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INTRODUCTION

Public school choice policies allow families to choose traditional public, charter, or magnet schools, and 18 states provide some form of private school vouchers or tax credits (EdChoice, 2019). Because of these policies, in part, 29 percent of students now attend schools to which they are not residentially zoned (US Department of Education, 2019). Charter schools—privately managed but publicly funded schools—account for the largest growth in unzoned enrollment, and currently educate 5 percent of K-12 students.

While the effects of school choice policies on student outcomes have received enormous attention, their implications for the distribution of students across public and private sectors, as well as within the public sector, remains unclear. This question is important for at least three reasons. First, school quality is partially constituted by the characteristics of other students, not only because multiple studies have demonstrated the presence of peer effects (Hoxby, 2000; Sacerdote, 2001; Legewie & DiPrete, 2012), but also because a social goal of public education is to facilitate social integration (Reardon & Owens, 2014). Second, teachers choose workplaces based on student characteristics, with high concentrations of Black, Hispanic, and poor students affecting the workforce schools can attract and retain (Boyd et al., 2005; Clotfelter et al., 2006). Third, research from fields beyond education suggests that political support for state-funded goods is also affected by users' characteristics. This claim is well-substantiated by studies that consider the effects of universal versus means-tested services on quality, distributional impacts, and operational longevity (Pampel & Williamson, 1988; Fox, 2004, 2012).

In this paper, we investigate how increased school choice has affected private school enrollment and school segregation within the public sector. To do so, we compile a 22-year panel (1993-94 through 2015-16 school years) which includes school enrollment data matched to county-level demographic, political, and economic data. These data allow us to examine enrollment change in traditional public, charter, magnet, and private schools. Using an event-study design, we first examine the determinants of charter growth, which is both an object of interest in itself, and important for assessing potential sources of bias in our estimation strategy. We establish that two contextual factors—local economic downturn and increasing school-aged population—precede charter growth, while changes in academic performance, racial composition, and partisan voting patterns do not.

Turning to the effects of charter growth on the distribution of students, we find that charter enrollment growth negatively and significantly affects private school enrollment, increasing the proportion of students who do not qualify for free and reduced price-lunch students (hereafter, referred to as "non-poor") in the public school system. We estimate that for every ten students enrolled in a charter school, at least one would have otherwise attended a private school. We then present robust evidence that charter enrollment growth leads to racial and especially socioeconomic school segregation, a finding that is partially explained by non-poor students' transition from the private to public sector. Examining heterogeneity at earlier and later charter growth stages, we show that charter growth's effect on racial segregation is positive only at early growth stages. However, the effect of charter growth on socioeconomic school segregation persists steadily at later stages. Finally, we replicate this analysis for magnet schools, which have admissions practices intended to enroll a diverse group of students, and find no evidence that magnet growth affects racial or socioeconomic school segregation.

Together, these findings show that charter growth drives public sector incorporation—that is, higher fractions of school-aged students attend public schools—while increasing within-public sector segregation. The null effects of magnet school growth on segregation suggest that choice system design features, such as their inclusionary admissions policies, may play an important role in moderating the effects of increased choice on school segregation.

LITERATURE REVIEW

The Political Economy of Charter Enrollment Growth

A large body of research has focused on the individual-level characteristics of children who enroll in charter schools. Our focus here is different: to understand the contextual factors that precede charter growth. The literature on this topic is scarce, and most studies have examined cross-sectional variation in charter enrollment across educational markets and its association with the passage of charter laws, charter school entry, or charters' neighborhood attributes (Gulosino & Miron, 2017). These studies have emphasized four main factors, which we review below.

The first is the *demography of the school district*, both in terms of its population size and composition. Stoddard and Corcoran (2007), for example, concluded that student population size is one of the main predictors of charter school presence, with mid-size and large cities more likely to have charter schools. These authors, along with Glomm et al. (2005), find that charter school presence was greater in school districts with higher levels of racial and income heterogeneity, as well as more educated populations.

Variables associated with *local politics* comprise the second set of contextual features. Available studies consistently conclude that state-level law characteristics, such as school financing formulas, the number and composition of charter authorizers, and the presence of charter enrollment caps, are associated with charter growth (Stoddard & Corcoran, 2007; Vakilifathi, 2018). The presence of state-level advocacy groups and the strength of teacher unions shape the growth of charter schools through their effects on charter laws, with higher rates of unionization associated with more restrictive laws (Stoddard & Corcoran, 2007; Vakilifathi, 2018). Notably, partisan politics has been less consistently aligned with charter growth at the contextual level (Stoddard & Corcoran, 2007), with support coming from Democrats and Republicans alike (Bulkley, 2005; Bushaw & Lopez, 2013; Reckhow et al., 2015).

A third contextual factor is *school performance* (Stoddard & Corcoran, 2007; Bifulco & Buerger, 2015), as an explicit objective of charter laws is to improve student learning (Wohlstetter et al., 1995). Lower school quality may lead local policymakers or parents to pursue alternate schooling options. Evidence on these issues is scarce and mixed, in part due to variation in the levels of analysis (e.g., districtwide, statewide, or nationwide) used by authors. Stoddard and Corcoran (2007) find that states with higher-than-predicted baseline SAT scores were less likely to expand the charter sector. Studies exploring within-state variation in charter growth suggest substantial variation between states, with some studies finding that low district-level student achievement predicts greater charter support (Corcoran & Stoddard, 2011), while other studies of charter location decisions find weak evidence that charter schools locate in low-performing school districts (Glomm et al., 2005, studying Michigan). It remains unclear, however, to what different conclusions drawn across states reflect between-state heterogeneity in the relationship between achievement and charter growth, or differences in the measures used to model it.

Finally, charter school growth is associated with the *economic features of school markets*. For example, Bifulco and Buerger (2015) find that charter schools are more likely to open in locations with higher per-pupil expenditures, where they receive higher payments. Conversely, the literature on educational decentralization underscores the role that economic trends play in their political economy, with some evidence that decentralization reforms follow economic crises and accompanying budget deficits (Eaton et al., 2011).

The Effects of Charter Enrollment Growth on the Distribution of Students

Multiple studies identify demand- and supply-side factors that shape the relationship between charter school growth and school segregation. On the demand side, families are responsive to socioeconomic and racial composition, and choose schools with composition similar to their own households (Bifulco & Ladd, 2008; Billingham & Hunt, 2016; Booker, Zimmer & Buddin, 2005; Hailey, 2020; Schneider et al., 1998; Dee & Fu, 2004; Hsieh & Urquiola, 2006; Mizala & Torche, 2012; Weiher & Tedin, 2002). For example, Elacqua et al. (2006), studying Chile's expansive school choice program, concludes that classmates' socioeconomic status, rather than schools' academic performance, affect families' choices. Some evidence suggests that families explicitly seek information on composition: Schneider and Buckley (2002) examined online school searches in Washington, DC, and found that users were more likely to search for the racial/ethnic composition of the school than any other school characteristic. Nonetheless, these observational studies cannot determine whether unmeasured school characteristics that proxy race and socioeconomic status, such as pedagogical or substantive foci, play a role in generating these patterns.

These observational findings are consistent with experimental evidence demonstrating that white parents avoid schools with higher proportions of Black students (Billingham & Hunt, 2016; Weither & Tedin, 2002), as well as evidence that white, Asian, and Latinx families choosing high schools avoid majority Black schools (Hailey, 2020). Because these experimental studies hold school quality factors constant, their results suggest that preferences for same-background classmates may drive segregation.

Finally, we know much less about the effect of charter growth on private school enrollment. Despite long-term private school enrollment declines, especially among middle-income families

(Murnane & Reardon, 2018), whether increased public school choice contributed to this trend remains unknown. Two studies, both analyzing data from Michigan, come to conflicting conclusions. Toma et al. (2006) finds that charter growth is correlated with meaningful decreases in private enrollment. On the other hand, Chakrabarti and Roy (2011), exploiting differences in the regulatory framework of Michigan's charter authorizers, find weak evidence that charter schools "crowd-out" private schools.

In sum, many studies from the US and around the world have found positive associations between school choice and segregation (Ayscue et al., 2018; Monarrez et al., 2019; Fiel, 2015; Saporito, 2003; Mizala & Torche, 2012). We build on these contributions by using a causal design, which few prior studies do (though see Monarrez et al., 2019), by explicitly integrating the full range of options available to students, and by providing an overall assessment of the effect of charter school enrollment growth on racial and socioeconomic segregation over a 22-year period.

Data and Methods

We analyze data from two major sources—the Common Core of Data (CCD) and the Private School Survey (PSS)—from 1993-94 through the 2015-16 school years (biennially in the case of PSS). The CCD is compiled annually and contains schools' racial and socioeconomic and a limited set of other variables, such as the student/teacher ratio and urbanicity of the school. The only proxy for socioeconomic status (SES) available in the CCD data is the proportion of students receiving free or reduced price lunch. We discuss the missing data for the free and reduced price lunch variable in Online Appendix, Part A, and use linear interpolation to address these issues.

Because of the uneven availability of charter schools at the elementary and secondary school levels, we separate these schools in our analysis and classify them by the first grade offered.

If the first grade offered is fourth grade or lower, the school is classified as “elementary”; if the first grade offered is fifth grade or higher, the school is classified as “secondary.” As the CCD data do not flag charter schools prior to 1998, we categorize charter schools as schools that are flagged in 1998 and match the school ID for previous years. To correct any potential error in the categorization due to traditional public school-to-charter conversions, we use data from the National Alliance for Public Charter Schools (2007-2016) survey, which collects charters’ initial year of founding or conversion (see Online Appendix, Part A).

The question of appropriate levels of aggregation to address our research question is complex. On one hand, enrollment in charter, private, and magnet schools is not limited to particular school districts; on the other, enrollment in traditional public schools is. Recent literature has demonstrated increased socioeconomic segregation *between* school districts (Bischoff & Owens, 2019; Owens et al., 2016), such that using school districts as the unit of analysis would provide downwardly-biased estimates of the effects of choice on segregation. Choosing a level of aggregation above the district level leaves open the possibility of county or MSA-level analyses. However, the latter are problematic because they sometimes include multiple state-level jurisdictions, whereas charter laws operate at the state level. Finally, the decision to choose counties as a level of analysis is further complicated by the fact that counties serve as school districts in many Southern states.

