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The market-level effects of charter schools on student outcomes: A national analysis of school districts

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ABSTRACT

We study the total, market-level effects of charter schools, and their mechanisms, on a national level and across multiple outcomes. Using a generalized difference-in-differences method, we find that increasing the charter market share by 10 percentage points increases math and ELA elementary/middle test scores of the entire geographic district in which they locate by 0.01 standard deviations and increases the high school graduation rate by 1–2 percentage points. The effects are concentrated in larger, urban districts. The main mechanism appears to be the participant effect, though competitive effects are increasing in the participant effect and driven partly by the closure of low-performing traditional public schools. Causal interpretation of these findings is reinforced by analysis of potential endogeneity of charter school location and timing using placebo analysis and other methods. This study improves understanding of the array of charter mechanisms and their effects on multiple outcomes, on a national basis.

1. Introduction

Charter schools have become one of the most hotly contested topics in American education since they first started three decades ago. These publicly funded schools are exempt from many of the state laws and regulations that govern traditional public schools (TPSs), allowing charter schools to operate with more autonomy in hiring and managing teachers and in choosing curricula and instructional methods. Families can also choose charter schools regardless of whether they live within the neighborhood attendance zones that apply to traditional public schools. Governments still maintain significant control, but their role focuses more on holding charter schools accountable for their outcomes than operational management, akin to government contracting.

Proponents argue that charter schools and other market-driven approaches could increase innovation, create a better fit between schooling options and student needs, reduce bureaucratic inefficiencies, and increase competition among schools. Theoretically, this improves outcomes for all students, including families who do not actively choose (Goldhaber and Eide 2003), so that a "rising tide lifts all boats" (Hoxby 2003). Others, however, argue that charter schools engage in strategic behavior by selecting motivated, high-performing students (Bergman et al., 2020) and focusing on superficial improvements, such as marketing, rather than improving actual school efficiency (Lubienski 2007, Loeb et al., 2011, Jabbar 2015, Harris 2020). Charter schools might also divert funds in ways that make it more difficult for TPSs to succeed, due to economies of scale (Ni 2009, Ladd and Singleton 2020, Buerger and Harris 2021).

While trends in student math scores are ambiguous, ELA test scores and high school graduation rates have increased slightly faster in districts with charter schools compared with no-charter districts (Fig. 1). The question is whether this reflects a causal effect and, if so, through which mechanisms. Empirical research has examined some of these mechanisms (e.g., participant effects and some types of competitive effects), but only a few other studies have examined the total or market-level effects of charter schools (Gilraine et al., 2021, Ridley and Terrier 2022, Harris and Larsen 2023) and only with regard to test scores in individual states and cities.[1] We apply a generalized difference-in-differences (GDD) method using nationwide data from the National Longitudinal School Database (NLSD), which includes nearly all districts in the U.S. from school years 1995 to 2018. To capture the market-level effects, we estimate the effects of charter entry on a weighted average of TPS and charter outcomes. We define the market and unit of analysis as the geographic school district, while also testing alternative definitions.

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[1] Mehta (2017) and Ferreyra and Kosenok (2018) have also developed and estimated structural models of charter school entry, school input choices, and student school choices.

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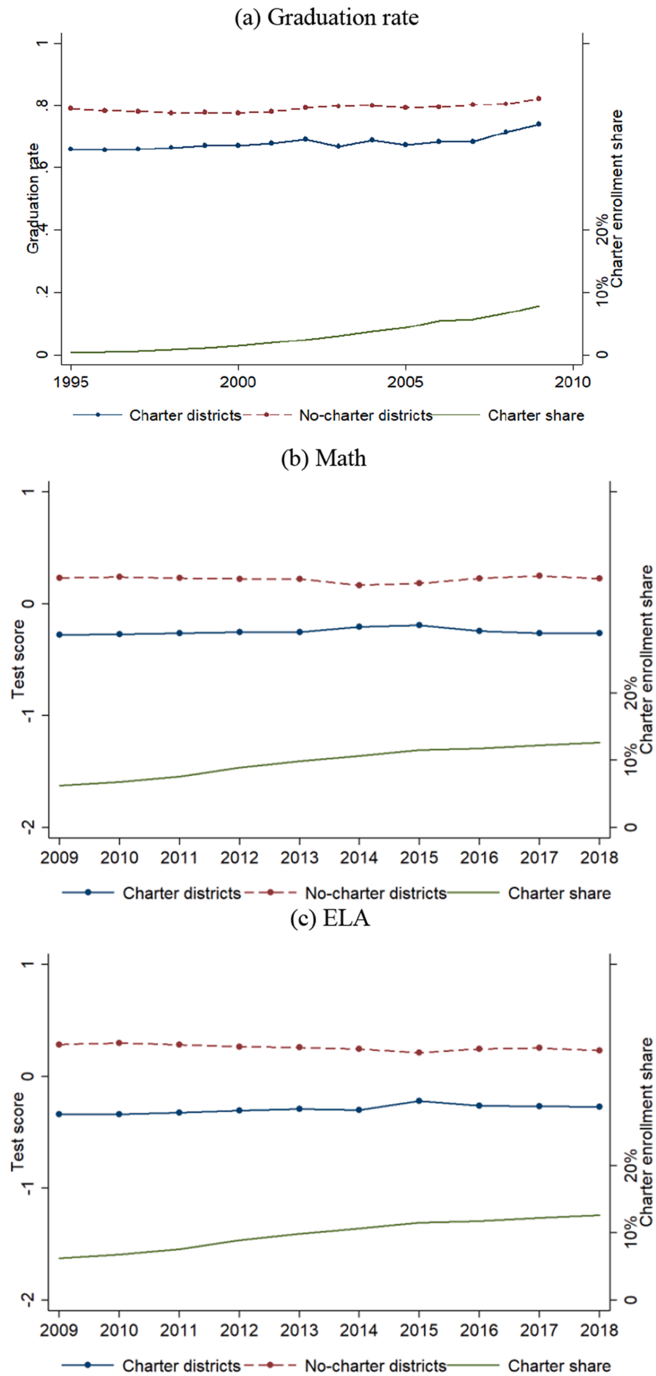

Fig. 1. Trends in graduation rate, Math, and ELA performance. Notes: This figure plots the trends in graduation rate, Math & ELA scores (s.d. units) of charter districts (in the solid line), and no–charter districts (in the dashed line). The green solid line plots the charter enrollment share of treated districts. (For interpretation of the references in colour in this figure legend, the reader is referred to the web version of this article.)

We find that charter entry increases market-level student outcomes. When charter market share increases by 10 percentage points, geographic district ELA test scores increase by 0.01 standard deviations and high school graduation rates increase by 1–2 percentage points. Positive effects also emerge for math in certain specifications, but, consistent with prior research (CREDO 2023), they are less robust. While the estimates may seem small, we note that they are average marginal effects across all students—not just charter school students—in districts with charter schools. The evidence on high school graduation is especially noteworthy given that almost all prior evidence on charter schools focuses on achievement and, to a lesser extent, college-going.[2]

Our GDD method accounts for all time-invariant differences across districts and assumes that there are no time-varying unobserved factors that are correlated with changes in charter market share *and* changes in student outcomes. That assumption might be plausible given prior evidence that charter schools tend to locate based on time-invariant, observable factors (Bettinger 2005, Glomm et al. 2005, Bifulco and Buerger 2015). But we still test for endogeneity in six ways: (1) we apply placebo analysis that tests for "effects" of the entry of charter *high schools* on *elementary/middle* school outcomes and vice versa; (2) we modify and implement the usual DD parallel trends test for this GDD setting; (3) we estimate "treatment effects" on population levels and demographics to test for sorting bias; (4) we estimate effects by state and compare these with the two prior well-identified studies that also incorporate multiple charter mechanisms (Ridley and Terrier 2022, Gilraine et al. 2021); (5) we use prior well-identified studies of the individual mechanisms comprising the market-level effect of charter schools (participant effects and near competitive effects) and "add them up" to test whether our estimated effect magnitudes are plausible; and (6) we aggregate the data up from the district to the commuting zone level as a further test for sorting bias. All six of the above tests reinforce that these are causal effects of charter school entry.

A second broad contribution of our study is identifying the broad array of potential mechanisms from which these positive effects emerge. First, charter schools might serve their own students more or less effectively than other schools, i.e., participant effects. The empirical strategies in this growing literature include matching (Furgeson et al. 2012, CREDO 2023), fixed effects (Brewer et al. 2003, Bifulco and Ladd 2006, Sass 2006, Booker et al. 2007, Hanushek et al. 2007), and lotteries (Hoxby and Rockoff 2004, Abdulkadiroğlu et al. 2011, Curto et al., 2014, Angrist et al. 2016). Positive participant effects could emerge if charter schools are innovative, use more efficient management designs, and/or experience greater pressure through their performance-based contracts (Harris, 2020). Participant effects vary, but the average is small, positive, and improving over time (Cohodes and Parham 2021).

