Fiscal and Education Spillovers from Charter School Expansion

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ABSTRACT

Do charter schools drain resources and high-achieving peers from noncharter schools? We provide new evidence on the fiscal and educational consequences of charter expansion for noncharter students in Massachusetts, which temporarily compensates districts losing students to charter schools. Exploiting a 2011 reform that lifted caps on charter schools for underperforming districts, we use complementary synthetic control (SC) and differences-in-differences instrumental variables (IV-DiD) estimators. Our results suggest charter expansion leaves districts' overall per pupil revenue and expenditure unchanged, but induces districts to shift expenditure from capital investment and support services to instruction and salaries and ultimately increases noncharter students' achievement in math.

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I. Introduction

Since its origins in the early 1990s, the charter school sector has grown to reach, as of 2019, more than 7,000 schools serving about 3.2 million children (National Alliance for Public Charter Schools 2019).[1] These publicly funded schools were conceived as a means to spur innovation in and competition for surrounding traditional public schools, which could lead to higher attainment for students. Yet concerns have emerged that charter schools could also harm noncharter students by draining resources or high-achieving peers from traditional public schools.

To address concerns about the financial impact of charter schools, several states have adopted refund schemes that temporarily compensate school districts for lost revenues due to charter expansion. These schemes have been adopted by Massachusetts, Illinois, New Hampshire, New York, and Pennsylvania, among others. In Massachusetts, when charter payments increase for a school district over the prior year, the state reimburses that district for 100 percent of the increased cost in the first year, and 25 percent for each of the subsequent five years. The state also fully reimburses the "facilities aid" that sending districts pay to charter schools to help pay for school buildings. In states without such a refund, some evidence suggests that charter schools have a negative effect on districts' financial health (Arsen and Ni 2012; Bifulco and Reback 2014; Cook 2018).[2] However, relatively little evidence exists on the fiscal and educational consequences of charter schools in contexts with a temporary refund scheme.

We use new empirical methods to dig into how charter expansion affects traditional school district finances and their students' achievement when a temporary refund scheme is available. Charter school proponents claim that increased competition from charter schools might induce districts to strategically reallocate spending in an

institutions concerning intellectual property. Carrie Conaway, the Chief Strategy and Research Officer of the Massachusetts Department of Elementary and Secondary Education, had the right to review this paper for whether it maintained student confidentiality before submission to a journal, and she exercised that right. The authors further confirm that any aspect of the work covered in this manuscript that has involved either experimental animals or human patients has been conducted with the ethical approval of all relevant bodies and that such approvals are acknowledged within the manuscript; the authors obtained Institutional Review Board (IRB) approval with "exempt" research status for this project by MIT and NBER. This paper uses data from the Annual Survey of School System Finances collected annually by the Census Bureau (U.S. Census Bureau 2017). These data are publicly available. This paper also uses Proprietary data from the Massachusetts Department of Elementary and Secondary Education. The replication of results is thus conditional on obtaining access to these proprietary data, for which there is a standard procedure. The data used in this article and all replication files are available online at https://doi.org/10.17636/10188880.

1. See Epple, Romano, and Zimmer (2015) for an excellent literature review on charter schools.
2. Bifulco and Reback (2014) provide case studies of traditional public schools' financial adaptations to declining enrollment in Albany and Buffalo, NY. Arsen and Ni (2012) find that higher charter school enrollment levels in Michigan school districts are strongly associated with declining fund balances.

attempt to attract and retain students (Hoxby 2003).[3] Yet charter expansion also drains resources from school districts—in most states, when a student switches from a traditional public school to a charter school, the funding follows the student. This might force school districts facing fixed costs to cut per pupil spending on variable costs (such as instructional staff), hampering their ability to respond to competition. Temporary refund schemes could overcome this problem by insulating districts from the short-term financial shock identified by previous studies and allowing them to adjust to competition over time.

The first objective of this work is to assess the fiscal impact of charter school expansion on traditional public school districts under a temporary refund scheme. We look at how charter expansion impacts (i) districts' average per pupil revenue, (ii) their average per pupil expenditures on noncharter students, and (iii) the share of these expenditures devoted to fixed and variable costs, as well as to support services, instruction, and salaries. A second focal point in the debate on charter expansion is its spillover effects on noncharter student achievement (Cordes 2018; Imberman 2011; Booker et al. 2008). If charter schools drain resources and high-achieving peers from noncharter schools, this could harm student achievement in traditional public schools. The second objective of this study is therefore to revisit the question of charter expansion's impact on student achievement, using a novel identification strategy.