Weighing these tradeoffs, we ultimately decided to aggregate school data to the county level, using sensitivity tests to examine the robustness of our strategy to the exclusion of counties that also serve as school districts. This allows us to examine SES and racial segregation net of between-district housing sorting that may be a response to school choice policies. In sum, our unit of analysis is the county-school level (elementary and secondary); for reasons previously noted, we calculate our outcomes and explanatory variables separately for elementary and secondary schools.

Our analysis sample includes counties that experienced a change in charter school enrollment, yielding a total sample of 596 elementary and 535 secondary school-counties. We use the PSS to calculate the private school enrollment of each county and year of the panel. Models predicting private school enrollment use biennial years between 1993-94 through 2015-16. As displayed in Figure 1, enrollment in charter and magnet schools has grown considerably over this period, while private school enrollment has declined.

Figure 1 around here

We supplement CCD data with a rich panel of covariates from five different sources, all of which are collapsed to the county level. Specifically, we include the per-pupil expenditures of school districts within counties using the NCES School District Finance Survey. We also generated a set of economic indicators available at the county level. We use the County Business Patterns (CBP) database, which provides precise yearly indicators of the scale of economic activities taking place at firms or establishments within counties. We use the annual payroll as a proxy for income (adjusted to 2015 dollars). This indicator is collected from the private nonfarm sector, but excludes agricultural production, the government sector, and self-employed individuals. We also include the share of the manufacturing sector in the local economy, and further supplement the panel with the Bureau of Labor Statistics' county unemployment records. As a proxy for the political context, we construct a measure using the Dave Leip's Atlas of Congressional elections (1992-2016, biennially) that collects the Republican/Democrat composition. Finally, as a proxy for school academic performance, we include the Stanford Education Data Archive's county-level mean

(Reardon et al., 2018) from 2008 to 2014, using 4th and 8th grade scores to correspond to elementary and secondary schools, respectively.

Measuring Segregation. We conceptualize segregation as the disproportionate concentration of members of one group in space relative to other groups in the population (Massey et al., 2009). Segregation can be operationalized in several ways. We use the information theory index (*H*), a well-known measure of "evenness" (Massey & Denton, 1998; Reardon & Firebaugh, 2002). The information theory index takes a value of 0 when there is no segregation and a value of 1 when there is complete segregation. Unlike other popular measures of evenness, including the traditional dissimilarity index, the information theory index satisfies the "principle of transfers," which means that a transfer of a student from a school in which her group is underrepresented to one in which her group is overrepresented will always register as an increase in measured segregation (James & Taeuber, 1985; Reardon & Firebaugh, 2002). In Online Appendix, Part B, we describe the calculation of the *H* index.

Changes in Charter Enrollment. Our variable of interest is the county-level change in charter enrollment in the county measured as a percentage of total public school enrollment. We consider this measure to be more precise than a dichotomous measure of charter school entry because charter schools, once opened, grow considerably over the following years; for example, charter schools, on average, double in size by year five. Beyond this issue, charter schools differ substantially in size.

The school-county panel allows us to precisely identify the timing of entry or exit of charter school students over time. We observe 98% of the charter schools operating through the 2015-

2016 academic year, with the remaining 2% constituted by charter schools that opened before the first year of our panel.

To demonstrate the viability of our strategy, Figure 2 shows the percent of counties gaining or losing charter enrollment during this period, together with the mean gain or loss in percentage points. Charter enrollment growth is more common and larger in magnitude than enrollment loss. We observe a total of 9,103 county-years with net charter enrollment gains and 3,605 county-years with charter enrollment decline. Among counties gaining charter enrollment, the average increase is around one percentage point (0.93%), which represents an increase of 253 students.

Figure 2 around here

Analytic Strategy

In this section, we describe our empirical strategy. Our reduced-form results are based on the following regression specification:

(1) $$S_{itr} = p_{ir} + \gamma_{ts} + \beta c_{itr} + \delta x_{itr} + \lambda Z_{it} + \varepsilon_{itr}$$

In equation 1, i indexes counties, r indexes the school level (elementary or secondary), s indexes states, and t indexes school years. Our outcomes of interest ($S_{it\tau}$) include both school segregation and private school enrollment for county i for school level r in year t. β is the parameter of interest, which is the effect of the percentage of charter enrollment c_{itr} on $S_{it\tau}$. We later replace this term with the magnet school enrollment percentage to compare effects of different types of choice. p_{ir} indicates a county-school level fixed effect and γ_{ts} is a state-year fixed effect. x_{itr} is a vector containing the following covariates: percentage of students that are Black, Asian, Hispanic and white; teacher-student ratio; percent of schools in the county classified as rural; and percent free and reduced-price lunch eligible students. We cluster all standard errors at the county-school level.

Motivated by previous studies on factors that drive the political economy of charter schools, Z_{it} includes a set of variables that we refer to as *charter determinants*, including (1) the size of the school market (log total public enrollment), (2) economic factors (median earnings, the share of manufacturing sector, unemployment rate and the average per pupil expenditure), (3) county-level academic performance, equated across states in the SEDA data, and (4) electoral politics (Republican-Democrat margin in congressional elections, included as percentage). The parameter λ denotes the extent to which charter support is related to our outcomes S_{itr} conditional on p_{tr}, γ_{is}, c_{itr}, and x_{itr}.

Our main identifying assumption is that charter support is unrelated to changes in our outcomes of interest when conditioning on school and county demographics and state-year fixed effects. The bias would be severe if segregation or private enrollment is a function of charter determinants ($\lambda \neq 0$). At a theoretical level, there are grounds to presume this is true. Unlike magnet schools, which were first created in the 1970s to address issues of racial and socioeconomic balance, none of the charter laws has an explicit policy objective of reducing or alleviating racial or socioeconomic segregation. We test the plausibility of this assumption by exploiting the precise timing of events as a difference-in-difference diagnostic. We explore pre-event trends by plotting changes in the outcomes in years before and after changes in charter enrollment occur to ensure that we are estimating the effects of "sharp on-impact changes" (Gentzkow et al., 2011, p. 2991), and we expect that the effect of the events is contemporaneous.

We also allow for significant pre-trends of charter enrollment growth on confounding variables Z_{it}, as specification in equation 2 reflects. We use the following specification as a diagnostic, where Δ denotes a first-difference operator:

$$(2) \qquad \Delta Z_{it} = \sum_{k=-8}^{8} a^k \, \Delta c_{ir(t-k)} + \Delta\gamma_{ts} + \Delta\delta x_{itr} + \Delta\varepsilon_{itr}$$

We plot coefficients a^k, so that (k<0) displays the relationship between current changes in charter enrollment and lagged values in the outcome. Values for which (k>0) represent current changes of charter enrollment on future changes in the outcome, while (k=0) represents the contemporaneous effect; that is, current changes in charter enrollment and current changes in the outcome.

In the next section, we use this basic diagnostic to examine the forces that influence charter school enrollment growth. We proxy ΔZ_{it} with each variable defining charter determinants, as indicated previously.

RESULTS

Determinants of Charter Enrollment Growth

In this section, we examine the determinants of charter enrollment growth with the goal of understanding its political economy and identifying the potential sources of bias in our estimates, to the extent that they may be related to our outcomes of interest. For the sake of exposition, we refer to the point of sharp change as "the event."

As discussed in the literature review, we expect that growing school-aged student enrollment is one of the main determinants of charter growth. Figure 3 plots coefficients predicting total enrollment (including students in private schools, measured biennially) in Panel A and public enrollment in Panel B, following the specification described in equation (2). Since the coefficients represent changes rather than levels, a single positive spike corresponds to a permanent positive effect on the outcome. Panel A shows a decreasing student population change between years 4 to 6 before the event. As can be observed in Panel B, this trend is reversed, with increased public enrollment in the period immediately before the event (significant at $p<0.1$). Due to the nature of the private school data, we cannot observe patterns from the year before the event in Panel A.

Additionally, Panels A and B consistently show that student enrollment continues to grow during the year of the event and the period immediately after. This evidence suggests that charter enrollment growth occurs in the middle of a positive cycle of student population growth, in line with previous evidence (Stoddard & Corcoran, 2007).

This evidence raises the question of whether current charter school growth induces future increases in student population. If student population is serially correlated and charter entry is affected by past values of student population, charter entry would predict future enrollment growth. We follow Gentzkow et al.'s (2011) strategy to test for serial correlation. In Online Appendix Figure A2, we confirm this conjecture by orthogonally estimating residuals of future values from past values of the outcome (sometimes referred to as a "whitened" estimate). As can be observed in Online Appendix Table A2, the association disappears after this adjustment, suggesting that contemporaneous charter growth is unrelated to future changes in student enrollment.

Figure 3 around here

We now turn to the relationship between charter growth and economic indicators that vary annually, including unemployment, per-pupil expenditures, median earnings, and manufacturing sector share. Figure 4 displays consistent significant pre-trends in multiple economic indicators, suggesting that charter enrollment growth is preceded by a local economic downturn. Specifically, we observe an increase in the unemployment rate and a decrease in median earnings, both seen primarily at the elementary level two years before charter growth and a significant decrease in the size of the manufacturing employment sector, which has consequences for middle-class wages (Wib, 2015).

We do not have a strong explanation for the relationship between economic downturns and charter growth. The institutional literature on decentralization, which argues that economic decline may prompt reorganization and challenge existing political arrangements, offers one potential account (Eaton et al., 2011).

Figure 4 around here

While we do not present the full results here, we do not find evidence that charter enrollment growth is affected by changes in mean or median test scores, racial composition, or county Republican-Democrat margins in Congressional elections. (See Online Appendix.)

Effects on School Segregation

We next estimate models of the form described in equation (2), measuring school segregation with the information theory index (H index). Figure 5 presents our core results, which show that charter enrollment growth increases white-Black and socioeconomic segregation. We do not find robust evidence that charter growth affects white-Hispanic segregation. (See robustness checks in Online Appendix, Part C).