Studies of the competitive effects of charter schools on student outcomes in TPSs are also common and generally find small positive effects on student achievement, which also vary across contexts, research methods, and measures of competition (Hoxby 2003, Bettinger 2005, Bifulco and Ladd 2006, Sass 2006, Ni 2009, Zimmer and Buddin 2009, Linick 2014, Cordes 2018, Ridley and Terrier 2022, Griffith 2019, Slungaard Mumma 2022). In a few cases, charter competition reduced student outcomes (Ni 2009, Imberman 2011).[3][4]

But these analyses of participant and competitive effects miss at least three other potential, less recognized mechanisms. One is that charter schools might induce low-performing TPS to close, improving overall outcomes by leading more students to attend high-performing schools. While this can be viewed as an extension of competitive effects, the above competition literature focuses only on the effects on TPS that *remain open*. This is what we would expect in other markets and there is evidence of it in the schooling market as well.[5] Previous studies have

[2] The main study of high school graduation is Booker et al. (2011).

[3] Results from Arizona (Hoxby 2003), Florida (Sass 2006), and Texas (Booker et al. 2008) suggest that there are positive competitive effects of charters on TPSs, and results from California (Zimmer and Buddin 2009) and North Carolina (Bifulco and Ladd 2006) suggest no effects. However, results from Michigan are mixed, with positive effects (Hoxby 2003), zero effects (Bettinger 2005), and negative effects (Ni 2009).

[4] A related literature exists on the competitive effects on TPS from private school vouchers (e.g. Figlio and Hart (2014).

[5] We also note that, if charter schools induce low-performing TPS to close, then this might bias "participant effects" downward as charter schools get compared with increasingly effective TPS.

examined the effects of school closure on student performance (Engberg et al. 2012, Bross, Harris, and Liu 2023, Carlson and Lavertu 2016) and charter entry effects on private school enrollment (Chakrabarti and Roy 2016), but not how charter entry affects the closure of TPS, i.e., "closure competitive effects."[6]

A fourth possible mechanism is that school district leaders respond to charter entry by changing policies and practices for TPS on a district-wide level. This, too, is a type of competitive effect, but most competitive effect studies do not capture this mechanism because they compare TPS located *nearby* charter schools to those located further away. But individual traditional public schools are usually funded at the discretion of district administrators and based on district-level finances, not school-level enrollment. This means that charter entry might spur district administrators to make broad-based changes and create a "far competitive effect," especially in geographically large districts where many TPS are located far away from newly entering charter schools.[7]

Fifth, in addition to participant effects, and the various types of competitive effects, the extension of choice could improve the match between students and schools. Campos and Kearns (2022) find that students in Los Angeles did better when attending their most preferred schools (measured by ranked preferences in a unified enrollment system), even after controlling for school value-added.[8] This "match effect" mechanism comes from a combination of the more differentiated set of schooling options that charters afford and the greater opportunity for families to choose schools aligned to their preferences.

Another contribution of the present study is providing evidence of the relative magnitudes of these mechanisms. We find that: (a) the market-level effects come largely, though not entirely, from the participant effects; (b) the competitive effects are increasing in the participant effect (i.e., charter school quality); and (c) a portion of the competitive effects may be due to the closure effect. Relatedly, we find little evidence that charter entry reduces private school enrollment.[9] This finding for private schools is useful for ruling out selection bias from charter schools inducing students to move in or out of private schools where we cannot measure their outcomes, and for understanding possible effects of charter schools on private schools, which could also affect educational outcomes and social welfare.

The national, system-level effects described above—0.01 s.d. on test scores and 1–2 percentage points on high school graduation—reflect the total effect of all of the above mechanisms.[10] Our analysis is most similar to three other studies.[11] Ridley and Terrier (2022) and Gilraine, Petronijevic, and Singleton (2021) study the net effects of multiple mechanisms on test scores in Massachusetts and North Carolina, respectively, leveraging exogenous changes in charter caps. In addition to being limited to individual states, these studies do not apparently capture all five charter mechanisms noted above.[12] Harris and Larsen (2023) also study the market-level effects of charter schools in New Orleans on test scores, high school graduation, and college-going, though this study is limited to a particular and unusual city. We build on this prior work by going beyond individual states and districts, studying the total effect of all the potential market mechanisms, and going beyond test scores to include high school graduation.

We also examine several novel forms of effect heterogeneity, starting with separate effects for each state. Arizona, Massachusetts, and Wisconsin are the only states where the point estimates are positive for all three outcomes and precise for at least two of them. While these state results are perhaps of interest in their own right, they also provide an informal test for identification of the national average treatment effects reported above. Our positive results for Massachusetts are qualitatively similar to those found by Ridley and Terrier (2022); and our small and imprecise effects for North Carolina are similar to those found by Gilraine et al. (2021).[13] As further corroboration, our results also align with prior findings that charter school effects are more positive in urban areas, for ELA (versus math) scores, and possibly for no-excuses charters (CREDO 2023, Chabrier et al. 2016, Cohodes and Parham 2021, Campos and Kearns 2022, Angrist et al., 2013b).

The paper proceeds as follows. Section 2 introduces the data. Section 3 discusses the methods and identification. Average treatment effects are discussed in Section 4 and heterogeneous analyses in Section 5. Section 6 provides additional analysis of the mechanisms behind these effects, especially the relative sizes, and interactions among, participant and competitive effects, and charter effects on TPS closure and private school enrollment. Section 7 concludes.

2. Data and descriptive statistics

This paper uses data from the National Longitudinal School Database (NLSD), which contains data for a near-census of all TPSs, charter schools, and private schools in the U.S. from 1991 to the most recent academic year.[14] The NLSD merges school and district level data from Common Core of Data (CCD), Stanford Education Data Archive (SEDA), Census Small Area Income and Poverty Estimates (SAIPE), and other sources. We are most interested in the student outcomes, test scores and graduation rates, but also include a vector of district-level covariates in some models. The street addresses of charter school also allow us to place the location of each charter school within its geographic school

[6] Dinerstein and Smith (2021) established a theoretical model of demand for and supply of private schooling and studied the private schools' response to public school funding.

[7] Case studies of school districts do sometimes suggest that school districts respond to charter competition (Hess 2004, Holley et al. 2013). Also, Figlio et al. (2022) find effects on whole districts from school vouchers. There is also evidence of Tiebout competition between districts (Hoxby 2000); see Rothstein (2007) for a response. Also, we note that, if these far competitive effects exist, then many of the competitive effect studies likely under-state effects on TPS because they identify effects from comparisons of near and far TPS, assuming the latter are untreated.

[8] While not specifically focused on charter schools, see evidence on the welfare effects of matching students to schools through centralized enrollment systems (Abdulkadiroğlu et al. 2017).

[9] One might argue that the absence of effects on private school enrollment is inconsistent with prior research that voucher programs create competition that improves TPS (Figlio and Hart 2014); however, those situations are different because voucher programs make private schools accessible to students who typically attend TPS. Here, we are focusing on the effects of charter schools on private schools that are financially less accessible.

[10] A sixth possible mechanism is that just the threat of charter entry could lead TPS to improve, but this is really just an element of the near, far, and closure competitive effects. That is, TPS might respond to the threat and/or to actual charter entry.

[11] Campos and Kearns (2022) focus on the introduction of Zones of Choice in Los Angeles, though this does not involve the entry of new schools or changes in charter market share.

[12] Ridley and Terrier (2022) explicitly omit charter outcomes and therefore focus on the various competitive effects. Gilraine et al. (2021) do use a weighted average of charter and TPS outcomes, but it is not clear how they handled TPS schools that close during the panel. An advantage of the present study is explicitly calling out these various mechanisms and how they are captured in the data.

[13] We cannot compare our results to Louisiana (Harris and Larsen 2023) as the vast majority of elementary/middle school charters entered in the aftermath of Hurricane Katrina in 2005 and the test score data do not start until 2009. Also, our high school graduation data end in 2010 and few high schools had been turned into charter schools at that point.

[14] All the school years mentioned in this paper are spring school years unless specifically stated otherwise.

district.[15] These districts define the scope of the market, i.e., which schools are assumed to compete with one another.

The specific dependent variables are the average freshmen graduation rate (AFGR) (hereafter, graduation rate) and student math and English Language Arts (ELA) test scores. The AFGR uses aggregate student enrollment data to estimate the size of an incoming freshman class and aggregate counts of the number of diplomas awarded four years later. For example, the AFGR for a school year in 2006 is the total number of diploma recipients in 2006 is divided by the average enrollment of the 8th-grade class in 2002, the 9th-grade class in 2003, and the 10th-grade class in 2004. The graduation rate sample includes schools covering grades 9–12 annually from 1995 to 2010.

The test scores are available in 3rd through 8th grade in math and ELA over the 2009–2018 school years from the Stanford Education Data Archive (SEDA). The SEDA provides nationally comparable, publicly available test score data for U.S. public school districts (Ho 2020).[16] For interpretation purposes, we normalize the test scores to have means of zero and standard deviations of unity within the grade, year, and subject.

Treatment status is defined as the charter enrollment share, or the percentage of students attending publicly funded schools who are enrolled in charter schools. These market share measures are created separately by grade level under the theory that charter schools only affect the geographic district outcomes in the specific grades being served (Jinnai 2014, Slungaard Mumma 2022).

