Working Paper 2022–3
SPRING 2022

Enrollment Flexibility and Charter School Impacts:

The Effect of Backfill on Students in Massachusetts Charter Middle Schools

Marcus A. Winters, PhD, Associate Professor, Boston University

Cheonghum Park, PhD, Associate Research Fellow, Korea Institute of Public Finance

BU

Boston University Wheelock College of Education & Human Development
Wheelock Educational Policy Center

WEPC
WHEELOCK EDUCATIONAL POLICY CENTER

Enrollment Flexibility and Charter School Impacts:

The Effect of Backfill on Students in Massachusetts Charter Middle Schools

Marcus A. Winters
Associate Professor
Boston University
marcusw@bu.edu

Cheonghum Park
Associate Research Fellow
Korea Institute of Public Finance
chpark@kipf.re.kr

Abstract

Unlike traditional public schools, charter schools in most states can avoid disruptions caused by incorporating new students into classrooms by choosing not to "backfill" enrollment vacancies. We leverage variation in exposure to a Massachusetts law that occurred across-sectors, within-schools, and over time to measure the causal effect of backfill on charter school impacts. The requirement substantially increased backfill within Boston charter middle schools but not traditional public schools or charter schools outside of Boston, which were already backfilling at high rates. However, the backfill requirement had no significant effect on the performance of incumbent students within Boston charter middle schools.

The data can be obtained by filling a request directly with the Massachusetts Department of Elementary and Secondary Education (MADESE). The authors are willing to assist. Neither author received financial support or has a conflicting interest. We received helpful comments from Josh Goodman, Kevin Lang, Daniele Passerman, Andrew Bacher-Hicks, Olivia Chi, Nathan Jones, Jesse Bruhn, Sarah Cohodes, Mark Chin, Lauren Sartain, and participants at the Wheelock Educational Policy Center PREREQ seminar, Boston University applied microeconomics seminar, and the annual conferences for the Association for Education Finance and Policy and the

Association for Public Policy Analysis and Management. For providing the necessary data and for their very helpful guidance, we'd also thank the staff of the Massachusetts Department of Elementary and Secondary Education, especially Matt Deninger, Elana McDermott, Brenton Stewart, and Alison Bagg. All remaining errors are our own.

Keywords: School Choice, Charter Schools, Student Mobility

1 Introduction

Incorporating new students into existing cohorts is a persistent challenge for urban public schools. New students can disrupt the flow of instruction by diverting the teacher's time and attention towards familiarizing them with classroom procedures (Lash and Kirkpatrick 1990) or bringing them up to speed on material (Lazear 2001). Unlike traditional public school districts, which must enroll eligible students regardless of their grade level or time of year they arrive, most charter schools can avoid disruptions caused by incorporating new students by choosing not to "backfill" vacated seats with new students from their waiting list during the year or at the start of the following school year. Only four of the 45 states with charter school laws have policies regulating backfill in charter schools (NAPCS 2019). The ability to limit new student entry is perhaps the most prominent example of a charter-specific feature that could meaningfully contribute to charter school effectiveness.

We leverage Massachusetts's adoption of a statewide requirement for charter schools to backfill vacancies in some, but not all grades in order to provide the first causal estimates for the effect of incorporating new students on the performance of incumbent charter school students. Specifically, we employ a difference-in-difference-in-difference (DDD) design that exploits variation in exposure to the backfill requirement occurring over time (first difference), across school sectors (second difference), and between impacted and not-impacted grades within each school (third difference). We begin by showing that the requirement altered enrollment patterns within charter middle schools in Boston, which were backfilling enrollments at very low rates prior to the change in the law, but not within traditional public schools or charter middle schools operating outside of Boston. Nonetheless, we then find the backfill requirement did not significantly impact the performance of students previously enrolled in Boston charter middle schools.

A limited body of prior research finds that incorporating new students, especially during the school year, reduces the achievement of incumbent students within traditional public schools (Hanushek et al. 2004; Whitesell et al. 2016; Raudenbush et al. 2011). But there is considerable reason to expect that these results may not directly apply to the charter sector. On one hand, the most common explanation for why some charter schools choose not to backfill despite their reliance on per-pupil state funding[1] is concern that incorporating new students would be especially disruptive within charter schools that have invested heavily in developing a consistent culture and norms with their existing students over time (Hill and Maas 2015). On the other hand, perhaps such investment in building a strong school culture and the skills of staff who developed it would prove to be powerful tools for addressing the challenges associated with incorporating new students, and thus mitigate the influence of such disruptions on the school's productivity.

In addition to providing new insights on the impacts of student mobility on school productivity, we contribute to the growing literature evaluating the nature of charter school impacts. Research documenting that charter schools can have large positive effects on student outcomes (Abdulkadiroğlu et al. 2011, 2016, 2017; Angrist et al. 2010, 2012, 2013, 2016; Cohodes et al. 2021; Dobbie and Fryer Jr 2011, 2013, 2015; Hoxby and Murarka 2009; Setren 2019; Winters and Shanks 2021; Curto and Fryer Jr 2014; Tuttle et al. 2013; Walters 2018) but that charter school effectiveness varies substantially both across and within localities (CREDO 2015; Angrist et al. 2013; Dobbie and Fryer Jr 2013) has spawned considerable interest in identifying the source of charter school impacts. Charter school effects appear not to be related to resource differences such as class size (Dobbie and Fryer Jr 2013) or to their more flexible teacher employment practices (Bruhn et al. 2020). Urban charter schools that emphasize principles related to student behavior, extended school days, teacher feedback, and intense focus on traditional math and reading skills – elements commonly referred to in the literature (though no longer as often in policy circles) as the "No Excuses" approach – are especially effective (Angrist et al. 2013; Winters and Shanks 2021; Cohodes et al. 2021; Angrist et al. 2012, 2010; Dobbie and Fryer Jr 2011; Tuttle et al. 2013; Walters 2018). Such results are encouraging because with sufficient political will, many of these practices

[1] Some states allocate per-pupil funding according to October enrollments and thus charter schools do not benefit financially from replacing students mid-year. However, this is not the case in Massachusetts, which considers enrollment throughout the year when determining school funding.

could be applied within struggling traditional public schools. Indeed, recent attempts to implement such practices within traditional public schools have produced similarly positive results (Abdulkadiroğlu et al. 2016; Fryer Jr 2014). But the ability to limit new student entry is an example of a charter school feature that could not feasibly be transferred into traditional public school districts. The extent to which such a non-transferable factor is responsible for charter school effectiveness would raise questions about the validity of cross-sector school quality comparisons.