We observe the effect of charter enrollment growth to be contemporaneous, supporting an arguably causal interpretation of these parameters. With the exception of a small negative spike predicting white-Black segregation five years before the event, there is no significant trend immediately before or after charter changes transpire. This finding suggests that current changes in charter school growth are not being driven by confounding trends. We test this assumption with an F test of equality for all pre-event coefficients, which yields non-significant results.

As plotted coefficients are expressed as first differences, the contemporaneous effect corresponds to a permanent positive effect on the level of segregation. The point estimate in Figure 5 suggests that a one percentage point increase in the charter enrollment share increases white-Black and socioeconomic segregation by around .02-.03 SD as measured by the H index.

Figure 5 around here

To test for the presence of differential charter growth effects at initial and later stages, Figure 6 shows separate estimates for counties that vary in charter market shares. Panel A and B suggest that the effect of an increase by one percent in charter growth at early stages (less than 10%) can lead to an increase in racial segregation of .05 SDs and socioeconomic segregation by .05-.1 SD. In our view, these effects are meaningful in size. Put differently, we can benchmark this effect against other policy interventions. For example, Reardon et al.'s (2012) study estimated that the dismissal of a court-ordered desegregation plan led to a white-Black segregation increase of .47 SD in the 10 subsequent years. Translated to our results, one additional charter enrollment percentage point, on average, is roughly equivalent to 10% of the effect of closing a desegregation order.

Early charter enrollment growth has a higher marginal effect on racial segregation than subsequent growth. The effect on racial segregation of early charter enrollment growth (counties with greater than 3% and less than 10% charter market share) is more than double the average estimated effect for all years. Moreover, after a market reaches 10% of charter enrollment, the effect on racial segregation fades in magnitude and statistical significance. On the other hand, the magnitude of our effect on socioeconomic segregation suggests that the marginal effect of charter enrollment on socioeconomic segregation persists steadily as charter market share increases. The long-lasting effect of charter growth on socioeconomic segregation may be related to the lower baseline level of socioeconomic segregation. Since schools' and neighborhoods' baseline levels of segregation already are much higher for race than SES, this may create a "ceiling effect" for additional segregation through choice.

Figure 6 around here

Magnet School Replication. Because an open debate in the literature is whether increased choice itself consistently increases segregation, or can be moderated by system design, we replicated the models above substituting magnet school enrollment growth for charter growth. Like charters, magnet schools offer families an alternative to their residentially-zoned traditional public school, though they have a very different history. Created in the 1970s as a federal initiative encouraging a voluntary approach to desegregation, magnet schools explicitly were “designed to bring students from different social, economic, ethnic and racial backgrounds together” (Betts & Loveless, 2005) while increasing educational quality (Steel & Levine, 1994). Siegel-Hawley and Frankenberg’s (2012) survey results indicate that approximately two-thirds of magnets employ practices to promote diversity, such as weighted admission lotteries to maintain compositional balance and increased outreach to diverse communities. As maintaining student diversity has been an explicit goal of magnet schools, it is unclear if this type of choice would affect segregation by race or SES in the same manner as charter schools (Rossell, 1991, 2002; Davis, 2014).

We find no evidence that changes in magnet school enrollments affect county-level racial or SES school segregation (see Figure A5, which parallels results for charters displayed in Figure 5). Nonetheless, a concern about comparing these school types is that magnet and charter school operators may locate in very different places. We address these issues in Table 1, bringing charter and magnet schools into the same models. Panel A reports results for white-Black segregation, while Panel B reports poor-non-poor segregation. In each panel, Model 1 restricts the sample to counties that experienced elementary and secondary charter school growth. Model 2 replicates Model 1, but restricts the sample to counties with both magnet and charter school growth. Finally, as the market penetration of magnet schools is higher at secondary levels, we show these models separately for elementary and secondary school growth in Models 3 and 4, respectively. As the F-test shows, we can reject at a 10 percent level the equality across magnet and charter coefficients for all models. One important limitation of these findings is that one-third of magnet growth occurred before the first year of our panel, in contrast to only 2 percent of charter growth. This means that earlier effects of these magnets on the distribution of students across schools are not fully captured here.

Another potential concern is that the determinants of magnet growth differ from charters. In analyses that parallel those we presented for charters, we found that magnet growth is subject to similar economic forces as charter schools. Local economic downturn—specifically, increased unemployment—precedes magnet enrollment growth. A difference worth noting, however, is that total student enrollment does not increase in the years preceding magnet growth. (Results available upon request.)

Table 1 around here

Our findings are robust to a variety of alternative specifications that we discuss in Online Appendix, Part C. First, as observed in Table A1, other specifications such as random effects, within-county correlation AR(1), and models clustering the standard errors by state-decade yield estimates comparable to those in our baseline results. Second, we test our results using the time-corrected Wald ratio (WTC) as an alternative identification strategy, which relaxes the assumption that treatment effects are stable and homogenous over time (Chaisemartin & D'Hautefeuille, 2019). These models corroborate our findings in terms of direction and statistical significance, though coefficients are smaller in size. Furthermore, using this strategy, we perform a placebo test to confirm that our estimators are not driven by unobserved trends or serial correlation, and find no evidence that this is the case. (See discussion and results in Online Appendix, Part C).

Results for Private School Enrollment

We now turn to estimating the effect of charter school growth on private school enrollment. For this purpose, we use a biennial subsample from 1993 to 2015 to match county-school-level information of private schools, identifying elementary and secondary private schools. In Figure 7, we display evidence that charter enrollment growth negatively and significantly affects private school enrollment. One additional percentage point of charter enrollment decreases private enrollment by 0.13 percentage points. Put differently, for every ten students enrolled in a charter school, at least one student would have otherwise attended a private school. (See alternate specifications, to which these results are robust, in Online Appendix, Figure A6.) We find no effect of magnet growth on private school enrollment. We note that due to the biennial nature of the PSS data, we are unable to model private school enrollment in the year preceding charter enrollment change. As we see meaningful shifts in the county's public enrollment and composition in the year preceding change, this is a limitation of our findings, and likely makes our estimate a lower bound.

Figure 7 around here

To the extent that charter schools moderately increase private school exit, we next analyze how this inflow of students affects public school composition. In Table 2, we show the estimated effect of current charter enrollment growth on current school demographics. Columns 1 and 2 predict the white student population (log enrollment and percentage), and Columns 3 and 4 do the same for non-poor students (free and reduced-price lunch). Presumably linked to private school enrollment shifts discussed above, we observe an important contemporaneous inflow of non-poor families to the public school system, such that the percentage of non-poor students increases by 0.2% for every additional charter enrollment percentage point. However, the effect of charter growth on the percentage of whites, though positive, is not statistically significant.

Table 2 around here

Conclusion

Despite major changes in the provision of public education, our understanding of choice policies' systemwide implications for families' sorting between and within sectors remains incomplete. In this paper, we demonstrate that charter growth is associated with declines in private school enrollment, and provide robust evidence that charter school growth leads to racial and especially socioeconomic school segregation, a finding that is partially explained by non-poor students' transition from the private to public sector. In our view, the effects reported here are non-trivial in size: we estimate that one additional percentage point of charter enrollment produce an effect on Black-white segregation equivalent to 10% of the effect of ending a desegregation order (Reardon et al., 2012).

That charter growth drives public sector incorporation, while also increasing segregation within the public system, raises many normative questions for policymakers. As Brighouse, Ladd, Loeb, & Swift (2018) skillfully clarify, the effects of any policy decision must be assessed against the values that one endorses. For those who believe that choice in itself is a core value, that charter enrollment growth increases segregation may be a secondary concern. Shifts from the private to charter sector, from this perspective, would be evaluated as a triumph for parents, who now have additional options. In contrast, for those concerned with the role of schools in integrating children from different racial, ethnic, and economic backgrounds, both as an end in itself and as a way of ensuring all students have access to the same resources, our results raise concerns about the role of increased choice in impeding or stalling that goal. Beyond these considerations, for all parties, the effects of charter schools on a range of student outcomes introduce additional tradeoffs to consider; some may be willing to tolerate higher or lower levels of segregation depending on the outcomes that schools produce.

Irrespective of the first principles to which one subscribes, that charter growth increased racial and socioeconomic segregation, while magnet school growth did not, suggests that magnet admissions practices are worthy of further study. Intentionally inclusionary admission policies may be a regulatory solution to alleviate or avoid the segregating consequences of charter enrollment growth. In addition to magnets, the small number of "diverse by design" charters may offer some early lessons on the potential of this strategy (Potter & Quick, 2018).

We close by noting the limitations of our study. First, area-level data, such as counties, undermine the precision of our estimates, as school enrollment is not consistently restricted by county boundaries (Chakrabarti & Roy, 2011). Second, data limitations meant that we could not fully test drivers of charter growth discussed in the literature, such us academic performance of traditional public schools, as we had access to data for a subset of years. Finally, though we find our estimates on private enrollment to be persuasive, the biennial nature of the PSS only allows us to test for bias in a subset of pre-trend years; therefore, we cannot fully rule out the possibility there are omitted components driving the effect.