For both outcomes, data are aggregated to the geographic district level by averaging TPS outcomes with those of charter schools located within their boundaries[17] (weighted by enrollment share[18]). This is crucial to the analysis as it allows us to capture charter effects operating through all of the various market mechanisms. In contrast, prior studies of charter schools have focused on the outcomes of individual schools in the examination of school participant effects or the competitive effects of individual charter schools on nearby TPS. Here, we are interested in the total effect of all the mechanisms, which means we need to account for the outcomes of all traditional *and* charter schools. Aggregating outcomes to the geographic district level also allows us to sidestep the main problem with prior studies, i.e., selection bias from students moving across schools (within districts).

The test score (graduation) data are available for 11,399 (10,658) districts, including 1,597 (1,073) districts that have at least one charter school. The final samples for the test scores (graduation) analyses include 97 (98) percent of the nation's publicly funded schools and 99 (97) percent of nation's charter schools.[19] Table 1 presents the summary statistics for the outcome and control variables. Compared with TPSs, charter schools nationwide tend to enroll a larger proportion of African American and Hispanic students. Charter schools also more likely to be in urban districts and where achievement is relatively low.

We are identifying total charter effects leveraging the timing of charter entry. One key factor affecting this is the timing of the passage of charter laws that allow them to entry. The first law allowing the establishment of public charter schools was passed in Minnesota in 1991. As of fall 2022, charter school legislation had been passed in 45 states and the District of Columbia.[20] Appendix Table A1 presents the years of charter school legislation as of 2022. As a result of the growing number of states allowing charter schools, the percentage of publicly funded schools that are charter schools increased from 0 to 7.3 percent, and their percentage of enrollment increased from 0 to 6.2 percent.[21]

Some districts, however, have much larger shares. New Orleans is the most well-known example and one of very few that are essentially 100 percent charter. Appendix Table A2 lists the top-20 districts in terms of charter market share, among districts with total enrollments larger than 10,000 students. Appendix Fig. A3 also reports the distribution of maximum charter shares, which shows the heavy left skew of the distribution; almost all districts with a charter school have less than 20 percent market share. At the same time, there are many high-charter-share districts other than Boston, New Orleans, and others that have dominated the research and public discourse.

To highlight the variation in charter market share we are exploiting, Fig. 2 lists the trajectory of charter school growth from the point of the first charter entrant. We report separate lines based on the maximum charter (enrollment) market share that appears in our data. As expected, growth rates are faster in geographic districts that have higher eventual market shares and diminish over time.[22]

3. Identification strategy

The GDD identifies effects from within-district variation in dosage over time (for those ever treated) as well as differences between treated and untreated units (Mouganie et al. 2020, Dow et al. 2020). This is preferable to a simple DD, in cases such as this with continuous, time-varying treatment. A simple DD would ignore most of the variation in charter market share and focus only on the point where the first charter school enters and treatment begins.

The dependent variables in our GDD analysis are test scores ($Test_{isgt}$) and high school graduation (GR_{ist}) in district i state s and grade g during year t (test scores are grade-specific; graduation is not). We include geographic unit fixed effects μ_i (usually geographic school districts) as

[15] The SEDA team uses the 2019 Elementary and Unified School District Boundaries https://nces.ed.gov/programs/edge/Geographic/DistrictBoundaries) to define the Geographic School District for test score sample (except for special education or virtual schools). We use the same boundaries to define the Geographic School District for the graduation sample.

[16] To make estimates are comparable across states, grades, and years. The SEDA research team took the following steps: (1) estimate the location of each state's proficiency "thresholds" in the distribution of scores; (2) place the proficiency thresholds on the same scale using the National Assessment of Educational Progress (NAEP), a test taken by a representative sample of students in each state; (3) estimate the mean test scores in each school, district, county, metropolitan statistical area, commuting zone, and state from the raw data and the threshold estimates, and (4) create estimates of average scale scores and achievement growth measures. See details in SEDA website https://edopportunity.org/methods/. While this method is of course based on assumptions (e.g., that the NAEP sample is truly state representative and the distribution of state scores is the same as the NAEP distribution despite the differing content of these various tests), this could only result in bias if test error in charter-entering districts differs from never-charter districts (within grades and states and conditional on other included covariates) and changes over time in conjunction with charter school entry. We could not come up with potential examples where this might be the case.

[17] SEDA provides test score data at the geographic district level. We converted the graduation rate data to the geographic district level ourselves. Please refer to Appendix B for the description that how the sample was created.

[18] Weighting is necessary to obtain a nationally representative effect (e.g., to avoid counting New York City the same as small rural districts). However, as Solon et al. (2015) note, weighting does not necessarily yield a consistent estimate of the population average treatment effect if the covariate variance depends on the weight. They therefore advise reporting results weighted and unweighted; and testing for effect heterogeneity by weight. We provide these additional results and discussion later as well.

[19] Since geographic districts are the unit of analysis, an observation is only missing if all the schools in the unit-by-school-type are missing. There are likely some missing schools within districts. Appendix B describes in detail how we compose the samples.

[20] The states in which public charter school legislation had not been passed by that time were Montana, Nebraska, North Dakota, South Dakota, and Vermont.

[21] Appendix Fig. A1 presents the trends in charter school share and charter enrollment share from spring 1991 to spring 2018.

[22] This non-linear pattern partially reflects the opening of the first charter school followed by enrollment growth in those schools, not just opening more schools.

Table 1
Summary statistics.

Districts	Graduation sample			Test score sample		
	All	No-charters	With charters	All	No-charters	With charters
Graduation rate	0.75	0.79	0.68	N.A.	N.A.	N.A.
Math	N.A.	N.A.	N.A.	0.00	0.24	−0.24
ELA	N.A.	N.A.	N.A.	0.00	0.28	−0.28
Enrollment	61,668	12,098	147,584	57,478	10,254	104,979
White	0.64	0.74	0.46	0.54	0.68	0.39
Black	0.16	0.12	0.24	0.17	0.11	0.23
Hispanic	0.14	0.09	0.24	0.24	0.15	0.32
FRL	0.30	0.26	0.36	0.52	0.45	0.59
Special education	0.13	0.12	0.13	0.13	0.14	0.12
Urban	0.30	0.16	0.54	0.29	0.12	0.46
Suburb	0.37	0.38	0.36	0.42	0.41	0.42
Town	0.13	0.18	0.05	0.11	0.17	0.05
Rural	0.20	0.28	0.05	0.18	0.29	0.07
Revenue per student	9,015	9,027	8,996	12,578	13,085	12,067
Expenditure per student	9,180	9,143	9,243	12,574	13,027	12,118
Student-teacher ratio	16.72	16.11	17.78	16.66	15.95	17.38
Teacher salary	79,394	77,023	83,503	101,674	101,698	101,650
No. magnet school	1.17	0.13	2.95	4.64	0.62	8.69
Observation	156,436	141,025	15,411	525,816	447,831	77,985
N (district)	10,629	9,556	1,073	11,399	9,802	1,597

Notes: This table presents means of outcome variables and control variables (weighted by the first-period enrolment).

well as state-grade-year fixed effects λ_{sgt}. This implies the following models:

$$GR_{ist} = \alpha + \beta_1 ChartShare_{ist} + X_{ist}\gamma + \mu_i + \lambda_{st} + \varepsilon_{ist} \tag{1}$$

$$Test_{isgt} = \alpha + \beta_2 ChartShare_{isgt} + X_{ist}\gamma + \mu_i + \lambda_{sgt} + \varepsilon_{isgt} \tag{2}$$

Treatment is the continuous, time-varying charter market share, $ChartShare_{ist}$. The coefficients of interest are represented by $\boldsymbol{\beta}$. X_{ist} is a vector of time-varying student covariates (log enrollment, share of black, white, Hispanic, FRL, and special education students) and other district-level covariates (district revenue per student, district expenditure per student, student–teacher ratio, teacher salary, number of magnet schools, and an indicator of whether a charter law has gone into effect). As these covariates are potentially endogenous, they are only included in certain specifications and as partial tests for charter effects on the student population.

The error term ε_{isgt} is clustered at the district level in the main specification. (We report results clustered at the state level in Appendix Table A3, which only slightly reduces precision.) The estimates are weighted by the district size, so that the estimates are nationally representative; specifically, we weight by graduation rate denominator in the high school sample and by grade-level enrollment for math and ELA. In our baseline analyses, we estimate the GDD model for both the current year charter enrollment share and with a one-year lag to allow delayed effects.

Our method accounts for time-invariant unobserved differences across districts and our main identifying assumption is that there are no unobserved time-varying factors that are correlated with changes charter market share *and* changes in student outcomes. We note that, even if charter market share changed based on time-invariant unobserved district characteristics that affected student outcomes, this would not introduce bias if the timing of charter entry were conditionally random, which is possible given all the steps involved in opening a charter school (e.g., putting in an application, getting it approved, gaining access to a building, recruiting students, and hiring staff).

Still, it is worth considering specific scenarios under which this might be violated. First, charter schools are more likely to locate in districts with low contemporaneous student outcomes (Glomm et al. 2005), signaling that we also might expect charter schools to also locate where *expected* future TPS performance is lower, though this is unobservable in our analysis.[23] This would lead to a downward bias in our estimates because TPS outcomes comprise the majority of the weighted average outcomes.

Alternatively, local governments might change non-charter education policies at the same time they introduce charter schools, as part of a package of reforms. Since school districts are often charter authorizers, it may be that a change in school board politics leads both to an increase in charter schools and, for example, new reading and math curricula in TPS. In this case, we might falsely attribute outcome changes to charter schools that were actually caused by other policy changes. We discuss various tests for these and related types of violations in the next section, after describing our main results.