The main challenge in isolating the causal impact of charter expansion is the nonrandom initial location and expansion of charter schools (Slungaard Mumma 2022; Glomm, Harris, and Lo 2005; Bifulco and Buerger 2015).[4] To deal with this endogeneity, we exploit a Massachusetts reform that led to charter sector expansion. In 2011, the state raised the limit on district funding allocable to charter schools from 9 percent to 18 percent, but only for districts with student performance in the lowest 10 percent of districts statewide. Several such districts took advantage of the reform to expand their charter sector (hereafter termed *expanding districts*). In the four years following the reform, the share of students attending charter schools jumped from 7 percent to 12 percent in expanding districts, while it remained relatively constant at about 3 percent in all other nonexpanding districts.

However, the 2011 reform induced far from all eligible districts to expand their charter sector. As a result, statistical power is too low for us to use eligibility to expand as an instrument for charter expansion. Instead, we account for selection into expansion by building a synthetic control (SC) for the expanding districts (Abadie and Gardeazabal 2003;

3. A large literature has looked at the competition effect of both private and public schools; see Epple, Romano, and Urquiola (2017) for a recent literature review, as well as Card, Dooley, and Payne (2010) and Clark (2009), but charter schools differ from both. Unlike private schools, charters do not charge tuition and are publicly funded. In that respect, charter schools compete with traditional public schools for funds, while private schools do not. The competition effect of charter schools also differs from that of traditional public schools. First, charter schools' opening and expansion are regulated differently. As a result, charter schools have considerably expanded in the last 30 years, while the number of traditional public schools has remained relatively constant. Second, in Massachusetts, traditional public schools receive temporary financial aid when students switch to a charter school but not when students switch to a different public school or district.

4. Charter schools tend to locate in districts where the population is diverse, per pupil expenditure is high, teacher costs are low, and public school achievement is relatively low (Glomm Harris, and Lo 2005; Bifulco and Buerger 2015).

Abadie, Diamond, and Hainmueller 2010, 2015). We use the SC to estimate the first-stage effect of the reform—that is, its effect on the share of students in charter schools—and the reduced-form effect of the reform—that is, its effect on financial or educational outcomes. Because the SC method has never been used to jointly estimate first-stage and reduced-form effects, we adapt the standard SC approach to ensure that expanding and synthetic control districts have similar pre-reform trends in *both* expenditures and charter share.

Our identification relies on the assumption that the synthetic control does not contain districts that were eligible to expand but decided not to expand for unobserved financial- or achievement-related reasons. To ensure that this assumption holds, we limit the donor pool of the SC to (i) marginally ineligible districts (those with student performance in the tenth to 20th percentile of districts statewide) plus (ii) eligible districts that marginally failed to open a new charter school. These are districts that received applications for new charter schools, but whose applications were rejected for reasons unrelated to their fiscal situation or attainment.

However, with the SC approach alone there are not established methods to generate IV estimates or conduct standard sampling-based statistical inference. Therefore, we complement the SC approach with a differences-in-differences instrumental variables (IV-DiD) method using the control group identified by the SC. Specifically, we instrument districts' charter share with the interaction between the post-reform years and whether or not a district expands its charter sector. To build the control group of nonexpanding districts, we reuse the group of nonexpanding districts identified by the synthetic control method as the control (but without their weights). Combining SC and IV-DiD gives us the opportunity to check that both methods yield similar results despite being based on different pre-trend assumptions. Furthermore, while statistical inference in the SC approach has to rely on placebo inference, we use standard large-sample inference methods with the IV-DiD.

Our results reveal that the expansion of the charter sector after the 2011 reform triggered larger payments to charter schools but also additional aid from the state. Our estimates suggest that a five point increase in charter share would increase districts' transfers to charter schools by \$450 per student and the temporary refund by \$222 per student. The temporary aid appears to offset about 49 percent of districts' transfers to charter schools. We also find that the charter expansion left districts' total per pupil revenue (after payments to charter schools and refunds) largely unchanged. This result is striking given that districts lose money to charter schools on a per pupil basis, which implies that all else equal, revenue per pupil should be constant before adding the refund, and a refund of any size should increase per pupil revenue. In fact, we find that all else is not equal—districts see a reduction in other sources of state revenue that offsets the temporary refund.