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Tables and Figures

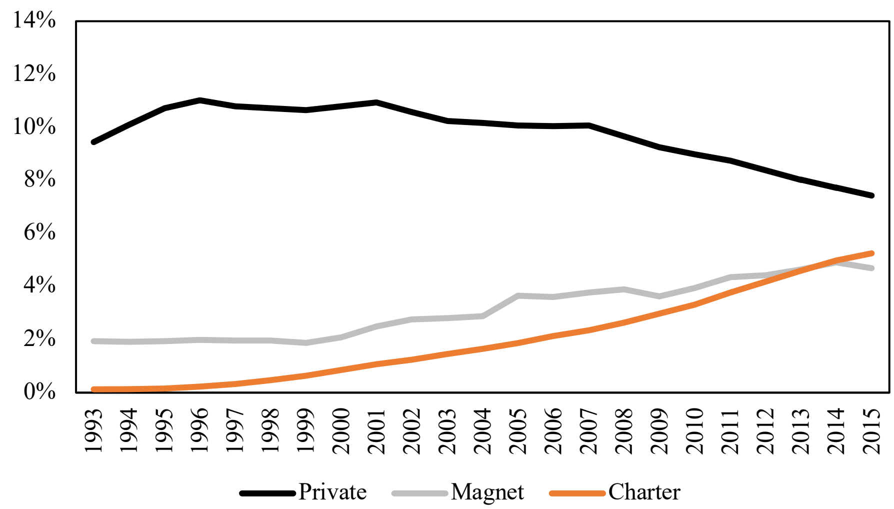
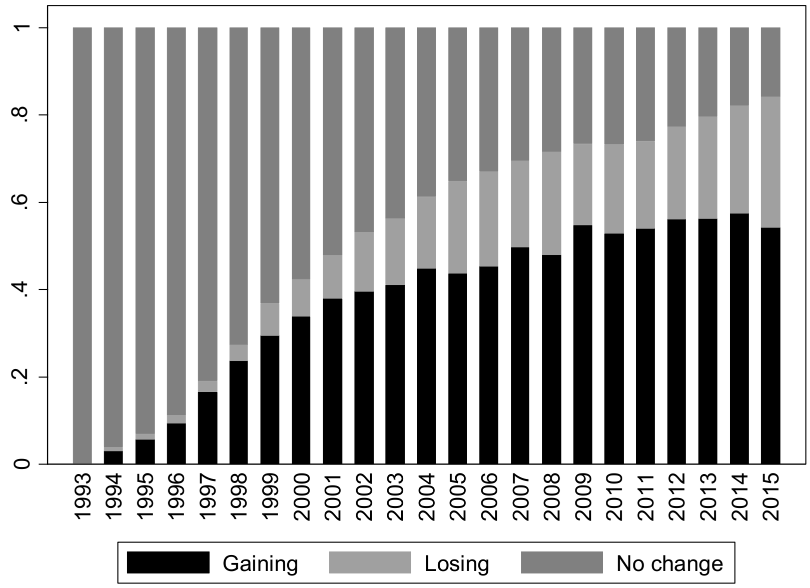
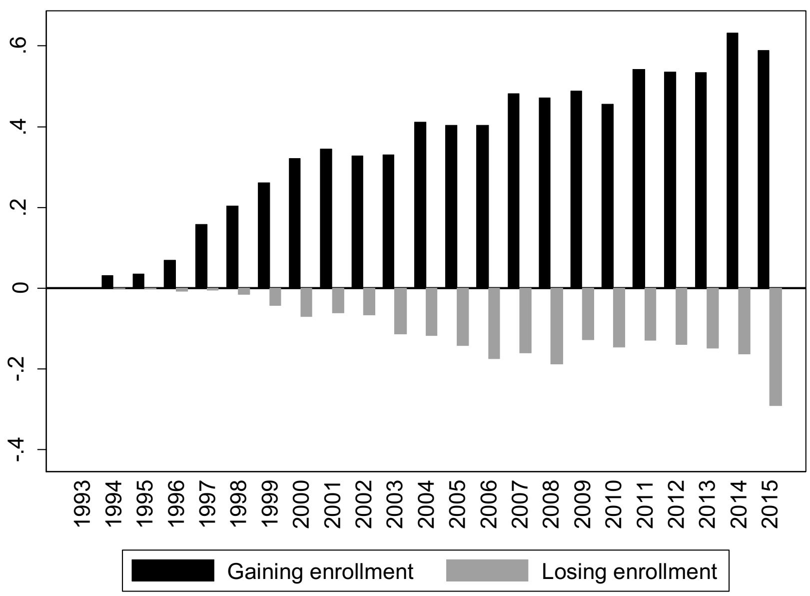


Figure 1: Percentage of K-12 Students Enrolled in Private, Magnet and Charter Schools, 1993-2016

Note: Authors' analyses of enrollment by school sector for K-12 and ungraded students using the Common Core of Data and Private School Survey. Private school enrollment is collected biennially and interpolated in this figure.



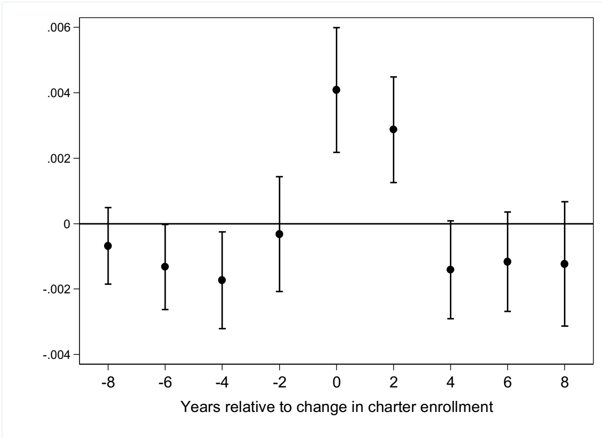
Panel A: Proportion of Counties Experiencing Charter School Enrollment Change



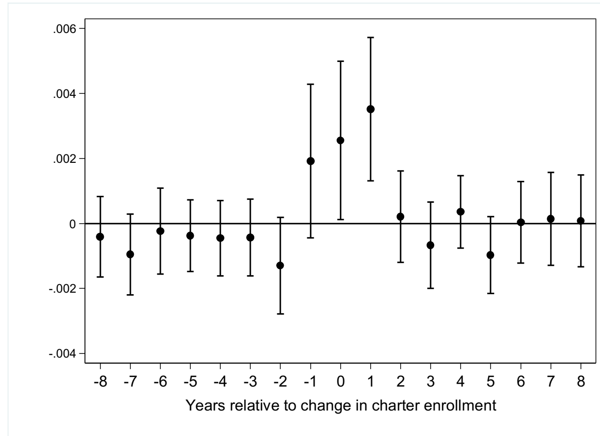
Panel B: Mean Percentage Point Change in County-Level Charter School Enrollment

Figure 2: County-Level Charter School Enrollment Change, 1993-2016

Note: The sample size is 23,896 county-level-year observations; N=1,086 counties x level of schooling.



Panel A: Public + Private Enrollment (log)



Panel B: Public Enrollment (log)

Figure 3: Determinants of Charter Enrollment Change: Enrollment (log)

Note**:** Panel A plots coefficients from a regression of change in total public and private school enrollment on a vector of leads and lags, relative to the timing of charter enrollment change (see equation 2); Panel B does so solely for public enrollment. The confidence intervals are constructed using standard errors clustered at the county x school level. Analysis is restricted to counties that experienced charter policies. All models include state-year fixed-effects and account for contemporaneous effects of non-educational covariates: rurality (%), county's median income, and unemployment. Sample size: Panel A: 11,924 county-level-year observations (N=1,086 counties x level); Panel B: 23,896. Period: 1993-2016.

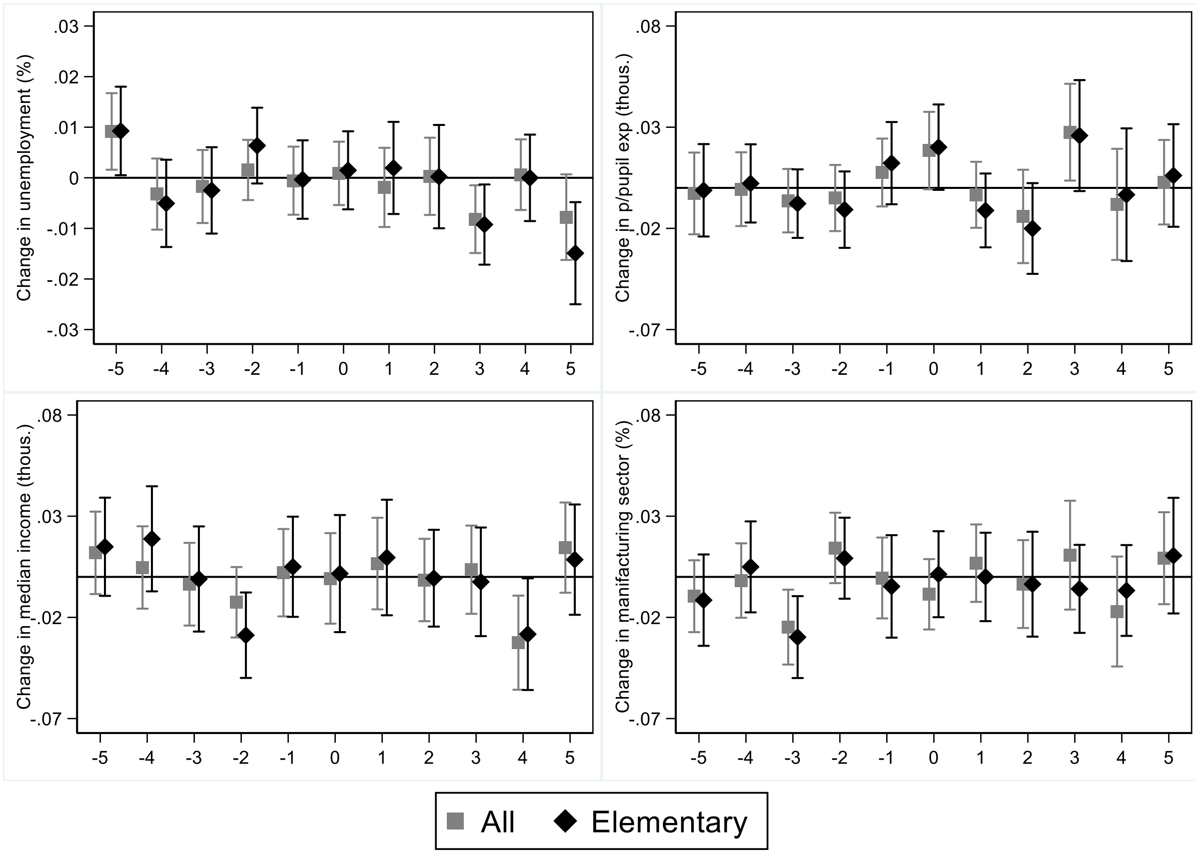
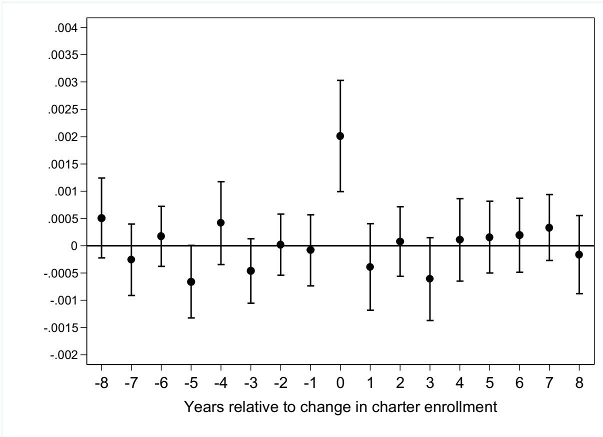
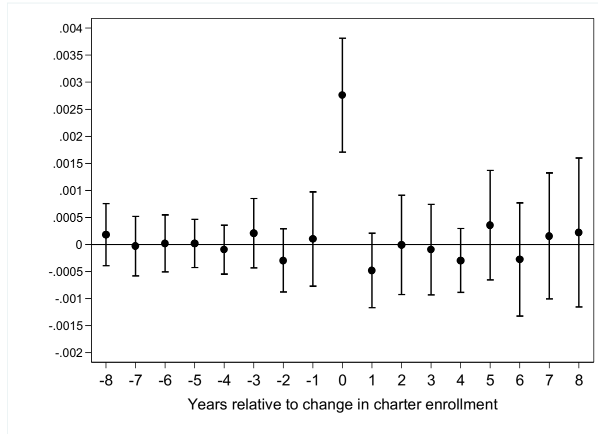