4. Results

4.1. Average Treatment Effects from GDD

We find consistent evidence that charter entry improves geographic-district-level outcomes. Table 2 reports estimates from the GDD models in equations (1) and (2) where a one-unit change in charter market share means going from zero to 100 percent charter market share. Column (4), for example, shows that a 10-percentage point increase in high school charter enrollment share increases the high school graduation rate one year later by 1.43 percentage points; the same change in elementary charter enrollment share increases ELA scores by 0.0113 standard deviation (s.d.). The estimates for high school graduation and ELA scores are generally larger and more precisely estimated and those for math. Contemporaneous effects are also smaller than the lagged effects

[23] Relatedly, charter schools might open where districts experience idiosyncratic shocks, in which case future outcomes regress toward the mean. If charter schools prefer to locate near low-performing school, as noted above, then this would yield an upward bias (i.e., charter schools enter because of the negative shock in existing schools, but then those outcomes bounce back in the next period). We do not see this scenario as very likely because it takes several years to create an organization that can assemble a charter application, submit the application and gain approval, hire personnel, purchase necessary capital, and recruit students. Also, with such a long-term investment, charter organizations are likely to consider multiple years of information in making their entry decisions, which reduces the chance of regression to the mean.

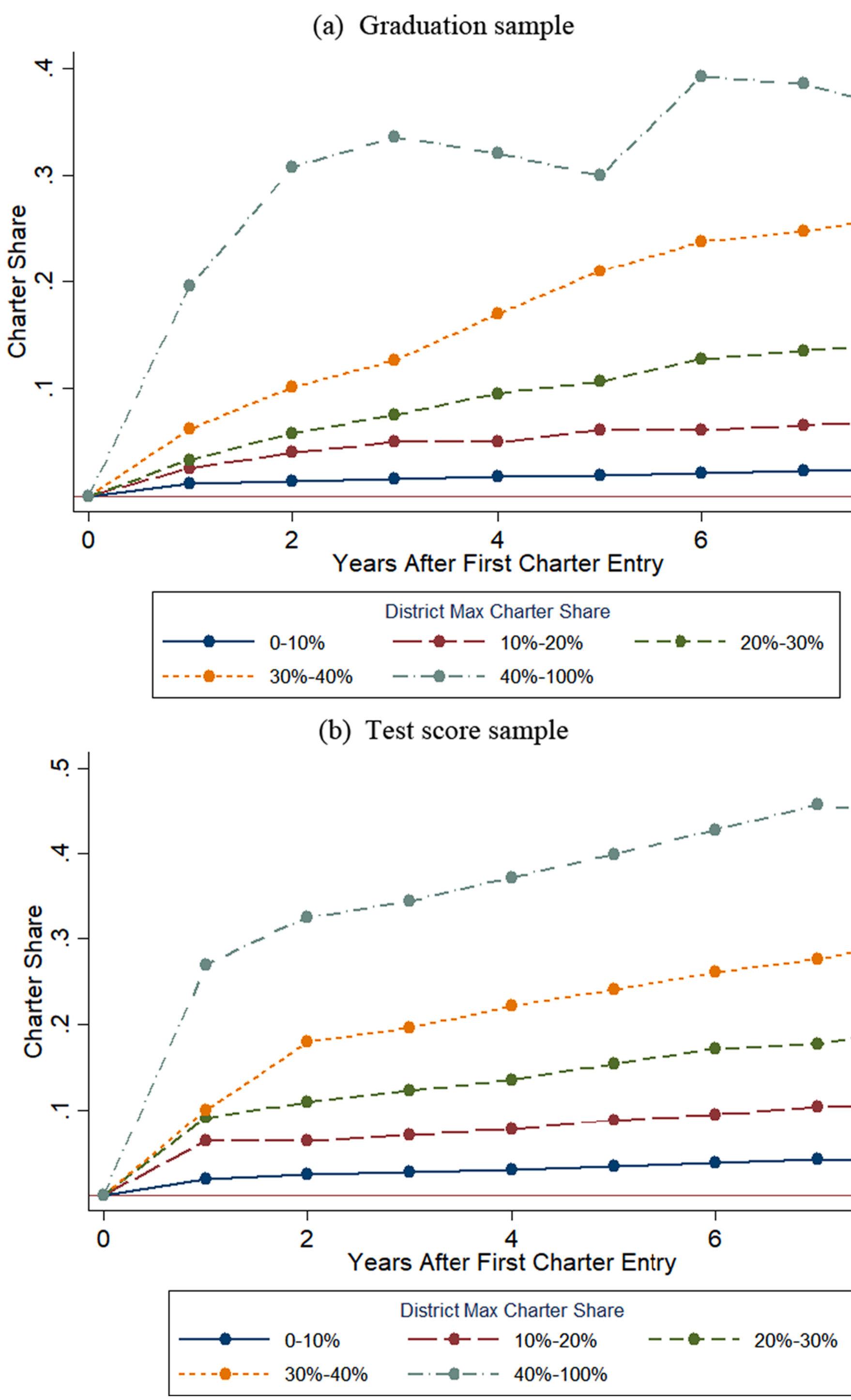

Fig. 2. Trends in charter share by maximum charter share.

(compare columns 1–3 versus 4–6). This delay could reflect gradual improvement of charter schools (participant effects) in their early years, a slightly delayed competitive response by TPS, and/or improved matching as students learn more about the new schools and sort over time.

The above main specification includes all districts (with non-missing data). However, as discussed later, the effects of the first charter entrant might differ from the effects of subsequent ones (e.g., because of diminishing returns); and, especially in the test score sample, many districts had their first charter schools enter *before* the first year in the panel. To address this, Table 3 reports the results using only treatment districts where the first charter school entered after the first year of data.

These results show more positive effects on math scores, while the ELA and graduation results remain similar to above.[24] In additional analysis, we also tested for non-linear effects using polynomials of charter market share, which also yielded results consistent with diminishing returns, though these estimates are imprecise.

[24] As an additional robustness check, we also re-estimated the models using a double-log specification (see Appendix Table A7). These results are qualitatively quite similar to Table 2, e.g., the results are still precisely estimated and positive for graduation rates and ELA and, in the latter case, only with a one-year delay.

Table 2
Effects of charter entry on student outcomes.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Graduation rate						
	Same year share			Lag one year share		
Charter share	0.066	0.078	0.076	0.143***	0.147***	0.146***
	[0.063]	[0.063]	[0.064]	[0.034]	[0.034]	[0.034]
R-squared	0.852	0.853	0.854	0.853	0.854	0.854
Observations	156,436	156,436	156,436	156,436	156,436	156,436
N(district)	10,629	10,629	10,629	10,629	10,629	10,629
Panel B: Math						
	Same year share			Same cohort last year		
Charter share	−0.059	−0.123	−0.076	0.025	−0.019	0.021
	[0.088]	[0.090]	[0.072]	[0.060]	[0.058]	[0.049]
R-squared	0.861	0.861	0.862	0.861	0.861	0.862
Observations	525,502	525,502	525,502	525,502	525,502	525,502
N(district)	11,399	11,399	11,399	11,399	11,399	11,399
Panel C: ELA						
	Same year share			Same cohort last year		
Charter share	0.138*	0.057	0.092	0.113*	0.055	0.089*
	[0.070]	[0.066]	[0.064]	[0.060]	[0.053]	[0.051]
R-squared	0.890	0.891	0.891	0.890	0.891	0.891
Observations	525,502	525,502	525,502	525,502	525,502	525,502
N(district)	11,399	11,399	11,399	11,399	11,399	11,399
District FE,State-by-year(-by-grade) FE	Yes	Yes	Yes	Yes	Yes	Yes
Student controls	No	Yes	Yes	No	Yes	Yes
District controls	No	No	Yes	No	No	Yes

Notes: The table shows estimated effects of charter entry on student outcomes based on equations (1) and (2). Charter share in the data set range from 0 to 1. In columns 1–3, charter enrollment share of grades 9–12 is used for graduation rate, and charter enrollment share of each grades is used for (SEDA) test scores. Student controls include log of enrollment, share of black, white, Hispanic, FRL, special education. District controls include revenue per student, expenditure per student, student–teacher ratio, teacher salary, number of magnet schools, and an indicator of charter law effectiveness. Regressions are weighted by the first-period enrollment. Standard errors are clustered at the district level.

Table 3
Effects of charter entry on student outcomes (drop always treated districts).