Boston is an especially interesting setting in which to investigate the effect of enrollment flexibility on charter school impacts because its charter schools are both highly effective as measured by their influence on student test scores (Abdulkadiroğlu et al. 2011; Angrist et al. 2010, 2012; Cohodes et al. 2021; Setren 2019) and they uniformly apply the "No Excuses" approach that invests heavily in the development of a strong school culture. Indeed, though Angrist et al. (2013) argued that urban charter schools' application of the "No Excuses" approach primarily explains the difference in the impact of the state's urban and non-urban charter schools, we show that during the time period they analyzed an unexamined difference was that Boston charter schools backfilled enrollments at very low rates compared to charter schools in other parts of the state.[2] We'd argue that if backfilling vacancies reduced charter school effectiveness we'd especially expect to see evi-

[2]Cohodes et al. (2021) found similarly positive effects from enrolling in a Boston charter school during years prior to and following the reform that included the backfill requirement. Though it is at least somewhat reassuring that our findings are consistent with that prior work, it is important to note that such concurrence in the results was not assured because the papers make meaningfully different comparisons in order to address different research questions. Cohodes et al. (2021) use admission lotteries to measure the effect of charter school enrollment for new students who enter a school within its initial grade. In contrast, we are concerned with whether there were within-school changes in the performance of students who were already enrolled in charter schools within grades that were required to backfill relative to students in the same school but within a grade that was not

dence of it in a high-performing charter sector with a large share of "No Excuses" schools such as Boston's.

Though there is no reliable estimate for the number of vacant seats in charter schools nationally, there is ample reason to believe that decisions not to backfill substantially reduce charter school enrollments in many localities. An education reporter's informal polling of various charter school operators nationwide found substantial variation in their reported backfilling practices (Barnum 2019). A study of enrollment patterns in Washington, DC found that about 8% of the city's traditional public school students enrolled in their school mid-year, compared to less than one percent in the city's charter schools (Campbell and Quirk 2019). A study of student enrollment in Oakland found a similar disparity in mid-year entry across the sectors (ERS 2017). Among the 53 New York City charter schools analyzed by Domanico (2015), 22 replaced 100% or more of students that exited mid-year, while 20 replaced less than 70% of their students who vacated seats. An official in Philadelphia estimated that there are about 1,400 empty seats in the city's charter schools each year due to the choice not to backfill (Windle 2017). A potential implication of our results from Boston is that by backfilling their vacancies charter schools in these and likely other localities could meaningfully increase student access without harming the achievement of their current students.

The remainder of the paper proceeds as follows. Section 2 provides background on Massachusetts's statewide backfill requirement and describes the data. Section 3 describes the analyses, and we report results in Section 4. Finally, we provide a brief summary and discuss the implications of our findings in Section 5.

---

required to backfill. That is, whereas Cohodes et al. (2021) convincingly analyze the existence of charter school impacts, our analysis sheds new light on the role of backfill specifically in driving charter school impacts.

2 Background and Data

2.1 Change to Massachusetts Charter School Law

Charter schools enroll about 5% of the public school students in Massachusetts. Of the state's 78 charter schools currently operating, 58 are located in urban areas; 21 of those urban schools are in Boston. The state limits charter school enrollments by capping the amount of tuition that may be transferred from a district to any charter school. Relative to traditional public schools, charter schools serve disproportionately higher percentages of Black and Hispanic students and smaller percentages of students with disabilities or who are not proficient in English.

Massachusetts adopted several significant changes to the rules governing charter schools when Governor Duval Patrick signed into law An Act Relative to the Achievement Gap in January of 2010. Arguably the most consequential, and certainly the most widely noted, aspect of the reform was to raise the state's cap on the amount of tuition certain districts could potentially have to pay to support enrollment in charter schools, which led to a substantial increase in charter school enrollments. The revised law also imposed new levels of oversight on the charter school sector, such as requiring charter schools to adopt recruitment and retention plans for disadvantaged students. Additionally, it limited their accumulation of surplus assets.

Most importantly for our purposes, the new law also amended the state's requirements related to backfilling seats vacated by students who leave a charter school cohort. Under the original law, charter schools had discretion over whether and when they filled such vacancies. The revised law required charter schools to backfill vacancies that occur within the bottom half of grades within the school, excluding Grades 10 or above. For grades subjected to the requirement, charter schools must attempt to fill seats vacated prior to February 15 by the end of the school year with students on its waiting list from the previous winter's enrollment process. Seats that the school is unable to fill or that became vacant after February 15 are to be replaced within the cohort at the beginning of the following school year. Charter schools are not permitted to meet the backfill requirement by admitting a student into a different grade than where the seat was vacated.

Students who are retained in a grade do not count toward fulfilling the backfill requirement. The backfill requirement does not apply to students who were offered admission to but did not enroll in the charter school.

The Massachusetts Department of Elementary and Secondary Education (DESE) tracks compliance with the statute as part of its role as the authorizing entity. The department considers failure to comply with the backfill requirement as part of a school's record of performance at renewal and when determining whether to grant future expansion or replication requests by the school.

In October 2010, DESE circulated a memo informing charter schools of their obligation to follow backfilling requirements. The policies were updated and enforced to the best of the department's ability during the 2011-2012 school year. For our analyses, we thus treat the 2011-2012 school year as the first under the requirement.

2.2 Data

2.2.1 Data Sources and Sample Construction

We use data on the universe of public school students enrolled in Grades 5 through 8 at schools that begin with Grade 5 or 6, from the academic years 2007-2008 to 2013-2014 (four pre-requirement years and three post-requirement years). Student-level enrollment status and demographic variables come from the state's Student Information Management System (SIMS). We restrict the sample to include only school-grade combinations that were observed in 2011 or before. This restriction does not meaningfully impact our primary estimates for the effect of the backfill requirement because, as described in Section 3, our model includes school-grade fixed effects. Thus, school-grades observed only in the post-requirement period would not directly contribute to the estimate for the treatment effect.

SIMS collects student data three times every academic year: in October, in March, and at the end of the year. We identify a student to be a mid-year backfill in a school if we observe them for the first time in the school in March or at the end of the year. Summer backfill students are identified by enrollment status in October, but we exclude entry grades and cohorts that expanded in size by more than 50%. We identify a student to have left the cohort mid-year if we observe them for the last time in the school in October or in March. Summer exits are identified when students not in the exit grade leave after the end of the year. The number of summer openings in a school is the sum of summer exit students and the previous mid-year exit students not filled within the year.