Figure 4. Determinants of Charter Enrollment Change: Economic indicators

Note: The graph shows coefficients from a regression predicting change (variables are included as first differencess) in different economic and financial indicators on a vector of leads and lags, relative to the timing of charter enrollment change. The confidence intervals are constructed using standard errors clustered at the county x level. Analysis is restricted to counties that experienced charter policies. Models include state-year fixed effects and account for contemporaneous effects on total public enrollment (log), and level of rurality (%). The sample size varies slightly across different models due to missing data on the outcomes: 1,086 counties x level (full models) and 563 (elementary level counties). Period: 1993-2016



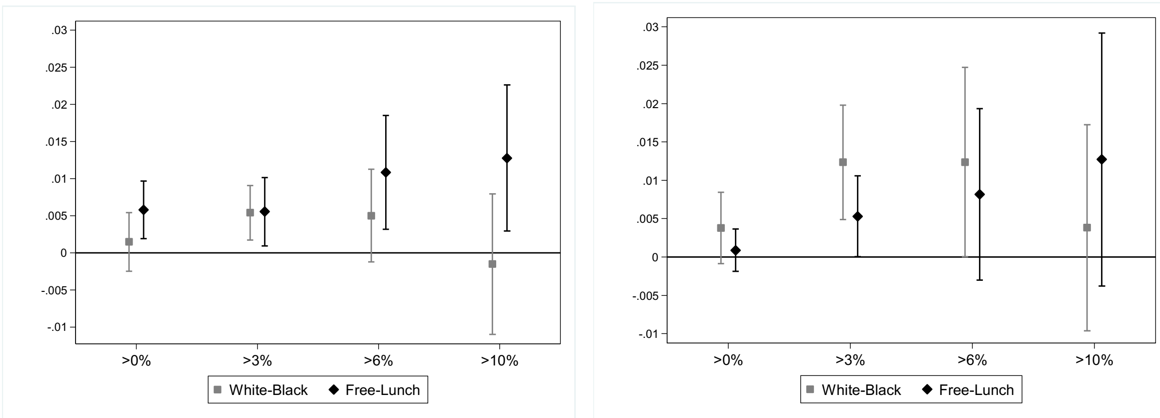
Panel A: Black-white segregation



Panel B: Poor-Non-Poor Segregation

Figure 5: Estimated Effect of Charter Enrollment Change on Black-white and Poor-Non-Poor Segregation

Note: The graph shows coefficients from a regression predicting change (variables are included as first differences) in segregation, as measured by the information theory index (*H*), on a vector of leads and lags, relative to the timing of charter enrollment change. The confidence intervals are constructed using standard errors clustered at the county x school level unit. Analysis is restricted to counties that experienced charter policies. Models include year x state fixed effects and account for contemporaneous effects on the following covariates: white, Asian, Hispanic, and Black (%), total enrollment (log), free-lunch enrollment (%), level of rurality (%), per pupil expenditures, teacher/student ratio, median income, unemployment (%), manufacturing size (%), and the Republican-Democrat electoral margin in the county election (%). The sample size is 23,892 county-school level-year observations (N=1,086 counties x level). Period: 1993-2016



Panel A: Elementary Schools **Panel B: Secondary Schools**

Figure 6: Segregation Effects, Earlier and Subsequent Charter Enrollment Change

Note: Models predict segregation (the information theory index (*H*)), given different levels of charter enrollment growth. Analysis is restricted to counties that experienced charter policies. Confidence intervals are constructed using standard errors clustered at the county level. Models include state-year fixed-effects. The sample size is 23,848 county-school level year observations (N= 561 elementary and N=523 secondary level counties). Period: 1993-2016.

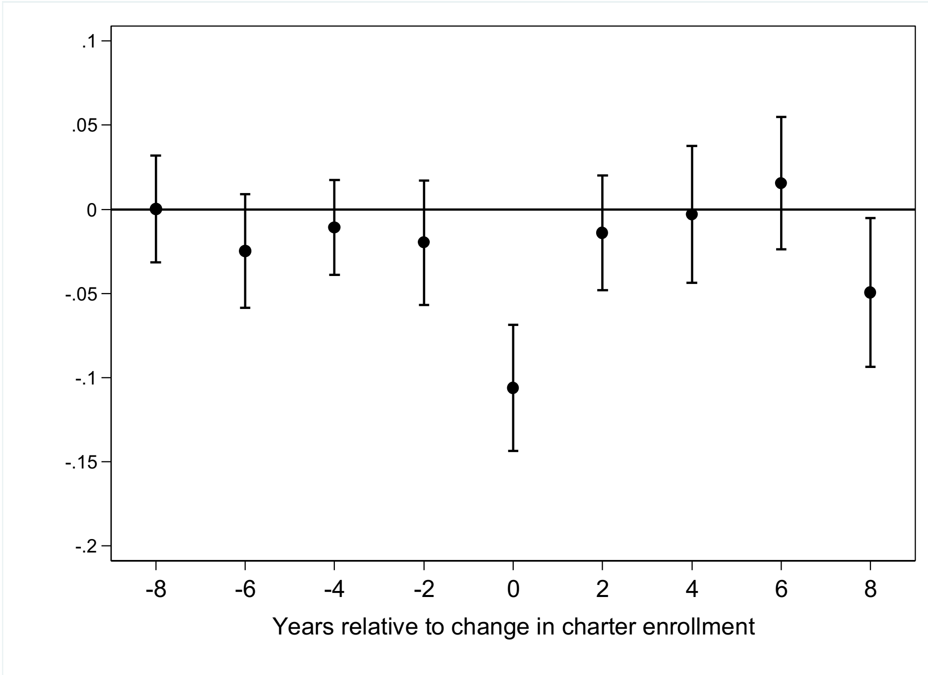


Figure 7: Estimated Effect of Charter Enrollment Change on Private School Enrollment (%)

Note: The graph shows coefficients from a regression predicting change, expressed as first differences, in private enrollment (%) on a vector of leads and lags, relative to the timing of charter enrollment change. We use two version of the outcome, as percent and as log (available in the Appendix Figure A6). The confidence intervals are constructed using standard errors clustered at the county x school level unit. Analysis is restricted to counties that experienced charter policies and private enrollment during the analyzed time-span. Models include year x state fixed effects and account for contemporaneous effects on white, Asian, Hispanic, and Black (%), total enrollment (log), free-lunch enrollment (%), per pupil expenditures, level of rurality (%), teacher/student ratio, median income and unemployment (%). The sample size is 7,370 county-school level year observations (N=670 counties x level). Period: 1993-2016 (biennial).

Table 1: Estimated Effect of Magnet and Charter Enrollment Change on Segregation

	Panel A: white-Black				Panel B: Poor-non-poor			
	General	General-restricted	Elementary	Secondary	General	General-restricted	Elementary	Secondary
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Charter enrollment	0.002**	0.0026**	0.0014**	0.0032**	0.0027**	0.0026**	0.0027**	0.002*
	(0.0005)	(0.0008)	(0.0006)	(0.001)	(0.0007)	(0.0005)	(0.0005)	(0.001)
Magnet enrollment	0.0001	0.0001	0.000	0.0003	0.0001	0.000	0.000	0.0001
	(0.0002)	(0.0002)	(0.0004)	(0.0002)	(0.0003)	(0.0003)	(0.0004)	(0.0004)
Demographics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F Test - Equality	12.7011	9.516	4.5009	8.1076	14.4997	22.6074	20.456	2.8092
P value	0.0004	0.0022	0.0343	0.0046	0.0002	0.000	0.000	0.0943
R-square	0.0522	0.1343	0.0643	0.1033	0.3036	0.197	0.2001	0.2591
Number of county x level	1086	367	563	523	367	1086	563	523
County x year x level	23892	8074	12386	11506	8074	23892	12386	11506

Note: ** denotes statistical significance at the 1% level, * at the 5% level, and † at the 10% level. The table show coefficients from a regression in first difference predicting current changes in school demographics. Standard errors clustered at the county x school level. Analysis is restricted to counties that experienced charter policies. Models 2 and 6 are restricted only for those counties that have magnet and charter schools. All models include year-state fixed-effects and account for contemporaneous effects on the following covariates: white, Asian, Hispanic, and Black (%), total enrollment (log), free-lunch enrollment (%), level of rurality (%), teacher/student ratio, median income and unemployment (%). Standard errors clustered at the county x school level. The F tests assess the equality of magnet and charter coefficients for each model. Period: 1993-2016.