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Graduation rate						
	Same year share			Lag one year share		
Charter share	0.063	0.074	0.071	0.140***	0.143***	0.140***
	[0.068]	[0.068]	[0.070]	[0.039]	[0.039]	[0.039]
R-squared	0.848	0.849	0.850	0.849	0.850	0.850
Observations	155,958	155,958	155,958	155,958	155,958	155,958
N(district)	10,597	10,597	10,597	10,597	10,597	10,597
Panel B: Math						
	Same year share			Same cohort last year		
Charter share	0.088	0.072	0.085	0.148*	0.137*	0.152*
	[0.089]	[0.087]	[0.087]	[0.083]	[0.082]	[0.082]
R-squared	0.831	0.832	0.832	0.831	0.832	0.832
Observations	471,144	471,144	471,144	471,144	471,144	471,144
N(district)	10,286	10,286	10,286	10,286	10,286	10,286
Panel C: ELA						
	Same year share			Same cohort last year		
Charter share	0.049	0.034	0.053	0.077	0.067	0.087
	[0.086]	[0.084]	[0.084]	[0.084]	[0.082]	[0.083]
R-squared	0.864	0.864	0.865	0.864	0.864	0.865
Observations	471,144	471,144	471,144	471,144	471,144	471,144
N(district)	10,286	10,286	10,286	10,286	10,286	10,286
District FE,State-by-year(-by-grade) FE	Yes	Yes	Yes	Yes	Yes	Yes
Student controls	No	Yes	Yes	No	Yes	Yes
District controls	No	No	Yes	No	No	Yes

Notes: The table is the same as Table 2, except dropping always treated districts. See other notes in Table 2.

Comparing across columns within Tables 2 and 3, we see that the results are robust to the addition of student and district controls. The results are also similar when we switch the charter market treatment variable from the share of *students*, in Table 2, to the share of *schools* (Appendix Table A4). Our main specification also includes state-grade-year fixed effects and the point estimates are also generally robust to the addition of district-specific time trends (Appendix Table A5) for high school graduation, but less so for test scores; again, when we drop the always-treated districts and focus on the first charter entrants, the results with district-specific trends are more robust. (Appendix Table A6).

Again, the dependent variable is a weighted average of outcomes for all publicly funded schools in the geographic district, so that the coefficients reflect estimates of the market-level average treatment effects, capturing the full range of mechanisms discussed earlier (participant effects, near competitive effects, and so on).

4.2. Testing Threats to Identification

The identifying assumption of our estimates is that there are no time-varying unobserved factors that are correlated with both changes charter market share and changes in student outcomes. In this section, we test this assumption in six ways.

4.2.1. Placebo using misaligned grades

First, we use placebo analyses that leverage the fact that the entry of charter high schools should not directly affect elementary student outcomes. The effects of elementary charter entry on high school graduation should also be minimal, within the scope of our panel, because there are so many years in between elementary school attendance and high school graduation.

Table 4 shows results for these placebo analyses. The first pair of columns reports the effects of elementary charter entry on high school graduation, while the last two pairs of columns show the estimated effects of high school entry on elementary/middle scores. As predicted, the estimated effects are much smaller, about one-tenth the size, of treatment effects in Table 2. We also re-estimated using a one-year lag in outcomes, to allow for possible delayed effects (as in Table 2), but the results are very similar.[25]

4.2.2. Parallel trends

In the usual DD, visual evidence of parallel trends is provided from event studies, which, in their two-way fixed effects (TWFE) formulation, would take the form:

$$GR_{ist} = \alpha + \sum_{r=-m}^{q} \beta_r (T_i \cdot d_{i,r}) + X_{ist}\gamma + \mu_i + \lambda_{st} + \varepsilon_{ist} \tag{3}$$

$$Test_{isgt} = \alpha + \sum_{r=-m}^{q} \beta_r (T_i \cdot d_{i,r}) + X_{ist}\gamma + \mu_i + \lambda_{sgt} + \varepsilon_{isgt} \tag{4}$$

where T_i is a treated district and $d_{i,r}$ is an indicator of the r years of leads or lags since district i initiated the first charter school. This test is less definitive with the GDD, which identifies effects using all the within-district variation in charter schools and outcomes over time. We therefore modify the usual test so that $T_i = 1$ only for districts that experience a spike in charter market share of at least two percentage points in three years (regardless of the initial charter market share level) and the treatment turns on at the point of the spike. The vector $\boldsymbol{\beta_r}$ represents measures of cohort-specific effects compared with the comparison group.

It is also now well known that the TWFE version of the DD can be biased when treatment timing is staggered (Goodman-Bacon 2021). Fig. 3 therefore reports the interaction weighted (IW) event study estimates recommended by Sun and Abraham (2021) where treated is defined by spikes in charter enrollment. All the results pass pre-trends for high school graduation, as well as ELA (albeit with a one-year drop at t = -2). With the math sample, we see some evidence of a declining pre-trend.

Another reason for reporting event studies is that we can also see the post-treatment dynamic effects. In Fig. 3, the results closely mirror the main treatment effects in Table 2 with clear evidence of positive effects on high school graduation and ELA scores. With math, it appears that charter schools stop the pre-treatment decline in scores. This may explain why the math results look less positive than ELA generally.

4.2.3. Effects on Population and Demographics

Student outcomes are positively correlated with household socioeconomic status, so it could be that charter entry arises in conjunction with changes in local social and economic conditions that are related to demographic sorting patterns. While the addition of covariates in the GDD suggests that population change is not an important factor, we can test this more directly by estimating "treatment effects" on population levels and demographics using the same method (see equations (1) and (2)).

In Table 5, we see no precisely estimated effects on either student cohort size or student poverty (as measured by free or reduced-price lunch (FRL) eligibility). The point estimates are also very small in magnitude. For example, the largest (in absolute value) coefficient suggests that increasing charter entry by 10 percentage points increases FRL by 0.44 percentage points. In short, we see no evidence that charter effects really reflect changing household demographics.

4.2.4. Aggregation to the Commuting Zone

The above analysis seems to rule out meaningful cross-district shifts in the population that could be driving the results. However, it is also possible, as suggested by our earlier discussion of the "far competitive effects," that the total effects extend beyond district boundaries. Since the total effects appear positive, this logic suggests that the geographic-district-level estimates are biased downward. Indeed, the community-zone-level results (Appendix Table A8) suggest even larger effects than previously reported, though these are imprecise at least in part because of the substantial drop in the sample size. These results also reinforce that the earlier effects are not driven by cross-district sorting based on unobservables.

4.2.5. Comparison with Massachusetts, North Carolina, and New Orleans Studies

Our analysis is most similar to Ridley and Terrier (2022) and Gilraine et al. (2021) who study the net effects of multiple mechanisms on test scores in Massachusetts and North Carolina, respectively, leveraging exogenous changes in charter caps. While we do not have access to this form of exogenous variation in all states, we can test for bias by re-estimating effects for each state separately and comparing our results in these two states to the prior studies (for test scores only as their estimates do not include graduation).

The state-by-state results, shown in Appendix Fig. A2 are qualitatively similar to those from Massachusetts where Ridley and Terrier (2022) also find positive effects on test scores; and similar to those from North Carolina where Gilraine et al. (2021) also find null effects. While

[25] The estimated "effects" of elementary schools on high school outcomes are very small but precisely estimated. This could imply either: (a) a small indirect effect of elementary charter schools on high schools; or (b) evidence of a small degree of endogeneity affecting the estimates of all the school levels. As an example of indirect effects, charter entry could affect parent or educator expectations for students at all levels. As an example of endogeneity, if charter schools of all grade levels enter in districts at a time when districts are undergoing unobserved shocks in population demographics, then elementary charter entry might give the misleading appearance of positive effect on high school outcomes, similar to what we see here. But, again, the very small size of the placebo estimates implies that neither mechanism is very influential and the bias is limited.

the magnitudes of the estimates are difficult to compare, given differences in research methodology (see footnote 12), the general alignment of conclusions reinforces our findings. This analysis also raises questions for future research about the possible reasons for state variation. In addition to the state policy rankings (see above and Table 6), possible explanations include variation in charter funding, the local supply of quality teachers, the degree to which charter schools are differentiated from TPS (Gilraine et al., 2021), and the proportion of schools that are in high-performing sub-categories (e.g., CMOs and no-excuses).

Our national results also happen to line up well with those of the third study of market-level effects. In New Orleans, the charter school share went from almost zero to 100 percent and increased test scores, high school graduation, and college outcomes (Harris and Larsen 2023). In Table 2, this type of shift implies effects of 15 percentage points on high school graduation and 0.06–0.11 s.d. increase in ELA test scores—figures similar to those reported by Harris and Larsen (2023).

4.2.6. Comparisons with Other Studies

As a final approach to testing for endogeneity, we use prior well-identified studies of the individual mechanisms and "add them up" to provide a rough test of whether our estimated effects of the system-level charter effects are plausible. If the system-level effects seem to go in the wrong direction or are too large in magnitude, then this would indicate that the identifying assumption is violated.

The effect sizes in Table 2 appear plausible given the prior literature. CREDO (2023) reports an average national treatment effect on reading scores, across 31 states, of 0.023–0.035 s.d.[26] While we do not have the precise average charter market share for the CREDO (2023) sample, if we assume a realistic average market share of 10 percent, this implies the participant effect portion of our system level effect for ELA is $0.10*0.035 = 0.0035$ s.d., or roughly one-half to one-third the system-level effect sizes reported in Tables 2 and 3. The "near competitive effects" are also usually small and positive. Bifulco and Ladd (2006), for example, find that TPS exposure to a charter school increases TPS achievement by 0.02 s.d. Again, this affect only applies to the fraction of TPS that are near charter schools, but this, too, points to the idea of a small, positive system-level effect of the sort found in Table 2 and other tables.