Student test scores in math and English language arts (ELA) come from the Massachusetts Comprehensive Assessment System (MCAS). We standardize the raw MCAS scores within year and grade to have zero mean and one standard deviation. Then, we merge the test scores with SIMS using the State Assigned Student Identifier (SASID), year, and grade to construct student-year level data. Our primary analyses pool math and ELA test score observations into a single regression so that the level of observation is the student-year-subject.

2.2.2 Descriptive Statistics

Table 1 reports the characteristics of students relevant for our analysis. We separate characteristics by school sector, whether or not the observation is within an impacted grade within the school, and whether the observation occurs prior to or following the backfill requirement.

Within each sector, there are no meaningful differences in student characteristics either across grade type or time period. Compared to traditional public schools, charter schools both within and outside of Boston enroll student populations that are more likely to be eligible for free or reduced priced lunch, and more likely to be Black. The large apparent difference in the lagged math and ELA scores across impacted and not-impacted grades within Boston charter middle schools is consistent with the positive test score impacts for these schools, since by definition the not-impacted grades are the higher offered grades within the school.

[TABLE 1 ABOUT HERE]

Table 1 also describes different measures of backfill by grade and school type before and after the change in the law. For both traditional public and non-Boston charter schools, there does not appear to be a meaningful difference in the number of students who entered mid-year or over the summer, or the proportion of students within a cohort that entered in those ways during the periods before and after the change in the law. We also do not observe a meaningful difference on these measures of backfill before and after the change in the law within not-impacted grades in Boston charter middle schools. However, consistent with our expectations, there does appear to be a clear increase in backfill, especially mid-year, within impacted grades of Boston charter middle schools following the change in the law.

3 Empirical Strategy

In order to estimate the causal effect of the backfill requirement on the performance of incumbent charter school students, we apply a DDD design that leverages variation in exposure to Massachusetts's backfill requirement that occurs over time, across sectors, and between impacted and not-impacted grades within a school. Our primary analysis pools math and ELA test score observations into a single regression and accounts for across-student differences with a student-subject fixed effect. Formally, we estimate:

$$\begin{aligned} y_{imgst} = &\alpha_{im} + \tau_{mgs} + \omega_{mst} + \beta X_{igst} + \sum_{j=0}^{1} \iota Begin_{it}^{j} \times [\delta_1 Post_t + \delta_2 (Post_t \times ImpactGrade_{gs}) \\ &+ \kappa_1 (Post_t \times BCH_s) + \kappa_{ddd} (Post_t \times ImpactGrade_{gs} \times BCH_s) \\ &+ \chi_1 (Post_t \times TPS_s) + \chi_{ddd} (Post_t \times ImpactGrade_{gs} \times TPSs)] + \mu_{imgst} \end{aligned} \quad (1)$$

where $Begin$ equals one if it is the student's first year in their school and it is not the first grade offered within the school, and equals zero otherwise; $Post$ indicates that the observation occurs after the backfill requirement was implemented; $ImpactGrade$ indicates that the observation is in a grade to which the backfill requirement applied; BCH and TPS are, respectively, indicators for whether the student is enrolled in a Boston charter school or a traditional public school; μ is a stochastic term; and all other variables are as previously defined. By interacting $Begin$ with the rest of the terms, we estimate the effect of the backfill requirement on the achievement of incumbent students separately from the effect on students who have just entered the school. We are exclusively interested in the incumbent students, and thus we report and focus the interpretation of results only on the estimates when $Begin$ equals zero.[3] We cluster standard errors by both school-grade-subject and student.

The coefficients on variables related to school sectors are estimated relative to the omitted group of students in non-Boston charter schools. We treat non-Boston charter schools rather than traditional public schools as the primary comparison group because in addition to the requirement not affecting their enrollments, we would expect non-Boston charter schools to have been similarly impacted by other components of the new law that were not directly relevant for traditional public schools. For instance, the new law also brought additional oversight over all charter schools that could have impacted their performance. Thus, we are primarily concerned with evaluating whether κ_{ddd} differs from zero. The trajectory of student performance in what would be impacted or not-impacted grades within traditional public schools, which were not directly affected by the reform, serves as a secondary or placebo group against which we can compare Boston charter schools. We employ an an F-test for inference comparing differences between κ_{ddd} and χ_{ddd}.

Note that our estimation strategy focuses on comparing changes in student performance across grades within schools and thus allows for changes in the overall effectiveness of Boston charter schools relative to traditional public and non-Boston charter schools over time. The identifying assumption required to interpret κ_{ddd} as the causal effect of the backfill requirement on the test scores of incumbent students within Boston charter schools is this: Without the change in the law, the difference in average performance of incumbent students in impacted and not-impacted grades within Boston charter schools before and after the backfill requirement would have been the

[3]Results are similar if interaction with $Begin$ is removed.

same as the change in the difference in average performance of incumbent students in impacted and not-impacted grades within non-Boston charter schools. The model's focus on differences across grades within schools over time is especially important because introduction of the backfill requirement occurred concurrently with other policy changes that could impact average charter school performance. For our purposes, factors such as the ability for some charter schools to expand and increased oversight for the charter sector threaten our identification strategy only to the extent that they impacted Boston charter schools differently in impacted and not-impacted grades.

Inclusion of the school-year fixed effect in the primary model accounts for changes in the relative performance of schools in the respective sectors over time. However, a limitation of employing a school-year fixed-effect is that it does not allow us to separately capture the conditional change in student performance within impacted and not-impacted grades, which could provide important context for interpreting the results. For example, it would be concerning if we identified an effect from the backfill requirement but it was driven by an increase in student performance in impacted grades within non-Boston charter schools and traditional public schools rather than a change in the relationship between impacted and not-impacted grades in Boston charter schools. In order to consider this context, we also estimate and report the results from models that remove the school-year fixed effects.

3.1 Effect of Backfill Requirement on Backfill Within Schools

Our justification for focusing on Boston charter schools is that the backfill requirement led to a substantial change in their enrollment practices, but it did not impact enrollments in non-Boston charter schools, which were already backfilling at high rates, or in traditional public schools, which were not subjected to the requirement. We test the appropriateness of this assumption by estimating Equation 1 using different measures of backfill as the dependent variable in order to produce DDD estimates for the effect of the requirement across school types. For these analyses, we aggregate the data to the school-grade-year level. When the dependent variable is a measure of student entry over the summer, we remove from the sample observations of students within a school's initially offered grade, since all students in that grade who were not retained are first entering the school that fall.