Table 2: Estimated Effect of Charter Enrollment Change on White and Non-Poor Enrollment

	Log white Enrollment	White Enrollment (%)	Log Non-poor Enrollment	Non-Poor Enrollment (%)
	(1)	(2)	(3)	(4)
Charter enrollment	0.0016	0.0165	0.0049*	0.15**
	(0.0012)	(0.0217)	(0.0024)	(0.04)
Demographics	Yes	Yes	Yes	Yes
R-square	0.7817	0.1624	0.3613	0.2659
Number of county x level	1086	1086	1086	1086
County x year x level	23892	23892	23892	23892

Note: ** denotes statistical significance at the 1% level, * at the 5% level, and † at the 10% level. The table show coefficients from a regression in first difference predicting current changes in school demographics. Standard errors clustered at the county x school level. Analysis is restricted to counties that experienced charter policies. All models include year-state fixed-effects and account for contemporaneous effects on the following covariates: total enrollment (log), level of rurality (%), teacher/student ratio, median income and unemployment (%). Standard errors clustered at the county x school level. Period: 1993-2016

Appendix Figures and Tables

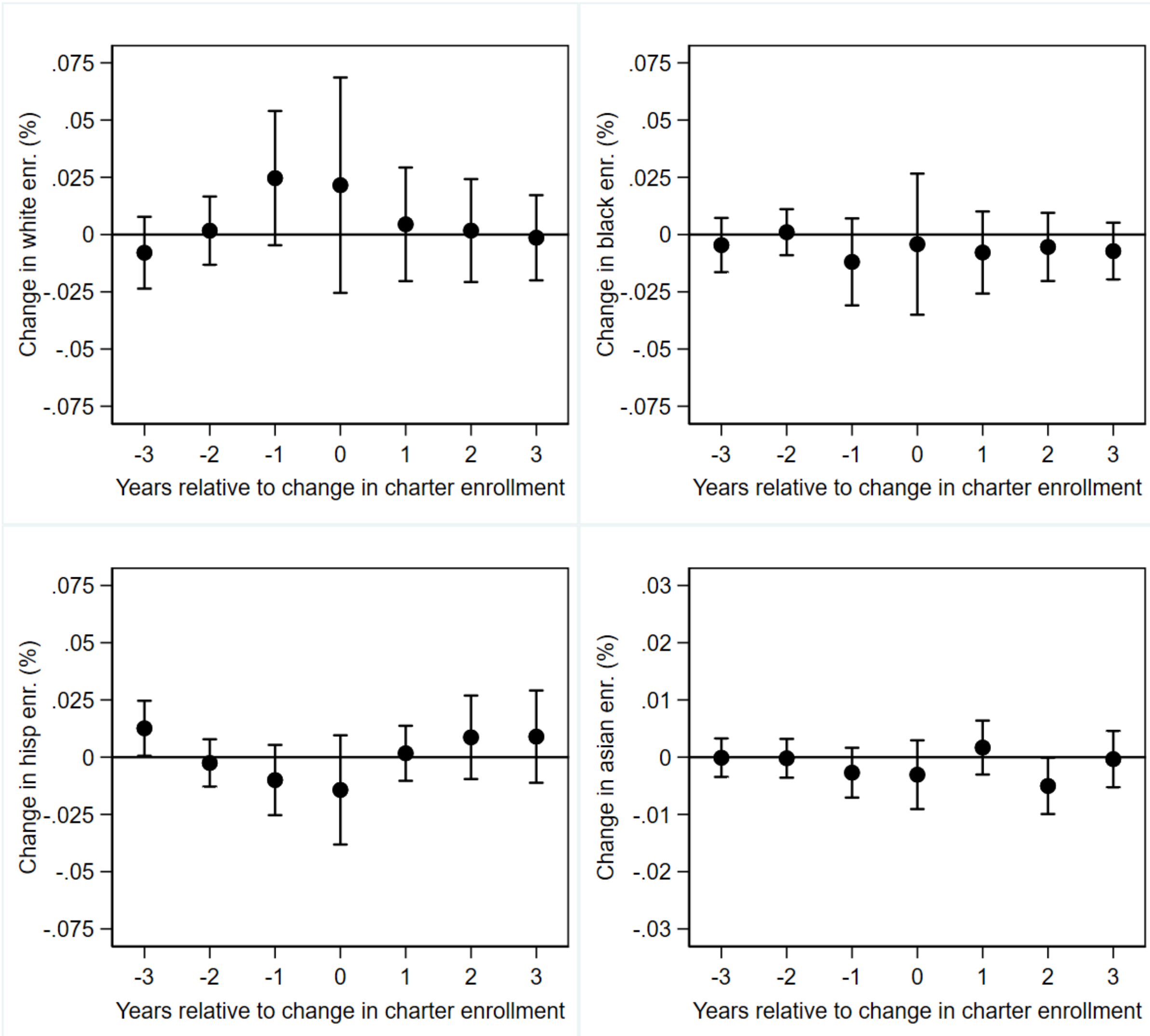


Figure A1: Determinants of Charter Enrollment Change: Racial composition

Note: Figures shows coefficients from a regression of change in the percentage of white, Black, Hispanic and Asian on a vector of leads and lags, relative to the timing of charter enrollment change (see equation 2). The confidence intervals are constructed using standard errors clustered at the county x school level. Analysis is restricted to counties that experienced charter policies. All models include state-year fixed-effects and account for contemporaneous effects on total enrollment (log), level of rurality (%), median income and unemployment (%). The sample size is 23,892 county-school level year observations (N=1,086 counties x level). Period: 1993-2016.

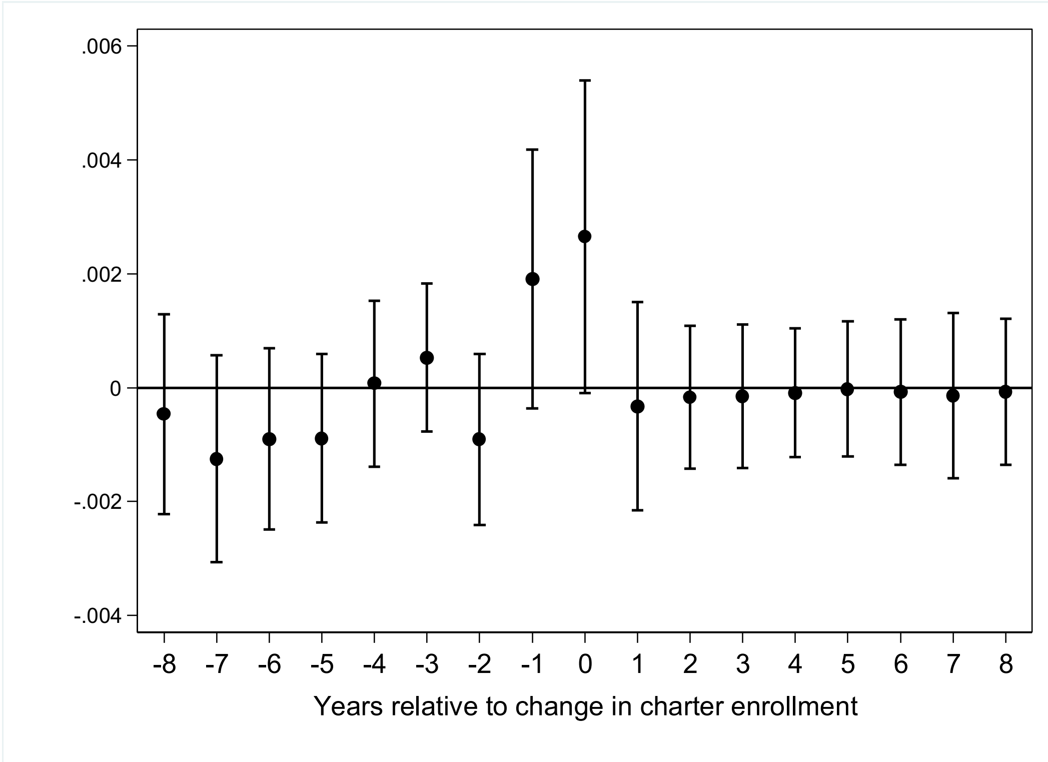


Figure A2: Total Public Enrollment (log), whitened series

Note: Figure shows coefficients from a regression of change in total public enrollment on a vector of leads and lags, relative to the timing of charter enrollment change (see equation 2). Coefficient are plotted from a regression predicting the innovation in log total enrollment on a vector of leads and lags. Innovation in a variable is its residuals from a regression of total enrollment on ten lags of the variable and charter change. Analysis is restricted to counties that experienced charter policies. The confidence intervals are constructed using standard errors clustered at the county x school level. The sample size is 23,892 county-school level year observations (N=1,086 counties x level). Period: 1993-2016.