5. Effect heterogeneity

5.1. By District Size

Prior research has suggested that charter school participant effects vary by urbanicity and student demographics. We also find these patterns. Column (1) on Table 6 presents the heterogeneous effects in Metropolitan Areas (MA) and non-MA. The results show that compared with non-MA districts, a 10 percent increase in charter share improves math and ELA scores in MAs by 0.01–0.02 standard deviations more than in non-MA districts. This pattern is consistent with prior charter school participant effects by urbanicity (CREDO, 2023, Chabrier, Cohodes, and Oreopoulos 2016).

One reason for carrying out effect heterogeneity tests by MA is that this is closely related to district size, which we have used to weight the effects throughout the paper. While our intent with weighting has been to identify an average treatment effect that is nationally representative, Solon, Haider, and Wooldridge (SHW, 2015) note that weighting does not necessarily accomplish this and can yield inconsistent estimates under some conditions. They advise, first, testing for effect heterogeneity by the weighting factor (in this case, geographic district enrollment). Since MA is closely related to district enrollment, Table 6 fulfills this first SHW recommendation. (The results are similar when we interact using the enrollment/cohort size instead of MA.) The fact that larger effects arise in larger districts means our preferred specification involves upweighting the more positive effects.

SHW (2015) also recommend estimating effects unweighted. As expected, given the above effect heterogeneity, these unweighted estimates are small in magnitude and imprecise because almost all the weight goes to small districts that, as demonstrated, have small/null treatment effects (Appendix Table A9). While this could be interpreted to mean that charter schools have no effects outside urban areas, we note that the effects are positive in non-MA districts when dropping the always-treated districts (Appendix Table A10).

5.2. By No Excuses School Status

Research has found particularly large effects of no-excuses charter schools, which embrace high expectations for students, make active use of achievement data in personnel and other management decisions, and often provide extra learning time (Angrist et al., 2013b). While the no-excuses orientation of schools is poorly measured on a national basis, we can identify particular no-excuses charter school networks: KIPP and Success Academies. Our results, shown in the last column of Table 6, are generally consistent with prior results. Though imprecise, the interaction terms are positive for all three outcomes.[27]

The other columns in Table 6 show more positive graduation effects for Black, Hispanic, and low-income (FRL) students; however, the test scores interactions for Black students run in the opposite direction. This also runs against the conventional wisdom, based on participant effect studies, which is not necessarily surprising since we are estimating market-level effects and not just participant effects. Some additional evidence in Florida suggests, consistent with our findings, that competitive effects are negative for Black students (Figlio et al., 2022), but the reasons for this are unknown.

It is also possible that charter effects vary because some traditional public schools are performing so poorly, so that charter schools address the "low hanging fruit" of school improvement. To test this, we estimate versions of equations (1) and (2) but adding an interaction between treatment and baseline student achievement level. These interactions, shown in column (6) of Table 6, are consistent with the low hanging fruit hypothesis, though only imprecisely estimated in the case of high school graduation.

Our average treatment effects also combine results across states that have different charter policies (e.g., how well funded charter schools are relative to traditional public schools, charter authorization and shutdown policies). Since there are many dimensions to these policies we interacted treatment with state charter law ratings reported by the National Alliance for Public Charter Schools (NAPCS) in 2020 (Ziebarth 2020).[28] The results in column (5) of Table 6 point toward a positive

[26] This range reflects the effects on math and ELA, respectively, after removing virtual charter schools. The number used in the calculation (0.03) is the average of these two numbers.

[27] These two charter networks include only two percent (0.25 percent) of charter market share at the elementary/middle (high) school levels. Prior studies have focused on KIPP's college outcomes over high school graduation (Demers et al. 2023). Even if they did report graduation, the results would be difficult to compare because we are examining market-level effects rather than the participant effects and our results also include Success Academies, not only KIPP. Also, prior research suggests that CMO charter schools are more effective than standalone charters, and KIPP and Success Academies are examples of CMOs, so the "no-excuses effect" is difficult to disentangle from the CMO effect. Clearer understanding of these distinctions will require additional data in future research.

[28] NAPCS rated each state on 21 key components of state law, such as accountability, authorization, flexibility, performance-based contracts, and funding equity. NAPCS gave each component a weight of 1 to 4 and multiplied these by the component rating to obtain an index for each state. We divided these scores by the total score and therefore, the value of scores ranges from zero to one with the mean of 0.53 and the standard deviation of 0.23.

Table 4
Placebo tests using charter shares of misaligned grades or subsequent years.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Graduation			Math			ELA		
Charter share	0.015***	0.015***	0.016***	−0.012	−0.013	−0.011	−0.003	−0.004	−0.002
	[0.005]	[0.005]	[0.005]	[0.017]	[0.017]	[0.016]	[0.015]	[0.015]	[0.015]
R-squared	0.853	0.853	0.854	0.861	0.861	0.862	0.890	0.891	0.891
Observations	156,436	156,436	156,436	525,502	525,502	525,502	525,502	525,502	525,502
N(district)	10,629	10,629	10,629	11,399	11,399	11,399	11,399	11,399	11,399
District FE,State-by-year(-by-grade) FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
District controls	No	No	Yes	No	No	Yes	No	No	Yes

Notes: The table shows three placebo tests using misaligned grade levels: effects of charter entry in grades (1–5) on high school graduation; effects of charter entry in grades 9–11 on Math and ELA in grade 3–5; and effects of charter in grades 1–3 on Math and ELA in grade 6–8. See other notes in Table 2.

interaction between charter effects and the policy index for all three outcomes, though, with the interaction term being at the state level, these estimates are very imprecise.[29]

Finally, we estimate heterogeneous effects by school grade levels for test scores and report the results in Appendix Table A11. The results show precisely estimated positive effects on both math and ELA test scores for middle school students (grades 6–8), but smaller and less robust effects for elementary school students (grades 3–5). Specifically, increasing the charter middle school market share 10 percentage points increases Math and ELA scores of middle schools by 0.01–0.04 standard deviations.

6. Mechanisms of the market-level charter effect

The above analyses estimate the total effects of charter entry on entire school districts, capturing all five mechanisms, collectively. In this section, we attempt to disentangle the participant and various competitive effects. We also drill down on the competitive effects to provide new evidence about the closure effect. These analyses of specific mechanisms require coupling geographic-district-level data with school-level data.

6.1. Participant versus Other Effects

One simple way to distinguish the participant effects from the various competitive effects (collectively) is to change the dependent variable to exclude charter school outcomes. This is similar to the method used in the competitive effects literature writ large, except that, by including all schools in the district, we capture not only near but far competitive effects and closure effects at the same time. To accomplish this, we need to restrict the dependent variable to the outcomes of traditional public schools. As this is not possible with the SEDA data, where outcomes are aggregated to the geographic district level, we instead rely on the data *underlying* SEDA. These data come from the federal EdFacts data system and provide more fine-grained test scores at the school-year-subject-grade level.

The EdFacts data do not, however, include scale scores and instead provide only the percent of students reaching particular achievement thresholds (e.g., proficiency). This has two disadvantages: (a) changes in proficiency over time only provide information about that portion of the distribution that is near the threshold; and (b) each state sets its own proficiency standard. Nevertheless, these are the only data that allow us to isolate district-level TPS achievement, so we re-estimate equation (2), changing only the dependent variable to TPS proficiency and geographic district proficiency, respectively. We are interested in the difference in estimated treatment effects between these two dependent variables as a gauge of the share of the total effect (when geographic district proficiency is the dependent variable) that is driven by the competitive effect (when TPS proficiency is the dependent variable).

The results are shown in Appendix Table A12. Consistent with our earlier results, we see positive effects of charter market share on geographic district outcomes. However, the point estimates are small and very imprecise when TPS proficiency is the dependent variable, which may suggest that the total effect operates through the participant effect. We reiterate the limitations of proficiency measures and view this as only suggestive.

6.2. Interaction Between Participant and Other Effects

All of the above analysis assumes that the participant and other effects are independent of one another. However, there are reasons to expect that TPS are more responsive to those charter schools of higher quality, i.e., they have larger participant effects. TPSs might feel a greater need to respond when charter schools are especially effective. To test this, we again use the proficiency data, but add an interaction term:

$$TPS\_Proficient_{isgt} = \alpha + \beta_2 ChartShare_{isgt} + \beta_2 \left(ChartShare_{isgt} \cdot ChartAchGr_i\right) + X_{ist}\gamma + \mu_i + \lambda_{sgt} + \varepsilon_{isgt} \tag{5}$$

where $ChartAchGr_i$ is the (time-invariant) achievement growth of all charter schools included in $ChartShare_{isgt}$ in geographic district i. We measure $ChartAchGr_i$ using another measure in SEDA that captures the achievement growth of individual cohorts of students as they pass through school. On average, SEDA-style cohort growth measures are useful proxies for longitudinal growth measures similar to school value-added (Reardon et al. 2019).[30]

The results of this analysis are provided in Appendix Table A13. We do see some evidence of a positive coefficient on the interaction $ChartShare_{isgt} \cdot ChartAchGr_i$, though this is only precise in math and when we allow a one-year lag. (Since this part of the analysis is only concerned with competitive effects, which should take more time to develop, we see the one-year lag as strongly preferred.) As a test of the validity of this analysis, we also estimated versions in which the dependent variable is $GeoDistrict\_Proficient_{isgt}$. These effects should be larger because $ChartAchGr_i$ is a measure of the charter participant effect. This is what

[29] The above effect heterogeneity results are based on the one-year lag, which is where we saw positive effects in columns 4–6 in Table 2. We also re-estimated using contemporaneous outcomes (similar to columns 1–3 in Table 2) and dropping the always-treated districts (similar to Table 3). These robustness checks yield qualitatively similar results.