[TABLE 2 ABOUT HERE]

The results reported in Table 2 confirm that the backfill requirement impacted enrollments in Boston charter schools more substantially than other charter schools and traditional public schools. Conditional on the fixed effects and other covariates, we estimate that the requirement led to an increase of about 7.455 students entering Boston charter schools in impacted grades, either mid-year or over the summer, relative to the comparison group of non-Boston charter schools. We find evidence that the requirement increased both mid-year and summer backfill, though the summer backfill effect is estimated imprecisely due to the smaller number of school-grade combinations observed and is not statistically significant. The results reported in the final three columns evaluate the effect of the policy on the proportion of students within the school-grade cohort that year who entered as a backfill student. We estimate that the requirement increased the proportion of students within a school-grade cohort in an impacted graded within a Boston charter middle school by about 6.7 percentage points relative to non-Boston charter schools. When the dependent variable is either a measure of mid-year backfill or overall backfill, the estimate for Boston charter schools also differs significantly from the estimate for traditional public schools.

Figure A.1 in the Online Appendix provides additional insight and helps to clarify the intuition of the estimates reported in Table 2 by illustrating the estimated effect of the backfill requirement in impacted and not-impacted grades for each school type within models that exclude the school-year fixed effect. Consistent with our expectations, the results show that the relative increase in backfill within Boston charter middle schools was driven by substantial increases within impacted grades, rather than changes within not-impacted grades within Boston charter schools or by changes in backfill across the grade types within non-Boston charter schools or traditional public schools.

4 Results

Table 3 reports the results from regressions estimating the effect of the backfill requirement on student test scores. The omitted school sector is non-Boston charter schools. Columns (4) through (6) report the results from our preferred specification, which incorporates student, school-grade, and school-year fixed effects, in ELA, math, and both subjects combined, respectively. In each case the coefficient on the DDD term for Boston charter middle schools is near zero. Combining the subjects into a single regression reduces the standard errors somewhat, but the estimates are fairly imprecise. In each case the coefficient on the DDD term for traditional public schools also does not differ significantly from the comparison group of non-Boston charter schools or from Boston charter schools.

Columns (1) through (3) of Table 3 report the results from models that exclude the school-year fixed effect, which allow us to further investigate the factors driving the main results. Figure A.2 in the Online Appendix further aids this interpretation by illustrating the estimated changes in student test scores over time by school- and grade-type resulting from these models. The positive DDD estimate for Boston charter middle schools in math found in these models is driven by an unanticipated decline in performance in not-impacted grades following the requirement. For other school and grade types in math, and for all such types on the ELA exam, we observe a small but statistically insignificant increase in average student performance following adoption of the new law.

[TABLE 3 ABOUT HERE]

Recall that the primary assumption necessary to interpret the DDD estimate as the causal effect of the backfill requirement is that in its absence the trend in the difference in student performance within impacted and not-impacted grades in Boston charter schools would have been the same as the trend in the difference in student performance within impacted and not-impacted grades within traditional public schools and non-Boston charter schools. As a robustness check,

Table A.1 reports the results from models that relax this common trends assumption by incorporating interactions between school sector, impact grade, and a linear time trend. Allowing for differential trends in student performance across grade-type and school-type does not alter the pattern of results. Relative to the respective analyses that do not control for trends, the DDD estimates for Boston charter middle schools within models that do not incorporate a school-year fixed effect move slightly toward zero, and the estimates for Boston charter middle schools that do incorporate a school-year fixed effect are very similar in ELA and only slightly more positive in math and for the combined MCAS test. As is common within models that control for differential trends, the coefficients in these models are estimated much less precisely.

5 Conclusion

The change in Massachusetts law significantly and substantially altered enrollment patterns for impacted grades within Boston charter middle schools. Nonetheless, the introduction of these new students through increased backfill did not significantly impact the performance of incumbent charter school students as measured by standardized test scores. Though some of our key results are estimated imprecisely, our estimates for the effect of the backfill requirement are all near zero, and we can confidently rule out the potential for large negative effects.

Why increased backfill would have no significant negative impact the performance of incumbent charter school students is a question worthy of future research. Indeed, our findings contrast not only with the concerns raised by charter school operators (Hill and Maas 2015) but also with prior evidence on the effect of incorporating new students within traditional public schools (Hanushek et al. 2004; Whitesell et al. 2016). One potential explanation for our finding is that rather than exacerbating the disruptive aspects of incorporating new students, the consistent culture and intense focus on student behavior and activities within the "No Excuses"-type charter schools found in Boston make them especially adept at addressing the challenges created by incorporating new students. It would be interesting to discover whether the effects of backfill would be different in areas with a larger variety of charter school types. Similarly, research investigating whether the impacts of incorporating new students within traditional public schools depends on aspects of the school's environment would also be informative.

Within the community of charter school supporters, we expect that whether one uses our findings to support calls to require charter schools to backfill their enrollments depends largely on their view of the importance of charter school autonomy. Nonetheless, perhaps more importantly, we believe that our results should be of interest to the seemingly large number of charter school leaders nationwide that leave seats vacant despite a financial incentive, and some would argue moral imperative to maximize their enrollments primarily because they are concerned that incorporating new students would harm their current students. Boston's experience provides strong evidence that high-performing charter schools can continue to substantially benefit their current students even as they incorporate new students into vacant seats.

Our results also provide new insights into the nature of charter school impacts that should inform both academic and policy conversations. In particular, our findings contrast with claims that comparisons between the effectiveness of charter and traditional public schools are inherently unfair because charter schools' ability to not fill vacant seats contributes substantially to their impacts, while traditional public schools must accept all eligible students regardless of when they arrive. Our finding that introducing required backfill had no significant effect on the performance of incumbent charter school students despite substantially increasing backfill suggests that this type of enrollment flexibility does not meaningfully contribute to charter school effectiveness. That our findings come from Boston's charter middle schools, which have been found by several prior studies to have large test score benefits relative to traditional public schools (Abdulkadiroğlu et al. 2011; Angrist et al. 2010, 2012, 2013; Cohodes et al. 2021) and uniformly apply the "No Excuses" approach that prioritizes developing school norms, is especially notable for policymakers and charter school leaders.

References

Abdulkadiroğlu, A., J. D. Angrist, S. M. Dynarski, T. J. Kane, and P. A. Pathak (2011). Accountability and flexibility in public schools: Evidence from Boston's charters and pilots. *The Quarterly Journal of Economics 126*(2), 699–748.

Abdulkadiroğlu, A., J. D. Angrist, P. D. Hull, and P. A. Pathak (2016). Charters without lotteries: Testing takeovers in New Orleans and Boston. *American Economic Review 106*(7), 1878–1920.