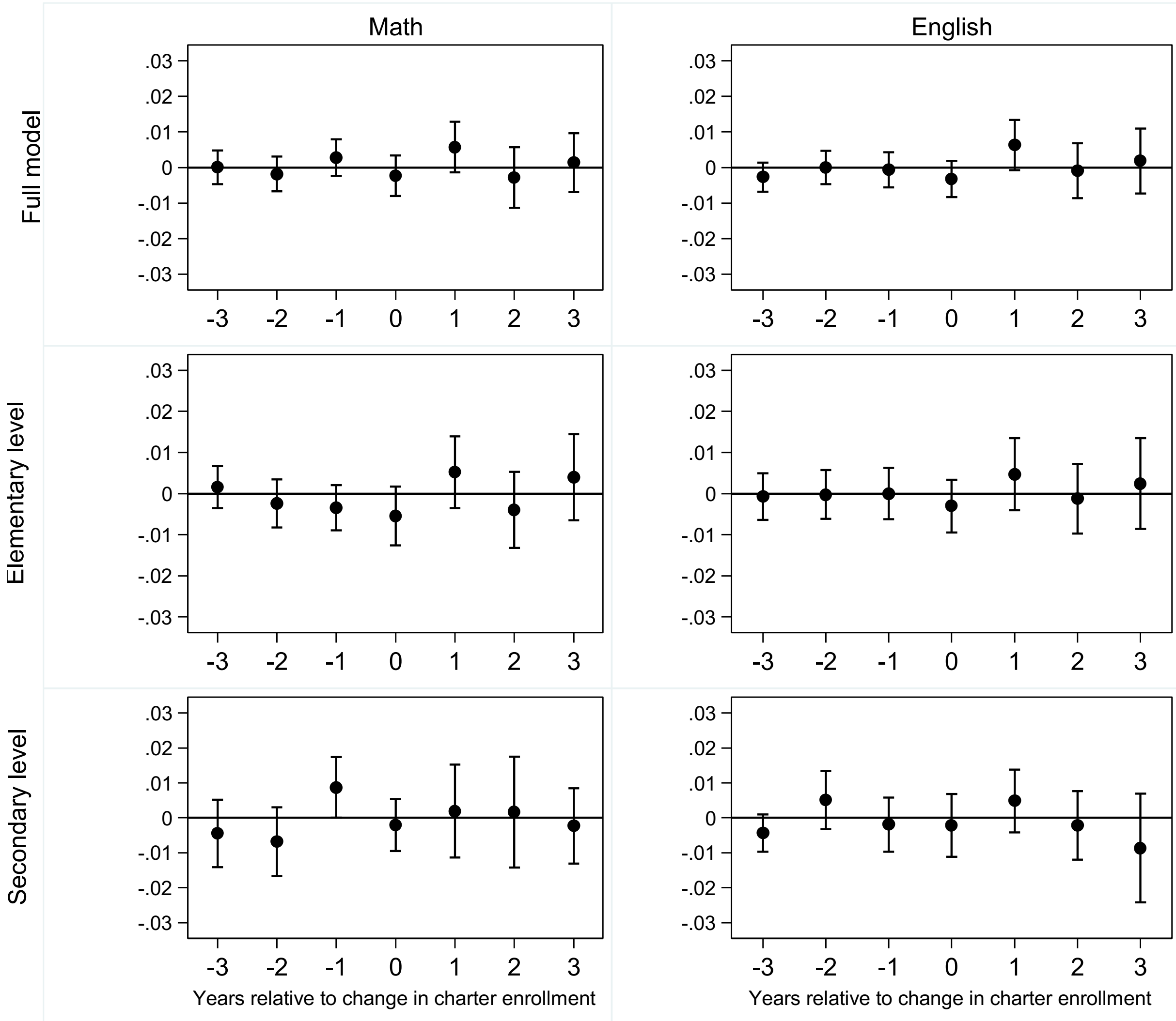
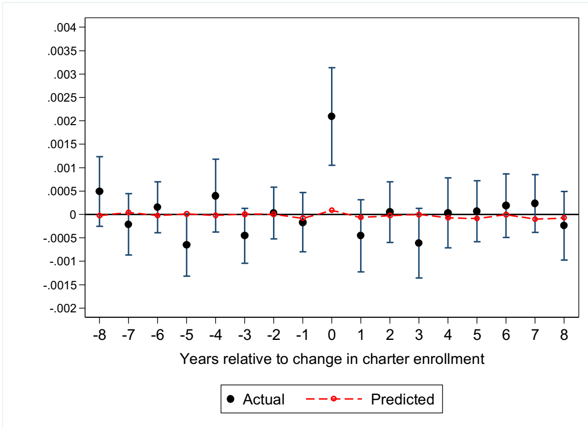
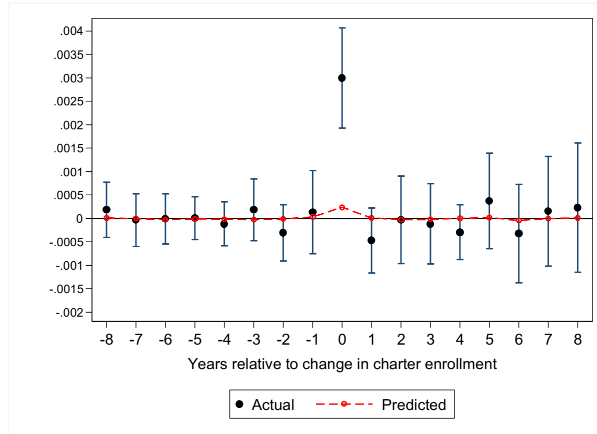


Figure A3: Determinants of Charter Enrollment Change: Academic Achievement

Note: Figures shows coefficients from a regression of change in academic achievement (NAEP mean scores) on a vector of leads and lags, relative to the timing of charter enrollment change (see equation 2). The confidence intervals are constructed using standard errors clustered at the county x level and models predict achievement mean in math (left column) and ELA (right column). Analysis is restricted to counties that experienced charter policies. All models include year fixed-effects and account for contemporaneous effects on white, Asian, Hispanic, and Black (%), log enrollment, per pupil expenditures, rurality (%), free lunch enrollment (%), median income and unemployment (%). Sample size: 393 (elementary) and 353 (secondary) models predicting math. And 451 (elementary) and 410 (secondary) counties for models predicting ELA. Period: 2008-2014



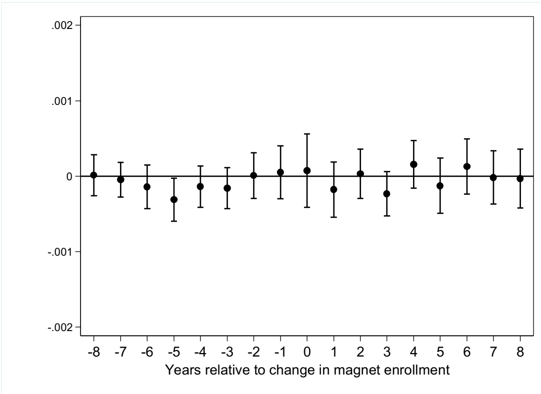
Panel A: Black-white segregation

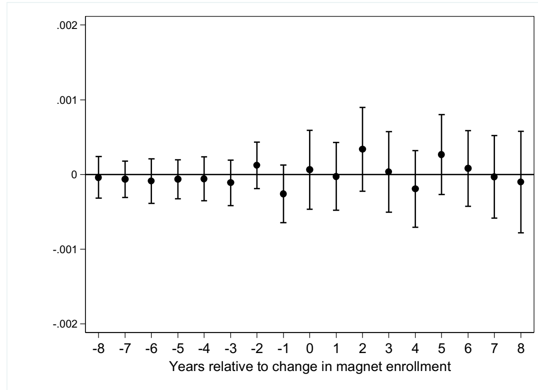


Panel B: Poor-Non-Poor Segregation

Figure A4: Estimated Effect of Charter Enrollment Change on Segregation (no controls)

Note: Graph show coefficients from a regression predicting change (variables are included as first differences) in the information theory segregation index (*H)* on a vector of leads and lags, relative to the timing of charter enrollment change. “Actual” refers to estimated coefficients and “Predicted” refers to estimated coefficients using as dependent measure the change in segregation as predicted from demographics. The confidence intervals are constructed using standard errors clustered at the county x school level unit and models predict white-Black (Panel A) and Poor-Nonpoor Theil segregation index (Panel B). Analysis is restricted to counties that experienced charter policies. Models include year x state fixed effects and account for contemporaneous effects on white, Asian, Hispanic, and Black (%), total enrollment (log), free-lunch enrollment (%), level of rurality (%), teacher/student ratio, income and unemployment (%). The sample size is 23,892 county-school level year observations (N=1,086 counties x level). F test of equality of coefficient for coefficients T<0 is non-significant for all models at conventional confidence levels. Period: 1993-2016





Panel A: Black-white segregation **Panel B: Poor-Non-Poor Segregation**

Figure A5: Estimated Effect of Magnet Enrollment Change on School Segregation

Note: The figure show coefficients from a regression in first difference predicting segregation on a vector of leads and lags, relative to the timing of magnet enrollment change. The confidence intervals are constructed using standard errors clustered at the county x level and models predict white-Black (left) and Free-Lunch segregation using the Theil segregation index. Analysis is restricted to counties that experienced charter policies. All models include year-state fixed-effects and account for contemporaneous effects on white, Asian, Hispanic, and Black (%), log public enrollment, per pupil expenditures, rurality (%), free lunch enrollment (%), median income and unemployment (%). The sample size is 11,792 county-school level year observations (N=536 counties x level). F test of equality of coefficient is non-significant for both models at conventional confidence levels. Period: 1993-2016.

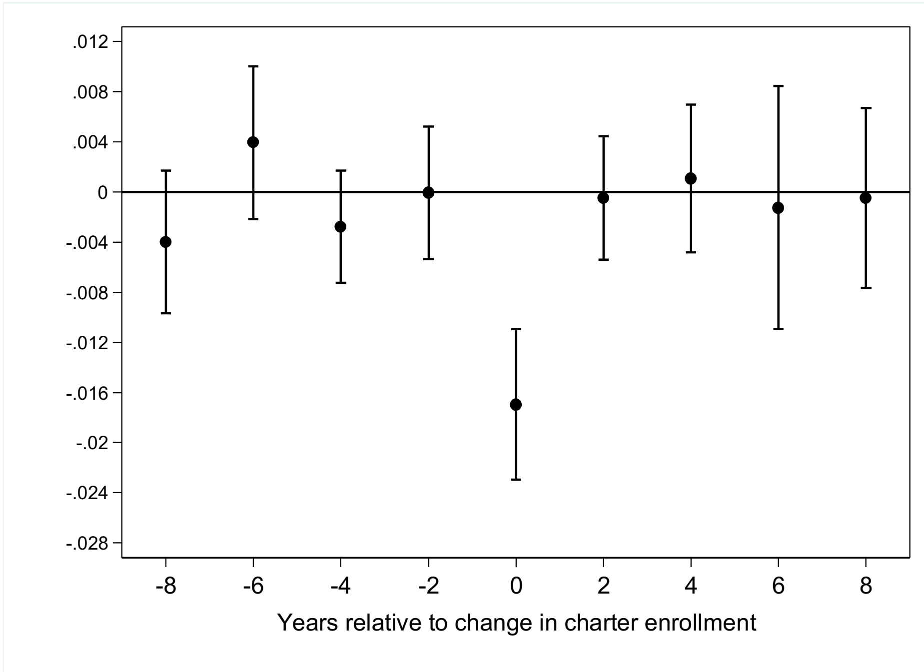


Figure A6: Estimated Effect of Charter Enrollment Change on Private School Enrollment (log)

Note: The graph shows coefficients from a regression predicting change (as first difference) in private enrollment (log) on a vector of leads and lags, relative to the timing of charter enrollment change. The confidence intervals are constructed using standard errors clustered at the county/level unit. Analysis is restricted to counties that experienced charter policies and private enrollment during the analyzed time-span. Models include year x state FE and account for contemporaneous effects on white, Asian, Hispanic, and Black (%), total enrollment (log), free-lunch enrollment (%), per pupil expenditures, level of rurality (%), teacher/student ratio, median income and unemployment (%). The sample size is 7,370 county-school level year observations (N=670 counties x level). F test of equality of coefficient for coefficients to the left of year "0" (T<0) is non-significant for all models at conventional confidence levels. Period: 1993-2016 (biennial).