[30] In our own analysis of the Louisiana SEDA measures, we find a correlation of at least +0.6 between SEDA growth and conventional school value-added measures based on student-level data.

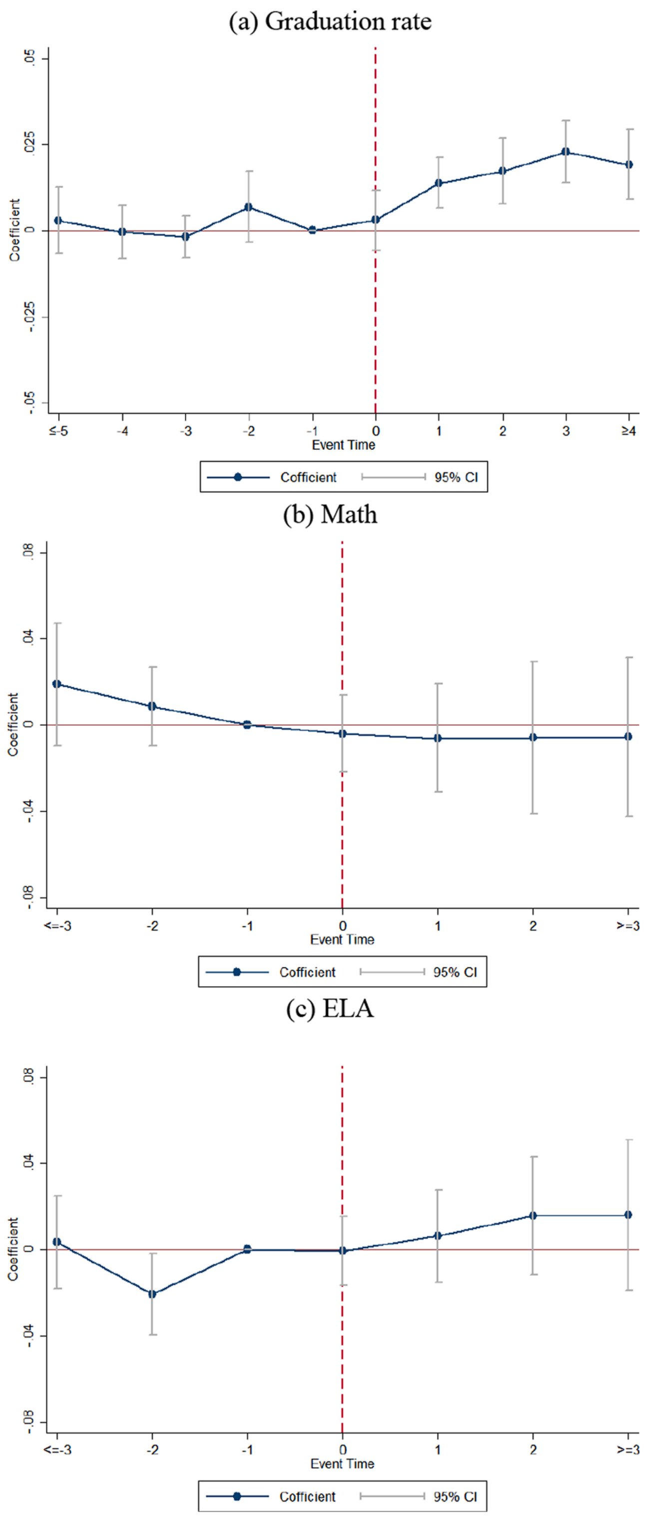

Fig. 3. Event study (IW) – Never-treated compared to districts with positive shocks in charter share. Notes: These event studies compare never-charter districts to districts with greater than two percentage point spikes in charter enrollment within a three-year period. See text for discussion.

we see in the table, which reinforces that the competitive effects increase in the participant effects. We still interpret these with some caution, however, because of the reliance on proficiency-based outcomes.

6.3. Charter Entry Effects on TPS Closures

We can further drill down on the different components of the competitive effects, particularly the charter entry effects on TPS closure. Harris and Martinez-Pabon (2022) provide a comprehensive data set on school closures where a closed TPS occurs when the school building is no longer used as a school. We therefore switch the dependent variable in the above GDD analyses from test scores and graduation rates to closure.

We find increases in TPS closure when charters enter (Table 7). Taking the estimate in column (1), a 10 percent increase in charter enrollment share increases the school closure rate by about 0.0023 percentage points (a 17 percent increase over the baseline rate). In two additional analyses, shown in Panels B and C, we also carry out the placebo version, examining "effects" of elementary charter school entry on high school closure (and vice versa). The closure effects are much smaller in the placebo estimates, though sometimes still precisely estimated, which leads us to interpret these results with some caution.[31]

Whether these induced TPS closures improve district performance depends on whether the closed schools are low-performing relative to the entering charter schools and the other TPSs to which students in closed schools may be forced to move (Bross, Harris, and Liu 2023). Our data suggest that the closed TPSs had lower performance than non-closed TPSs. The SEDA achievement growth measure of math and ELA test scores (discussed in Section 6.2) is 0.005 s.d. for non-closed TPSs in treated districts, but −0.006 s.d. for closed TPSs in treated districts.[32] This suggests that charter entry may lead lower-performing schools to close, which increases overall district student outcomes through mechanisms other than the usual participant and competitive effects.

6.4. Effects of Charter Entry on Private School Enrollment

So far, the above analyses only look at the charter effects on student outcomes in public school systems, however, charter schools may also have some impacts on enrollment patterns in private schools, which also matter for economywide human capital and therefore for social welfare. Also, if some private school students end up in publicly funded schools as a result of the charter entry as some studies have suggested (Toma et al. 2006, Buddin 2012), then our estimated effects might be biased upwards.[33]

We focus on the districts that have at least one private school at baseline.[34] The sample period is 1996–2018 and note that the private school data are only available biannually. We find no evidence that charter entry reduces the share of private schools' enrollment (Table 8).

[31] Unlike the effects on student outcomes, the effects on school closures arise immediately; the lag effects are smaller and less precise. This may suggest that charter entry is part of district plans, especially in cases where districts are charter authorizers. They might, for example, close one TPS because they are aware of an incoming charter school entrant. (In some cases, the charter entrant might even be taking over the building being vacated by a TPS, but this would not show up as a closure in these data.).

[32] The practical effects of these differences might be smaller because the low-performing teachers in closed TPS may end up moving to the remaining TPS and charter schools. However, we note: (a) prior research suggests that teachers in closed/takeover schools are more likely than other teachers to leave the profession entirely and/or move into non-teaching positions (Lincove et al., 2019); and (b) schools and school leaders vary in their ability to convert individual teacher skill into student outcomes (Branch et al. 2012), so, even if teachers were all re-sorted to other schools, average school performance would still improve to some degree.

[33] Chakrabarti and Roy (2016), in contrast, found no causal evidence that charter schools in Michigan led to declines in private school enrollment. They used a fixed-effects model as well as an instrumental variables strategy that exploits exogenous variation from Michigan charter law.

[34] A total of 6,449 school districts (roughly one-half of all geographic districts) have one private school during the sample period.

Table 5
Effects of charter share on cohort size and FRL students.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Graduation sample				Test score sample			
	Same year share		Lag one year share		Same year share		Same cohort last year	
Panel A log of cohort size								
Charter share	−0.035	0.002	−0.055	0.008	−0.034	−0.046	−0.022	−0.040
	[0.115]	[0.090]	[0.151]	[0.115]	[0.081]	[0.077]	[0.061]	[0.057]
R-squared	0.996	0.997	0.996	0.997	0.994	0.994	0.994	0.994
Observations	156,436	156,436	156,436	156,436	525,502	525,502	525,502	525,502
N(district)	10,629	10,629	10,629	10,629	11,399	11,399	11,399	11,399
Panel B FRL Students								
Charter share	0.021	−0.016	0.044	0.024	0.002	0.012	0.007	0.014
	[0.052]	[0.049]	[0.037]	[0.037]	[0.018]	[0.015]	[0.016]	[0.013]
R-squared	0.925	0.931	0.925	0.931	0.960	0.961	0.960	0.961
Observations	156,436	156,436	156,436	156,436	525,777	525,777	525,777	525,777
N(district)	10,629	10,629	10,629	10,629	11,399	11,399	11,399	11,399
District FE, State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student controls	No	Yes	No	Yes	No	Yes	No	Yes
District controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: These results come from estimating equations (1) and (2) with cohort size and FRL as dependent variables, as a test for endogenous charter entry. See other notes in Table 2.

Table 6
Effect heterogeneity.