Abdulkadiroğlu, A., J. D. Angrist, Y. Narita, and P. A. Pathak (2017). Research design meets market design: Using centralized assignment for impact evaluation. *Econometrica 85*(5), 1373–1432.

Angrist, J. D., S. R. Cohodes, S. M. Dynarski, P. A. Pathak, and C. R. Walters (2016). Stand and deliver: Effects of Boston's charter high schools on college preparation, entry, and choice. *Journal of Labor Economics 34*(2), 275–318.

Angrist, J. D., S. M. Dynarski, T. J. Kane, P. A. Pathak, and C. R. Walters (2010). Inputs and impacts in charter schools: KIPP Lynn. *American Economic Review 100*(2), 239–43.

Angrist, J. D., S. M. Dynarski, T. J. Kane, P. A. Pathak, and C. R. Walters (2012). Who benefits from KIPP? *Journal of Policy Analysis and Management 31*(4), 837–860.

Angrist, J. D., P. A. Pathak, and C. R. Walters (2013). Explaining charter school effectiveness. *American Economic Journal: Applied Economics 5*(4), 1–27.

Barnum, M. (2019). Charter schools in some cities enroll few students mid-year. here's why that matters. *Chalkbeat*.

Bruhn, J. M., S. A. Imberman, and M. A. Winters (2020). Regulatory arbitrage in teacher hiring and retention: Evidence from Massachusetts charter schools. NBER Working Paper 27607, National Bureau of Economic Research.

Campbell, N. and A. Quirk (2019). Student mobility, backfill, and charter schools. *Center for American Progress*.

Cohodes, S. R., E. M. Setren, and C. R. Walters (2021). Can successful schools replicate? Scaling up Boston's charter school sector. *American Economic Journal: Economic Policy 13*(1), 138–67.

CREDO (2015). Urban charter school study report on 41 regions. Technical report, Center for Research on Educational Outcomes.

Curto, V. E. and R. G. Fryer Jr (2014). The potential of urban boarding schools for the poor: Evidence from SEED. *Journal of Labor Economics 32*(1), 65–93.

Dobbie, W. and R. G. Fryer Jr (2011). Are high-quality schools enough to increase achievement among the poor? Evidence from the Harlem Children's Zone. *American Economic Journal: Applied Economics 3*(3), 158–87.

Dobbie, W. and R. G. Fryer Jr (2013). Getting beneath the veil of effective schools: Evidence from New York City. *American Economic Journal: Applied Economics 5*(4), 28–60.

Dobbie, W. and R. G. Fryer Jr (2015). The medium-term impacts of high-achieving charter schools. *Journal of Political Economy 123*(5), 985–1037.

Domanico, R. (2015). School indicators for New York City charter schools 2013-2014 school year. Technical report, New York City Independent Budget Office.

ERS (2017). Informing equity: Student need, spending, and resource use in Oakland's public schools. *Education Resource Strategies*.

Fryer Jr, R. G. (2014). Injecting charter school best practices into traditional public schools: Evidence from field experiments. *The Quarterly Journal of Economics 129*(3), 1355–1407.

Hanushek, E. A., J. F. Kain, and S. G. Rivkin (2004). Disruption versus Tiebout improvement: The costs and benefits of switching schools. *Journal of Public Economics 88*(9-10), 1721–1746.

Hill, P. and T. Maas (2015). Backfill in charter high schools: Practices to learn from and questions to be answered. *Seattle, WA: Center on Reinventing Public Education*.

Hoxby, C. M. and S. Murarka (2009). Charter schools in New York City: Who enrolls and how they affect their students' achievement. NBER Working Paper 14852, National Bureau of Economic Research.

Lash, A. A. and S. L. Kirkpatrick (1990). A classroom perspective on student mobility. *The Elementary School Journal 91*(2), 177–191.

Lazear, E. P. (2001). Educational production. *The Quarterly Journal of Economics 116*(3), 777–803.

NAPCS (2019). Backfilling in public charter schools. *National Alliance for Public Charter Schools*.

Raudenbush, S. W., M. Jean, and M. Art (2011). Year-by-year and cumulative impacts of attending a high-mobility elementary school on children's mathematics achievement in Chicago, 1995 to 2005.

Setren, E. (2019). Targeted vs. general education investments: Evidence from special education and english language learners in boston charter schools. *Journal of Human Resources*, 0219–10040R2.

Tuttle, C. C., B. Gill, P. Gleason, V. Knechtel, I. Nichols-Barrer, and A. Resch (2013). KIPP middle schools: Impacts on achievement and other outcomes. Final report. *Mathematica Policy Research, Inc.*.

Walters, C. R. (2018). The demand for effective charter schools. *Journal of Political Economy 126*(6), 2179–2223.

Whitesell, E. R., L. Stiefel, and A. E. Schwartz (2016). Unexpected arrivals: The spillover effects of mid-year entry on stable student achievement in New York City. *Educational Evaluation and Policy Analysis 38*(4), 692–713.

Windle, G. (2017). Replacing students who leave: The debate over 'backfilling'. *Chalkbeat*.

Winters, M. A. and C. Shanks (2021). The effect of attending a charter school in Newark, New Jersey on student test scores. *Eductional Evaluation and Policy Analysis*.