Next, adding districts' expenditures to the picture, we find that per pupil expenditures did not increase more than per pupil revenue in traditional public schools. This finding suggests that charter expansion did not have a negative fiscal impact on district finances. However, we document significant changes in the way districts allocate their resources when they face larger charter competition. Moving from 7 percent to 12 percent of students attending charter schools (which is the average jump for expanding districts four years after the reform) increases per pupil expenditures on instruction by 11.4 percent and on salaries by 6.2 percent, but reduces per pupil expenditures on capital by 87.2 percent and spending on support services by 7.4 percent. This contrasts with evidence of very limited reallocation effects in states without temporary reimbursement aid (Arsen and Ni 2012; Cook 2018). While increasing instruction-related expenditures might be a source of student progress, cutting spending on support services might be detrimental to students' attainment, in particular when cuts target pupil support, such as guidance, health, and psychological support.

Our second objective, given these results on spending, is to investigate the impact of charter expansion on student achievement. This raises additional challenges in terms of identification. Ample evidence shows that charter students differ from noncharter students and that charter students become an increasingly selected sample as charter schools expand (Epple, Romano, and Zimmer 2015; Baude et al. 2014; Cohodes, Setren, and Walters 2021). Restricting achievement outcomes to noncharter students, as we do for expenditure outcomes, would bias estimation. Unlike most previous studies, we therefore estimate a model of achievement for all students, in which charter expansion has a spillover effect on students' potential outcomes from not attending a charter school. We show that this spillover effect can be estimated as the effect of the charter enrollment share on average achievement of all students after controlling for the causal effect of individual charter enrollment.[5] We identify the individual-level causal effect by using charter admissions lottery offers as an instrumental variable for individual charter enrollment (Angrist et al. 2010, 2016; Dobbie and Fryer 2011, 2016; Abdulkadiroğlu et al. 2011; Angrist Pathak, and Walters 2013; Cohodes, Setren, and Walters 2021).

Our results show that charter sector expansion has a positive impact on the achievement of students who stay in traditional public schools. Moving from 10 percent to 15 percent of students attending charter schools increases the achievement of noncharter students by 0.11 standard deviations in math and by 0.06 in English language arts (ELA). These results confirm, to some extent, the findings of previous studies showing charter expansion has an impact on student achievement (Bettinger 2005; Imberman 2011; Zimmer and Buddin 2009; Davis 2013; Sass 2006; Winters 2012; Slungaard Mumma 2022; Gilraine, Petronijevic, and Singleton 2021). Looking at potential mechanisms, we find that class size reduction and teacher transition from traditional public schools to charter schools are unlikely to explain our results.

The Massachusetts reimbursement funding scheme is only temporary, so a natural question is what happens after the end of the refund period. To investigate this question, we use an event-study analysis of all charter school openings in Massachusetts that persistently raised the charter share before 2011. We find that the effects of charter expansion on both expenditures and achievement tends to vanish in the longer run, and in particular after the refund ends. The distinction between the short-term and long-term effects partially reconciles our results with the negative fiscal spillover effect identified by previous studies in states that do not have temporary refund

5. Specifically, we estimate the spillover effect as ρ in the following model: $Y_{idt} = \alpha + \rho C_{dt} + \hat{\beta}_{idt} c_{idt} + \gamma' X_{idt} + u_{dt} + \epsilon_{idt}$, where Y_{idt} is achievement for student i in district d at year t, c_{idt} is individual charter attendance, and C_{dt} is the share of students attending charter schools in district d and year t. This is a version of the standard "peer-effects" specification including both an individual-level treatment and its group average, which is often used to measure spillovers (Acemoglu and Angrist 2000).

schemes (Arsen and Ni 2012; Bifulco and Reback 2014; Ladd and Singleton 2020; Cook 2018). Overall, the small long-run effect suggests that the refund cushions the short-run financial blow to districts in order to give them time to adjust to lower student numbers.