Group	(1) MA	(2) Black	(3) Hispanic	(4) FRL	(5) Policy	(6) Baseline Ach.	(7) No Excuses
Panel A: Graduation rate							
Charter share* group	0.104*	0.238***	0.125	0.407***	0.030	−0.368***	0.539
	[0.057]	[0.085]	[0.179]	[0.142]	[0.454]	[0.105]	[0.407]
Charter share	0.057	0.071**	0.131***	0.012	0.127	0.249***	0.146***
	[0.044]	[0.035]	[0.042]	[0.044]	[0.288]	[0.050]	[0.034]
R-squared	0.854	0.854	0.854	0.854	0.854	0.855	0.854
Observations	156,436	156,436	156,436	156,436	156,436	153,168	156,436
N(district)	10,629	10,629	10,629	10,629	10,629	10,261	10,629
Panel B: Math							
Charter share* group	0.121	−0.490*	0.737***	−0.037	2.131	−0.123	0.683
	[0.101]	[0.258]	[0.172]	[0.159]	[1.566]	[0.171]	[0.538]
Charter share	−0.081	0.112**	−0.103*	0.037	−1.308	0.07	0.011
	[0.085]	[0.054]	[0.054]	[0.075]	[0.988]	[0.094]	[0.049]
R-squared	0.862	0.862	0.862	0.862	0.862	0.863	0.862
Observations	525,502	525,502	525,502	525,502	525,502	508,052	525,502
N(district)	11,399	11,399	11,399	11,399	11,399	10,648	11,399
Panel C: ELA							
Charter share* group	0.193**	−0.501*	1.025***	0.011	0.576	−0.156	0.425
	[0.098]	[0.291]	[0.190]	[0.180]	[1.055]	[0.187]	[0.595]
Charter share	−0.075	0.182***	−0.084	0.084	−0.271	0.148	0.082*
	[0.078]	[0.057]	[0.056]	[0.069]	[0.664]	[0.106]	[0.050]
R-squared	0.891	0.891	0.891	0.891	0.891	0.892	0.891
Observations	525,502	525,502	525,502	525,502	525,502	508,052	525,502
N(district)	11,399	11,399	11,399	11,399	11,399	10,648	11,399
District FE, State-by-year (-by-grade) FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table shows estimates of heterogeneous effects of the charter share on student outcome by groups with a one-year lag, analogous to column (6) in Table 2. *MA* is an indicator for metropolitan areas. *Black, Hispanic, and FRL* are continuous, ranging from 0 to 1, and time-constant (using values from the first year in the sample). *Baseline Ach.* refers to the outcome level at baseline. For consistency with the other interactions in this table, we also recoded so that baseline achievement is also in the range of 0–1 (using percentiles of the baseline score distribution and dividing by 100). *Policy* indicates higher state charter policy ratings from the National Alliance of Public Charter Schools (also rescaled 0–1). *No Excuses* is the district enrollment share of charter school enrollment in schools whose names include “KIPP” or “Success Academy.” See other notes in Table 2.

Table 7
Effects of charter share on TPS closure.

	(1)	(2)	(3)	(4)	(5)	(6)
	Same year share			Lag one year share		
Panel A: district charter share on all TPS closure (main effect)						
Charter share	0.023*	0.022*	0.018	−0.005	−0.004	−0.010
	[0.012]	[0.013]	[0.012]	[0.007]	[0.008]	[0.010]
R-squared	0.134	0.136	0.138	0.133	0.136	0.138
Observations	279,652	279,652	279,652	279,652	279,652	279,652
N(district)	12,435	12,435	12,435	12,435	12,435	12,435
Panel B: high school charter share on elementary TPS closure (placebo)						
Charter share	0.012*	0.012	0.009	−0.005	−0.005	−0.009
	[0.007]	[0.007]	[0.007]	[0.005]	[0.006]	[0.007]
R-squared	0.084	0.086	0.088	0.084	0.086	0.088
Observations	279,652	279,652	279,652	279,652	279,652	279,652
N(district)	12,435	12,435	12,435	12,435	12,435	12,435
Panel C: elementary charter share on high school TPS closure (placebo)						
Charter share	0.007**	0.007**	0.005*	0.000	0.000	−0.002
	[0.003]	[0.003]	[0.003]	[0.002]	[0.002]	[0.002]
R-squared	0.100	0.101	0.102	0.100	0.101	0.101
Observations	279,652	279,652	279,652	279,652	279,652	279,652
N(district)	12,435	12,435	12,435	12,435	12,435	12,435
District FE, State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Student controls	No	Yes	Yes	No	Yes	Yes
District controls	No	No	Yes	No	No	Yes

Notes: This table presents estimated effects of the charter share on the TPS closure rate. The other panels show the placebo results for this analysis. The sample period is from the year 1995 to 2018. Regressions are weighted by the total number of public schools. See other notes in Table 2.

Table 8
Effects of charter share on private school enrollment share.

	(1)	(2)	(3)	(4)	(5)	(6)
	Same year share			Lag one year share		
Panel A: district charter share on private school enrollment share						
Charter share	0.032	0.020	0.014	0.049	0.037	0.034
	[0.036]	[0.031]	[0.033]	[0.045]	[0.039]	[0.043]
R-squared	0.898	0.902	0.902	0.899	0.902	0.903
Observations	65,255	65,255	65,255	65,255	65,255	65,255
N(district)	6,449	6,449	6,449	6,449	6,449	6,449
Panel B: high school charter share on elementary private school enrollment (placebo)						
Charter share	0.119	0.083	0.067	0.142	0.108	0.097
	[0.103]	[0.085]	[0.087]	[0.127]	[0.112]	[0.118]
R-squared	0.886	0.889	0.89	0.887	0.889	0.89
Observations	65,255	65,255	65,255	65,255	65,255	65,255
N(district)	6,449	6,449	6,449	6,449	6,449	6,449
Panel C: elementary charter share on high school private school enrollment (placebo)						
Charter share	0.067	0.054	0.050	0.067	0.056	0.053
	[0.054]	[0.045]	[0.047]	[0.059]	[0.053]	[0.057]
R-squared	0.875	0.877	0.877	0.876	0.877	0.877
Observations	65,255	65,255	65,255	65,255	65,255	65,255
N(district)	6,449	6,449	6,449	6,449	6,449	6,449
District FE, State-by-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Student controls	No	Yes	Yes	No	Yes	Yes
District controls	No	No	Yes	No	No	Yes

Notes: This table presents estimated effects of charter share on the share of private school enrollment. The sample period is limited to districts with at least one private school, for the years 1996 to 2018. The data are only available biannually, which explains why the number of observations drops more than the number of districts. Regressions are weighted by the district total enrollment. See other notes in Table 2.

A further implication is that our earlier estimates on district-level graduation rate and test scores are not biased by shifts in enrollments to or from private schools.

7. Conclusion

This study makes many new contributions to the research on charter schools and market-based school reforms generally. First, we provide some of the first analysis of the total combined effect of charter school entry on student outcomes at a national level. Using the GDD strategy combined with various methods for addressing endogeneity, and with a sample including almost all charter schools in the country, our analysis suggests that increasing charter market share by 10 percentage points increases elementary and middle school test scores by 0.01–0.02 s.d. nationally. Our placebo estimates, analyses of changing student demographics, parallel trends tests, re-analysis at the commuting zone level, and comparisons with prior, well-identified studies suggest that these are causal effects and are not noticeably influenced by endogenous charter location and timing.

While these point estimates may seem small, we note that these are market-level effects that apply to all students, not just sub-groups to whom a particular policy is designed (e.g., early childhood education or class size reduction in early grades) and not just the small share of students who happen to take up programs designed for broad access (e.g., after-school tutoring). Our estimated effects reflect increases in average outcomes encompassing *every student* attending any publicly funded school in the more than one thousand districts nationally that have at least one charter school.

Second, this is the first study to our knowledge to outline the mechanisms of charter entry effects—participant, near competitive, far competitive, closure, and matching effects—And to show what mechanisms are, and are not incorporated, in different types of research designs. We also find that: (a) the market-level effect is driven mostly by participant effect; (b) the competitive effects increase in participant effect; and (c) a portion of the competitive effects may be due to charter schools replacing low-performing TPS—the closure effect. Understanding why charter effects emerge is as important as estimating the effects themselves.

Third, we contribute to a small but growing literature to study effects on student outcomes other than test scores (Imberman 2011, Booker et al. 2011, Furgeson et al. 2012, Angrist et al., 2013a, Angrist et al., 2013b, Dobbie and Fryer 2013, Wong et al. 2014, Booker et al. 2014). We find that increasing charter market share by 10 percentage points increases high school graduation rate by about two percentage points. This is important given how strongly predictive high school graduation is for long-term life outcomes.

Fourth, with such a large sample, we are able to provide new evidence on charter effect heterogeneity. In addition to reinforcing past evidence regarding the concentration of effects in urban/metropolitan areas and in middle schools, we find that the market-level effects are larger in larger, urban districts, where baseline achievement is low, and in no-excuses charter schools.

Fifth, the rising number of districts—Detroit, New Orleans and Washington, DC—that are majority-charter also raises the question: Is there some limit regarding how much charter market share is good for students? We find evidence that market-level effects are larger for the first charter entrant than subsequent ones. While the results in New Orleans have been especially positive (Harris and Larsen 2023), with near 100 percent charter market share, this may be an outlier, or perhaps the effects would have been just as large in New Orleans with a smaller charter market share.

Charter schooling has been arguably the most influential school reform efforts of the past several decades. Informing these ongoing debates, our analysis helps better understand the effects on multiple student outcomes, at national level, as well as when, how, and for whom charter school effects operate.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

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Appendix A. Supplementary material

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jpubeco.2023.105015.

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