Tables

Table (1) Descriptive Statistics by School Type and Time Period

	TPS				Other charter				Boston charter			
	Not impact		Impact		Not impact		Impact		Not impact		Impact	
	Pre	Post	Pre	Post	Pre	Post	Pre	Post	Pre	Post	Pre	Post
ELL	0.0327	0.0388	0.0414	0.0474	0.00899	0.0155	0.00813	0.0283	0.0252	0.0245	0.0112	0.0426
	(0.178)	(0.193)	(0.199)	(0.212)	(0.0944)	(0.124)	(0.0898)	(0.166)	(0.157)	(0.155)	(0.105)	(0.202)
Free lunch	0.243	0.281	0.244	0.286	0.339	0.270	0.295	0.329	0.482	0.537	0.447	0.480
	(0.429)	(0.449)	(0.430)	(0.452)	(0.473)	(0.444)	(0.456)	(0.470)	(0.500)	(0.499)	(0.497)	(0.500)
Reduced lunch	0.0563	0.0502	0.0539	0.0484	0.0749	0.0590	0.0623	0.0672	0.170	0.145	0.167	0.132
	(0.230)	(0.218)	(0.226)	(0.215)	(0.263)	(0.236)	(0.242)	(0.250)	(0.376)	(0.353)	(0.373)	(0.339)
Female	0.490	0.491	0.490	0.491	0.484	0.496	0.485	0.509	0.529	0.546	0.505	0.496
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.499)	(0.498)	(0.500)	(0.500)
Any disability	0.164	0.166	0.171	0.171	0.176	0.153	0.132	0.130	0.122	0.148	0.147	0.175
	(0.371)	(0.372)	(0.377)	(0.376)	(0.381)	(0.360)	(0.338)	(0.337)	(0.327)	(0.355)	(0.354)	(0.380)
White	0.877	0.865	0.866	0.852	0.668	0.685	0.705	0.745	0.385	0.416	0.416	0.459
	(0.328)	(0.342)	(0.340)	(0.355)	(0.471)	(0.464)	(0.456)	(0.436)	(0.487)	(0.493)	(0.493)	(0.498)
Black	0.0814	0.0900	0.0936	0.100	0.331	0.236	0.231	0.199	0.404	0.485	0.549	0.506
	(0.273)	(0.286)	(0.291)	(0.300)	(0.471)	(0.424)	(0.422)	(0.399)	(0.491)	(0.500)	(0.498)	(0.500)
Asian	0.0546	0.0637	0.0557	0.0688	0.0433	0.0895	0.0759	0.0756	0.0172	0.0230	0.0171	0.0225
	(0.227)	(0.244)	(0.229)	(0.253)	(0.203)	(0.286)	(0.265)	(0.264)	(0.130)	(0.150)	(0.130)	(0.148)
Lag ELA	0.0410	0.0499	0.0462	0.0458	0.0954	0.0951	-0.00261	0.0332	0.572	0.260	-0.152	-0.0622
	(0.965)	(0.967)	(0.971)	(0.976)	(0.886)	(0.827)	(0.935)	(0.916)	(0.661)	(0.833)	(0.897)	(0.906)
Lag math	0.0402	0.0481	0.0450	0.0408	0.191	0.0820	0.0495	0.0722	0.656	0.513	0.0347	0.143
	(0.963)	(0.965)	(0.976)	(0.974)	(0.896)	(0.898)	(0.979)	(0.921)	(0.588)	(0.709)	(0.863)	(0.847)
# Mid yr	5.337	4.879	5.667	5.267	0.586	0.767	2.849	1.788	0.000	0.0435	0.828	3.122
	(6.648)	(5.453)	(8.484)	(5.937)	(1.044)	(2.223)	(3.049)	(2.330)	(0.000)	(0.209)	(1.518)	(3.358)
# Summer	9.581	9.186	9.669	9.453	3.345	2.000	10.97	7.928	0.846	1.435	5.659	8.394
	(10.26)	(9.630)	(12.37)	(10.84)	(5.482)	(3.913)	(9.721)	(6.645)	(2.111)	(4.176)	(6.533)	(7.894)
# Fill	14.90	14.17	10.90	10.30	3.931	2.767	9.643	7.048	0.815	1.478	4.453	8.776
	(14.64)	(14.11)	(14.95)	(12.44)	(6.043)	(4.470)	(9.801)	(7.172)	(2.076)	(4.368)	(6.092)	(8.474)
% Cohort mid	0.0709	0.0635	0.145	0.123	0.0120	0.0097	0.0450	0.0227	0.000	0.0002	0.0099	0.03
	(0.402)	(0.239)	(0.650)	(0.497)	(0.0321)	(0.0273)	(0.0650)	(0.0292)	(0.000)	(0.00132)	(0.0178)	(0.0447)
% Cohort summer	0.0673	0.0598	0.0691	0.0613	0.0601	0.0260	0.133	0.0988	0.0157	0.0166	0.0709	0.0978
	(0.0851)	(0.0645)	(0.101)	(0.0802)	(0.110)	(0.0494)	(0.111)	(0.0895)	(0.0395)	(0.0384)	(0.0800)	(0.0657)
% Cohort fill	0.138	0.123	0.184	0.156	0.0721	0.0357	0.128	0.0882	0.0151	0.0169	0.0553	0.104
	(0.416)	(0.254)	(0.654)	(0.500)	(0.128)	(0.0558)	(0.126)	(0.0972)	(0.0388)	(0.0392)	(0.0739)	(0.0812)
Student-years	186694	171070	380447	227689	2335	1999	9838	8100	872	1306	4624	3241
School-grades	1,006		1,744		30		108		22		56	

Note: Table reports mean and standard deviation (in parentheses) for student characteristics and test scores separated by school sector, grade type, and time period. ”Pre”/”post” refer to observations in years before/after the reform. Data come from estimation sample for the analysis for the impact of the backfill requirement on student performance in ELA.

Table (2) Effect of Backfill Requirement on School Backfill

	(1) # Mid yr	(2) # Summer	(3) # Fill	(4) % Cohort mid	(5) % Cohort summer	(6) % Cohort fill
Policy x impact grade	-1.250**	-2.411	-2.883*	-0.016*	-0.015	-0.023
	(0.525)	(1.936)	(1.565)	(0.010)	(0.019)	(0.021)
Policy x impact grade x TPS	1.276**	2.540	3.085*	0.008	0.017	0.015
	(0.541)	(1.961)	(1.592)	(0.015)	(0.019)	(0.024)
Policy x impact grade x Boston ch	3.386***	4.163	7.455***	0.041***	0.041	0.067**
	(0.692)	(2.569)	(2.260)	(0.011)	(0.031)	(0.027)
School-grade FE	X	X	X	X	X	X
School-year FE	X	X	X	X	X	X
F-test comparing coefficients (p-value)						
DDD Boston ch. = DDD TPS	0.000	0.440	0.016	0.004	0.389	0.012
N	7156	5003	7156	7034	4936	7034
R^2	0.927	0.912	0.915	0.852	0.734	0.850

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Dependent variable listed at the top of each column. # Mid yr, # Summer, and # Fill are the number of students who entered the school-grade mid-year, at the beginning of the year, or either, respectively. % Cohort mid yr, % Cohort summer, and % Cohort fill is the percentage of students in the school-grade in the respective year who entered the cohort during the school year, at the beginning of the year, or either, respectively. All regressions include school-grade and school-year fixed effects. Sample includes only school-grade combinations that were first observed in 2011 or before. Omitted school sector is non-Boston charter schools. Sample for regressions reported in Columns (2) and (5) exclude the first grade level offered in the school. Bottom of table reports p-values resulting from F-tests comparing the null hypothesis that the triple interaction for Boston Charter Schools differs from the triple interaction for traditional public schools. Standard errors clustered at school-grade level reported in parentheses.