Table A1: Robustness Checks: Segregation Measures

	White-Black	Free-lunch
	(1)	(2)
Baseline	0.0022**	0.0024**
	(0.0005)	(0.0005)
Cluster standard errors by state-decade	0.0022**	0.0024**
	(0.0004)	(0.0008)
County-level Random Effects	0.0022**	0.0024**
	(0.0002)	(0.0003)
AR (Within Counties)	0.002**	0.0023**
	(0.0002)	(0.0002)
Small changes (<0.5%)	0.0012	0.0004
	(0.0019)	(0.0022)
Large changes (>0.5%)	0.0022**	0.0024**
	(0.0005)	(0.0005)
Eight-year cumulative effect	0.0017**	0.0022**
	(0.0005)	(0.0005)

Note: ** denotes statistical significance at the 1% level, * at the 5% level, and † at the 10% level.

Table A2: Effect of One Additional Charter Enrollment Percentage Point

	Free-lunch	White-Black	White-Hispanic	Private Enrollment (log)
	(1)	(2)	(3)	(4)
Charter enrollment (WTC)	0.0014**	0.0013**	0.0002	-0.009*
	(0.0007)	(0.0006)	(0.001)	(0.004)
Number of observations		22,620		7,200
Placebo (WTC)	-0.0001	-0.0007	0.0003	-0.001
	(0.002)	(0.0012)	(0.0008)	(0.005)
Number of observations		16,569		4,140

Note: ** denotes statistical significance at the 1% level, * at the 5% level, and † at the 10% level. Table A2 shows Time-Corrected Wald DID estimators. This method exploits charter decrease as a second "treatment." Model 4 predicting private enrollment is biennial (half the sample). Likewise, counties with no private education are excluded from the sample (~30%). The placebo estimates predict a lagged version of the outcomes and only counties with a stable charter enrollment share between t and t-1 are included in the estimation. Standard errors are clustered at the county x school level. Period: 1993-2016.

Online Appendix, Part A: Data

National Center of Education Statistics Common Core of Data: To construct the county-level panel, we use enrollment counts from the Common Core Data by race and by free or reduced-price lunch eligibility. Regarding the latter, the CCD reports missing data due to some states' nonresponse, school non-response, and changes in the CCD survey format. In the early years of the CCD in particular, some states did not provide race and free and reduced-price lunch eligibility enrollment counts by school. Since the 1993–1994 school year, however, all states have reported school-level racial enrollment data, except for Idaho (which did not report race data until 2000–2001) and Tennessee (which did not report race data after 1998–1999), and most states reported free and reduced-price lunch eligibility data. Also, in each year, roughly 2 percent of schools have missing racial enrollment data, despite the fact that their state reported CCD data that year. We used linear interpolation and imputed 62,655 of 223,591 missing observations for the free and reduced price lunch variable. 10% of these imputed values come from one single year (2011) for which 80% of California schools did not provide this variable, whereas NY was missing all observations for 2003. 84% of our sample derives from school panels with 2 or few years missing for the free or reduced-lunch variable over the 22 year period, and missingness does not vary by the charter status of the school.

Charter school flag: In the years prior to 1998–1999, NCES does not report the type of public school provider. To address this issue, we applied the charter/magnet flag for 1998 to schools for the previous years. To minimize the potential misspecification due to conversions, we use the National Alliance Charter Public data of charter schools that has surveyed charters annually since 2007. We were able to match approximately 80% of the charter schools in the CCD data. 14% of the matched charter schools were corrected in the starting year. Charters that appeared in the CCD data as operating before the opening year in NACPS were flagged as traditional public for those observations. This resulted in labeling 71 new schools as charters that were not flagged in the CCD.

County Business Patterns panel: We used annual payroll averages weighted by the number of employees. Payroll and employment data, however, had additional data problems, since they vary more within establishments over time. We cleaned establishments with obvious errors in payroll or employment. To get payroll estimates, we dropped all records with zero annual payroll. We dropped all records with flags indicating inactivity, or that the record does not pertain to an establishment.

Financial school district panel: We use the NCES finance survey (F-33) to include per pupil expenditures (adjusted as to 2014 dollars) for each local education agency (LEA) and then aggregated at the corresponding county level. Following Lafortune et al. (2016), we excluded those with highly volatile enrollment trends and observations with implausible per-pupil funding, and consider "volatile" districts those in which enrollment growth more than doubled the district's average enrollment for all years. Then, we created a balanced school finance panel of approximately 11,000 school districts. We then collapsed the district measures to the county-by-year level, dividing total expenditures by total enrollment. We also dropped districts with an average enrollment for all years fewer than 100 students; this represented 7% of the sample, but is insignificant in terms of total enrollment.

Private school panel: Likewise, with the PSS data, if a school could not be observed in T, but was observed for T-1 and T+1, we imputed the information via linear interpolation (less than 2% of total school-year observations). We kept only elementary and secondary schools in our sample, as in the CCD data; we thus excluded schools which have pre-school as the highest grade.

Appendix, Part B: Information Theory Index

The Information Theory Index (*H index)*, originally proposed by Theil (1972), departs from traditional dissimilarity indices by computing the average deviation of each student's school racial diversity (level 1) from the county-wide racial diversity (level 2). In other words, this index departs from local environment group proportions:

(1) $$E = P * \ln(P) + (1 - P)\ln(1 - P)$$

And the diversity of school j is:

(2) $$E_j = p_j * \ln(p_j) + (1 - p_j)\ln(1 - p_j)$$

The information theory index is then:

(3) $$H = \sum_{j=1}^{j} \frac{t_j(E - E_j)}{TE}$$

Where j indexes schools and tj denote the total enrollment and proportion of, i.e. of Hispanic students, in school j, and T and P denote the total enrollment proportion of Hispanic in a given county.

Appendix, Part C: Sensitivity Analyses

We conducted multiple sensitivity analyses, and our findings were robust to these alternate specifications. For comparison's sake, in table A1, we include our baseline model in row 1, and alternative parametric models on subsequent rows. These include clustered standard errors by state-decade, county-level random effects models, and within-county correlation AR(1). Additionally, we restrict the sample to compare small and large changes in charter growth on segregation using 0.5% as cutoff. We note that only large changes have a statistically significant effect. Finally, we estimate a cumulative eight-year effect. Overall, we observe comparable estimates to our baseline estimates.

Appendix Table A1 around here

However, as Chaisemartin and D'Hautefeuille (2018) point out, our estimates are still likely to be biased to the extent that our identification presumes that the effect of our "treatment" is stable over time. There are reasons to think this assumption may not hold in our case: (a) information flows can change the selective nature of parental choices, especially when they have access to data produced by accountability systems (Hastings & Weinstein, 2008); (b) the underlying residential sorting impose "ceilings" on racial or socioeconomic segregation; as well as (c) the actions of schools as they respond to market-driven incentives (Lubienski et al., 2009), among others.

Following this strategy, we use the time-corrected Wald ratio (WTC) as an alternative identification of our parameter of interest, which does not rest on the assumption that treatment effects are stable and homogenous over time. Chaisemartin and D'Hautefeuille (2018) contend that units that have been treated and whose exposure to the treatment remains stable can be introduced as a valid potential outcome when the common trend assumption is satisfied. The identifying assumption here is that counties with an increasing exposure to charter programs would, in the absence of that increase, have had segregation trends similar to units with a stable exposure to the treatment. Therefore, units can switch back and forth between being exposed or not to the treatment.

We group together counties where the charter enrollment share remains stable between T and T+1 into a "super" control group, counties where charter enrollment increased into a "super" treatment group, and counties where the enrollment of charter decreased as a second "treatment" group. We rounded charter enrollment percent to the nearest 0.2 multiple to create a discrete measure that assumes "stability" of units. The DID identifies the local average treatment effect (LATE) of treatment group switchers. Then, a weighted sum of the estimands for each pair of dates identifies a weighted average of the LATEs of units switching at any point in time. Then standard errors are bootstrapped.

The Wald-DID estimand has two restrictions in addition to the common trend assumption: The average treatment effect of units treated at both dates must not change over time. When the share of units treated varies in the control group, the LATE of the treatment and control group switchers must be equal (stable treatment assumption). Let's consider a treatment D and an outcome Y and two periods:

$$W_{did} = \frac{E(Y_{11}) - E(Y_{10})}{E(D_{11}) - E(D_{10})} \quad (1)$$

The WTC is equivalent to the WDID except that its numerator compares and normalizes the mean outcome in the treatment group in period 1 to the counterfactual mean we would have observed if switchers had remained untreated. So, assuming δ_d represents the change in the mean outcome between period 0 and 1 for control group units with treatment status d:

(2) $$W_{tc} = \frac{E(Y_{11}) - E\left(Y_{10} + \delta_{D_{10}}\right)}{E(D_{11}) - E(D_{10})}$$

Table A2 includes our core models predicting racial and socioeconomic segregation using a fuzzy difference-in-difference estimation. Though smaller in size, the Wald Time-Corrected estimates corroborate our findings in terms of direction and significance. As in Chaisemartin and D'Hautefeuille (2018), we also perform a placebo test to check that our estimators are not driven by unobserved trends or serial correlation. To do so, we regress the change in counties' segregation at t−1 on their change in charter enrollment share between t−1 and t, restricting the sample to counties that did not experience a change in their charter enrollment share between t−1 and t−2. As Table 2 shows, the placebo coefficients are very close to 0 for all models, and are not statistically significant.

Appendix Table A2 around here