Year FE	X	X	X	X	X	X	X	X	X	X	X	X	X
District FE	X	X	X	X	X	X	X	X	X	X	X	X	X
Covariates	X	X	X	X	X	X	X	X	X	X	X	X	X
Lower-Triangle F (p-value)	.028	.599	.694	.354	.882	.824	.235	.372	.212	.1	.042	.015	
Upper-Triangle F (p-value)		.385	.218	.713	.512	.733	.732	.796	.784	.597	.112	.355	.314
R^2	0.86	0.88	0.88	0.86	0.88	0.87	0.85	0.84	0.84	0.82	0.83	0.83	0.81
Total Obs.	35,301	35,450	35,400	35,394	35,405	35,419	35,351	35,347	35,299	35,257	35,182	35,099	34,944

Note: Standard errors are clustered at the district level.

Table A.3: First Stage and Reduced Form Estimates

Black or Hispanic	School Districts		Municipalities		Counties		Metro Areas	
	(1) FS	(2) RF	(3) FS	(4) RF	(5) FS	(6) RF	(7) FS	(8) RF
Pred. charter percent	0.585***	0.054***	0.603***	0.053***	0.767***	0.087***	0.886***	0.073***
	(0.039)	(0.008)	(0.042)	(0.008)	(0.029)	(0.017)	(0.022)	(0.027)
Black	School Districts		Municipalities		Counties		Metro Areas	
	(1) FS	(2) RF	(3) FS	(4) RF	(5) FS	(6) RF	(7) FS	(8) RF
Pred. charter percent	0.584***	0.052***	0.603***	0.042***	0.767***	0.082***	0.885***	0.057**
	(0.039)	(0.008)	(0.042)	(0.006)	(0.029)	(0.019)	(0.022)	(0.028)
Hispanic	School Districts		Municipalities		Counties		Metro Areas	
	(1) FS	(2) RF	(3) FS	(4) RF	(5) FS	(6) RF	(7) FS	(8) RF
Pred. charter percent	0.585***	0.021***	0.603***	0.029***	0.767***	0.037***	0.885***	0.044**
	(0.039)	(0.005)	(0.042)	(0.006)	(0.029)	(0.012)	(0.022)	(0.021)
Asian	School Districts		Municipalities		Counties		Metro Areas	
	(1) FS	(2) RF	(3) FS	(4) RF	(5) FS	(6) RF	(7) FS	(8) RF
Pred. charter percent	0.585***	0.026***	0.603***	0.016***	0.767***	0.012*	0.886***	0.027*
	(0.039)	(0.005)	(0.042)	(0.004)	(0.029)	(0.006)	(0.022)	(0.015)
White	School Districts		Municipalities		Counties		Metro Areas	
	(1) FS	(2) RF	(3) FS	(4) RF	(5) FS	(6) RF	(7) FS	(8) RF
Pred. charter percent	0.585***	0.050***	0.603***	0.050***	0.767***	0.083***	0.885***	0.082***
	(0.039)	(0.008)	(0.042)	(0.007)	(0.029)	(0.017)	(0.022)	(0.030)
System-Year FE	X	X	X	X	X	X	X	X
System-Grade FE	X	X	X	X	X	X	X	X
State-Grade-Year FE	X	X	X	X	X	X	X	X
Covariates	X	X	X	X	X	X	X	X
N	831,042	831,042	847,078	847,078	554,162	554,162	86,212	86,212

Note: Standard errors are clustered at the school district level in all models. Covariates are log total enrollment and the enrollment share of the group.

Table A.4: Effects on the Dissimilarity Index of Segregation

Black or Hispanic	School Districts		Municipalities		Counties		Metro Areas	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Charter percent	0.197***	0.200***	0.173***	0.187***	0.139***	0.158***	-0.011	0.017
	(0.015)	(0.020)	(0.015)	(0.018)	(0.026)	(0.031)	(0.034)	(0.038)
Black	School Districts		Municipalities		Counties		Metro Areas	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Charter percent	0.235***	0.261***	0.185***	0.201***	0.163***	0.182***	0.118**	0.150***
	(0.019)	(0.026)	(0.016)	(0.019)	(0.034)	(0.044)	(0.050)	(0.055)
Hispanic	School Districts		Municipalities		Counties		Metro Areas	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Charter percent	0.157***	0.147***	0.141***	0.146***	0.103***	0.111***	0.016	0.018
	(0.013)	(0.019)	(0.014)	(0.016)	(0.024)	(0.030)	(0.034)	(0.040)
Asian	School Districts		Municipalities		Counties		Metro Areas	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Charter percent	0.230***	0.240***	0.191***	0.213***	0.161***	0.198***	0.119**	0.114*
	(0.018)	(0.026)	(0.018)	(0.024)	(0.034)	(0.042)	(0.052)	(0.061)
White	School Districts		Municipalities		Counties		Metro Areas	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Charter percent	0.190***	0.197***	0.169***	0.185***	0.110***	0.129***	-0.006	0.042
	(0.015)	(0.019)	(0.015)	(0.018)	(0.026)	(0.030)	(0.039)	(0.047)
Dep. Var. Mean	32.69		32.97		43.02		52.35	
System-Year FE	X	X	X	X	X	X	X	X
System-Grade FE	X	X	X	X	X	X	X	X
State-Grade-Year FE	X	X	X	X	X	X	X	X
Covariates	X	X	X	X	X	X	X	X
N	831,042	831,042	847,078	847,078	554,162	554,162	86,212	86,212

Note: Standard errors are clustered at the school system level in all models. Covariates are log total enrollment, number of schools, and the enrollment share of the group. Dissimilarity is defined as $D = \sum_k \frac{p_k|q_k - Q|}{2PQ(1-Q)}$, where k indexes schools, p_k is total school enrollment, q_k is the group share of enrollment at the school, Q is the group share of school system enrollment, and P is total school system population.