Table (3) Effect of Backfill Requirement on Incumbent Student Test Scores

	(1) ELA	(2) Math	(3) MCAS	(4) ELA	(5) Math	(6) MCAS
Post	0.033	0.003	0.019			
	(0.031)	(0.024)	(0.021)			
Post x impact grade	0.027	0.010	0.016	-0.002	0.024	0.009
	(0.032)	(0.030)	(0.024)	(0.038)	(0.039)	(0.029)
Post x TPS	-0.024	0.008	-0.010			
	(0.031)	(0.025)	(0.022)			
Post x impact x TPS	-0.028	-0.014	-0.020	-0.003	-0.020	-0.010
	(0.032)	(0.030)	(0.024)	(0.039)	(0.040)	(0.029)
Post x Boston	0.011	-0.085*	-0.037			
	(0.049)	(0.044)	(0.037)			
Post x impact x Bos ch	-0.038	0.082	0.023	-0.004	0.004	0.002
	(0.057)	(0.055)	(0.043)	(0.059)	(0.066)	(0.047)
Student FE	X	X		X	X	
School-grade FE	X	X		X	X	
School-year FE				X	X	
Student-subject FE			X			X
School-grade-subject FE			X			X
School-year-subject FE						X
F-test comparing coefficients (p-value)						
DDD Boston ch. = DDD TPS	0.840	0.038	0.239	0.977	0.656	0.757
N	992856	993896	1993949	992782	993840	1993813
R^2	0.892	0.922	0.907	0.894	0.924	0.909

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Dependent variables listed at the top of each column are standardized by grade-subject-year to have mean zero and standard deviation of one. Sample includes only school-grade combinations that were first observed in 2011 or before. “Post” refers to an indicator that equals one if the observation occurs in 2012 or later. “Impact” refers to an indicator for if the observation is within a grade that would have been subjected to the backfill requirement if the school was a charter school in the post-law change period. Omitted school sector is non-Boston charter schools. Regressions (3) and (6) pool observations from ELA and math. Regressions also control for indicators for whether the student is an English learner, eligible for free lunch, or eligible for reduced priced lunch. All regressions also include interactions between the reported variables and an indicator that equals one if the student is in their first year within the school and the grade is not the first offered within the school. Bottom of table reports p-values resulting from F-tests comparing the null hypothesis that the triple interaction for Boston Charter Schools differs from the respective estimate for traditional public schools. Standard errors reported in parentheses are clustered at school-grade and student for regressions (1), (2), (4), and (5) and by school-grade-subject and student for regressions (3), and (6).

Appendices

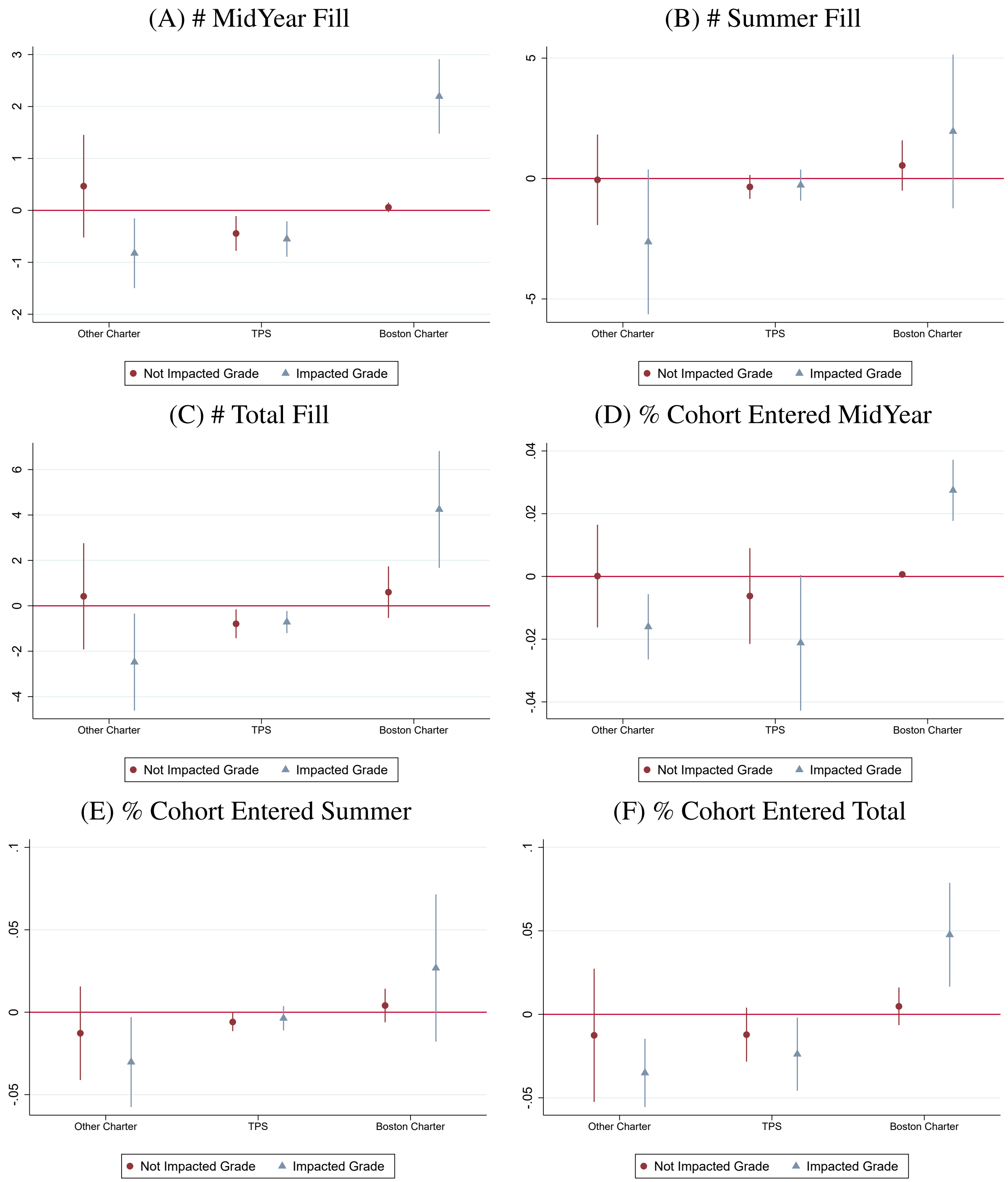
Table (A.1) Effect of Backfill Requirement on Student Test Scores, Including Linear Time Trend by School and Grade Type

	(1) ELA	(2) Math	(3) MCAS	(4) ELA	(5) Math	(6) MCAS
Post	0.125***	0.034	0.079**			
	(0.047)	(0.053)	(0.039)			
Post x impact grade	-0.106*	-0.062	-0.084*	-0.072	0.025	-0.026
	(0.056)	(0.060)	(0.045)	(0.058)	(0.068)	(0.047)
Post x TPS	-0.136***	-0.041	-0.089**			
	(0.048)	(0.053)	(0.039)			
Post x impact x TPS	0.130**	0.078	0.104**	0.090	-0.013	0.040
	(0.057)	(0.061)	(0.045)	(0.059)	(0.068)	(0.048)
Post x Boston	-0.056	-0.089	-0.068			
	(0.081)	(0.068)	(0.059)			
Post x impact x Bos ch	-0.022	0.029	-0.001	0.000	0.031	0.014
	(0.097)	(0.086)	(0.070)	(0.090)	(0.100)	(0.071)
Student FE	X	X		X	X	
School-grade FE	X	X		X	X	
School-year FE				X	X	
Student-subject FE			X			X
School-grade-subject FE			X			X
School-year-subject FE						X
Type x impact x trend	X	X	X	X	X	X
F-test comparing coefficients (p-value)						
DDD Boston ch. = DDD TPS	0.0567	0.4213	0.5453	0.1962	0.0544	0.6206
N	992856	993896	1993949	992782	993840	1993813
R^2	0.892	0.922	0.907	0.894	0.924	0.909

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Dependent variables listed at the top of each column are standardized by grade-subject-year to have mean zero and standard deviation of one. Sample includes only school-grade combinations that were first observed in 2011 or before. “Post” refers to an indicator that equals one if the observation occurs in 2012 or later. “Impact” refers to an indicator for if the observation is within a grade that would have been subjected to the backfill requirement if the school was a charter school in the post-law change period. Omitted school sector is non-Boston charter schools. Regressions (3) and (6) pool observations from ELA and math. Regressions also control for indicators for whether the student is an English learner, eligible for free lunch, or eligible for reduced priced lunch. All regressions also include interactions between the reported variables and an indicator that equals one if the student is in their first year within the school and the grade is not the first offered within the school. Bottom of table reports p-values resulting from F-tests comparing the null hypothesis that the triple interaction for Boston Charter Schools differs from the respective estimate for traditional public schools. Standard errors reported in parentheses are clustered at school-grade and student for regressions (1), (2), (4), and (5) and by school-grade-subject and student for regressions (3), and (6).

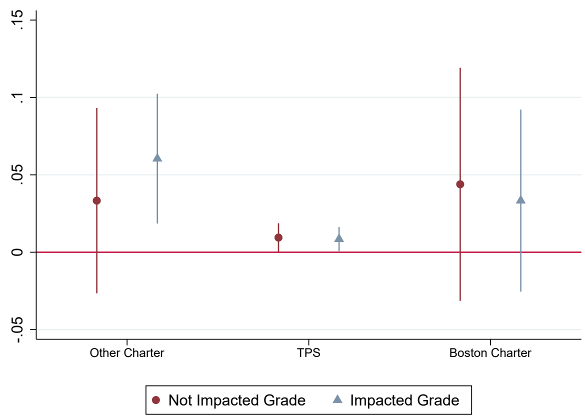
Figure (A.1) Effect of Backfill Requirement on Backfill



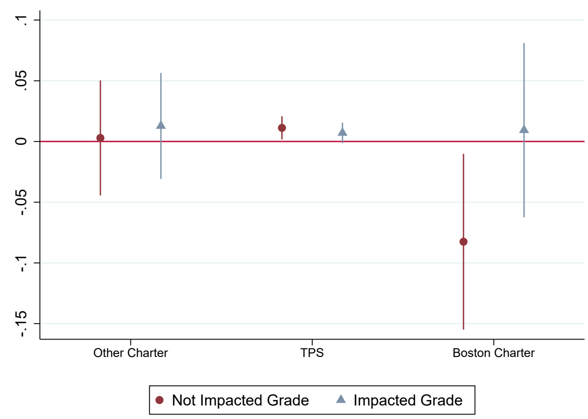
Note: Figure illustrates the effect of the backfill requirement on different measures of backfill for by school- and grade-type implied by the estimates from models similar to those reported in Table 2, but that exclude the school-year fixed-effect. The Dot/Triangle indicates the estimate and line illustrates the 95% confidence interval.

Figure (A.2) Effect of Backfill Requirement on Incumbent Student Test Scores

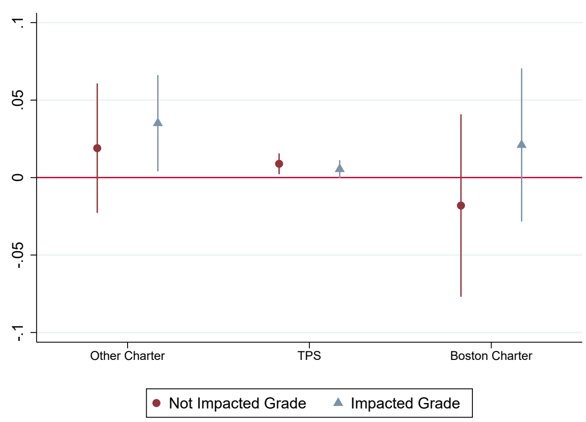
(A) ELA



(B) Math



(C) MCAS



Note: Figure illustrates the effect of the backfill requirement on incumbent student test scores by school- and grade-type implied by the estimates from models reported in Table 3 that exclude the school-year fixed-effect. The Dot/Triangle indicates the estimate and line illustrates the 95% confidence interval.

Suggested Citation: Winters, M.A., & Park, C. (2022). *Enrollment Flexibility and Charter School Impacts: The Effect of Backfill on Students in Massachusetts Charter Middle Schools.* (Working Paper 2022-3). Wheelock Educational Policy Center. Available at wheelockpolicycenter.org.

OUR MISSION

The Wheelock Educational Policy Center (WEPC) conducts and disseminates rigorous, policy-relevant education research in partnership with local, state, and federal policymakers and stakeholders to improve pk-20 educational opportunities and holistic outcomes for underserved students.

www.wheelockpolicycenter.org
wheelockpolicy@bu.edu

Boston University Wheelock College of Education & Human Development
Wheelock Educational Policy Center

WEPC
WHEELOCK EDUCATIONAL POLICY CENTER