Table A.5: Effects by Quartiles of District Total Enrollment

Black or Hispanic	Q1		Q2		Q3		Q4	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Charter percent	0.053***	0.073***	0.120***	0.123***	0.098***	0.083***	0.106***	0.096***
	(0.010)	(0.021)	(0.022)	(0.026)	(0.018)	(0.026)	(0.018)	(0.019)
Black	Q1		Q2		Q3		Q4	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Charter percent	0.034***	0.086***	0.114***	0.131***	0.073***	0.062***	0.067***	0.072***
	(0.009)	(0.022)	(0.026)	(0.030)	(0.016)	(0.022)	(0.014)	(0.018)
Hispanic	Q1		Q2		Q3		Q4	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Charter percent	0.023***	0.010	0.053***	0.051***	0.040***	0.031**	0.055***	0.049***
	(0.006)	(0.013)	(0.012)	(0.014)	(0.008)	(0.015)	(0.011)	(0.012)
Asian	Q1		Q2		Q3		Q4	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Charter percent	0.015**	0.034***	0.023***	0.021***	0.040**	0.039*	0.054***	0.058***
	(0.008)	(0.011)	(0.006)	(0.008)	(0.019)	(0.020)	(0.009)	(0.011)
White	Q1		Q2		Q3		Q4	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Charter percent	0.058***	0.081***	0.107***	0.112***	0.088***	0.072***	0.087***	0.082***
	(0.011)	(0.018)	(0.020)	(0.024)	(0.017)	(0.025)	(0.021)	(0.022)
Mean LEA Population	2975.38		3953.84		17505.1		119275	
District-Year FE	X	X	X	X	X	X	X	X
District-Grade FE	X	X	X	X	X	X	X	X
State-Grade-Year FE	X	X	X	X	X	X	X	X
Covariates	X	X	X	X	X	X	X	X
R^2	0.866	0.018	0.864	0.021	0.879	0.013	0.944	0.010
N	204,245	204,245	207,615	207,615	208,240	208,240	208,223	208,223

Note: Standard errors are clustered at the school system level in all models. Covariates are log total enrollment, number of schools, and the enrollment share of the group.

Table A.6: Gradual Addition of Fixed Effects and other Control Variables

Black or Hispanic	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8) IV
Charter percent	0.1467***	0.1012***	0.0915***	0.0953***	0.0950***	0.0949***	0.0942***	0.0925***
	(0.0160)	(0.0090)	(0.0093)	(0.0093)	(0.0092)	(0.0092)	(0.0091)	(0.0121)
Black	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8) IV
Charter percent	0.1202***	0.0715***	0.0753***	0.0768***	0.0766***	0.0766***	0.0733***	0.0882***
	(0.0167)	(0.0078)	(0.0094)	(0.0095)	(0.0094)	(0.0094)	(0.0092)	(0.0122)
Hispanic	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8) IV
Charter percent	0.1076***	0.0499***	0.0393***	0.0427***	0.0430***	0.0430***	0.0439***	0.0356***
	(0.0132)	(0.0060)	(0.0049)	(0.0048)	(0.0049)	(0.0049)	(0.0048)	(0.0070)
Asian	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8) IV
Charter percent	0.0460***	0.0344***	0.0340***	0.0319***	0.0329***	0.0329***	0.0343***	0.0384***
	(0.0062)	(0.0044)	(0.0058)	(0.0058)	(0.0059)	(0.0059)	(0.0064)	(0.0072)
White	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8) IV
Charter percent	0.1348***	0.0932***	0.0817***	0.0868***	0.0864***	0.0863***	0.0853***	0.0851***
	(0.0157)	(0.0087)	(0.0091)	(0.0090)	(0.0090)	(0.0090)	(0.0089)	(0.0116)
Dep. Var. Mean	12.64							
Year FE		X						
District FE		X						
Grade FE		X						
District-Year FE			X	X	X	X	X	X
District-Grade FE			X	X	X	X	X	X
State-Grade-Year FE				X	X	X	X	X
Population Ctrl.					X	X	X	X
Num. Schools Ctrl.						X	X	X
Composition Ctrl.							X	X
R^2	0.017	0.693	0.892	0.901	0.901	0.901	0.902	0.016
N	834,555	834,527	831,059	831,042	831,042	831,042	831,042	831,042

Note: Standard errors are clustered at the school district level in all models.

Table A.7: Effects on Theil's H Index of Multi-Group Entropy

	School Districts		Municipalities		Counties		Metro Areas	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Charter percent	0.090***	0.092***	0.075***	0.082***	0.083***	0.090***	0.045**	0.059**
	(0.007)	(0.009)	(0.007)	(0.009)	(0.018)	(0.022)	(0.022)	(0.028)
Dep. Var. Mean	13.7		13.67		20.46		28.09	
System-Year FE	X	X	X	X	X	X	X	X
System-Grade FE	X	X	X	X	X	X	X	X
State-Grade-Year FE	X	X	X	X	X	X	X	X
Covariates	X	X	X	X	X	X	X	X
N	831,042	831,042	847,078	847,078	554,162	554,162	86,212	86,212

Note: Standard errors are clustered at the school district level in all models. Theil's H index of multi-group entropy index is defined as $H = \sum_k \frac{p_k(E-E_k)}{EP}$ where entropy $E_k = \sum_r \pi_{rk} ln(1/\pi_{rk})$ and π_{rk} is group r's share of enrollment in school k; E is the entropy of the school system; p_k is school enrollment, and P is total system population. See (Iceland 2004)