This work contributes to the literature in a number of ways. First, our paper adds to the literature on the fiscal spillovers of charter schools (Arsen and Ni 2012; Ladd and Singleton 2020; Cook 2018), by considering an environment in which traditional public schools benefit from a temporary refund from the state when they lose students to charter schools. The effect of this highly policy-relevant tool has been quantified before by Bifulco and Reback (2014) using case studies in Albany and Buffalo, NY. We confirm their findings using a different approach that allows us to measure not only the mechanical effect of the aid on districts finances, but also districts' indirect adaptation to competition, through cost cutting or budget adjustments (which we quantify). Accounting for districts financial reaction to competition is particularly important if one wants to look not only at the short-term fiscal effect, but also at the long term. This is the second contribution of our paper.

Our paper also contributes to a rich literature that has reached mixed conclusions (primarily neutral to positive) on the effect of charter competition on the educational attainment of students in traditional public schools (Bettinger 2005; Imberman 2011; Zimmer and Buddin 2009; Davis 2013; Sass 2006; Winters 2012; Slungaard Mumma 2022; Gilraine et al. 2021). Our contribution to this literature is twofold. First, we provide one of the first estimations of the joint effect of charter expansion on fiscal and education outcomes. So far, these two questions have been separately studied, yet they are closely intertwined. We also use a novel identification method that simultaneously solves two sources of endogeneity: (i) changes over time in the nonrandom selection of students into charter schools and (ii) the nonrandom expansion of the best existing charter schools.

Our work builds more specifically on two recent papers that shed light on when charter competition affects traditional public school student attainment. Gilraine et al. (2021) find that charter schools that differentiate themselves more from traditional public schools (for example, through a focus on project-based pedagogy) exert less competitive pressure on public schools and therefore have no effect on student test scores. Slungaard Mumma (2022) compares schools near opened charter schools to schools near proposed charter sites that were never approved, which enables her to account for time-varying differences between schools that face high and low levels of charter exposure. The author finds no effect of charter schools on traditional public school achievement in Massachusetts, a result which contrasts with our findings. This difference might stem from the estimation of a school-level competition effect in Slungaard Mumma (2022) as opposed to a district-level competition effect in our paper. Schools located in the district but further away from a charter school may benefit from the temporary aid (and, possibly, resource reallocation) while being less affected by the reduced enrollment.[6] Taken together, our study and these recent studies help to better understand the variety of conclusions reached so far by the literature on charter competition. In

6. Compared to Slungaard Mumma (2022), our paper also jointly estimates the fiscal and educational effects of charter schools.

particular, both the level (school or district) at which the spillover is measured and the role of financial aid policies may matter. This could be relevant more broadly to the literature that examines competition in education markets, notably from private schools (Hoxby 2000; Figlio and Hart 2014; Epple, Romano, and Urquiola 2017).

Our paper also speaks to a large literature that has linked spending on education and student achievement (Jackson, Johnson, and Persico 2016; Lafortune, Rothstein, and Whitmore Schanzenbach 2018; Hyman 2017; Card and Payne 2002; Hoxby 2001). While there is a fair amount of evidence that links district spending and student learning, less is known on the best way to allocate resources. Spending on capital or building maintenance might not generate student progress to the same extent as spending on instruction, textbooks, or teachers' salaries. Although our paper does not identify the causal effect of spending, we jointly document the effect of charter competition on districts' spending and on their education outcomes.

Finally, our study contributes two methodological innovations with broad applicability in applied microeconomics. The synthetic control method has previously been used to estimate the reduced-form effect of interventions (Abadie and Gardeazabal 2003; Abadie, Diamond, and Hainmueller 2010). We show that it can also be used when traditional instrumental variables analysis is not possible due to low or highly selected take-up. We offer an adaptation of the SC method that allows us to estimate both first-stage and reduced-form effects. This could open the door to credible research designs to address questions that had so far been abandoned because of take-up or selection issues.[7]

More generally, the way we use the synthetic control method illustrates a broader usage of this method that can be employed in combination with difference-in-differences (or other empirical methods) in settings in which there is a treated group but no obvious control group. Rather than arbitrarily selecting a control group (think of the seminal Mariel boatlift paper, for instance, Card 1990),[8] we show that the control group can be selected using the synthetic control method, before being used in a standard difference-in-differences framework.

In addition to contributing to the long-running debate on the consequences of charter expansion for school districts, our empirical results are of immediate policy interest. Opposition to charter school expansion has grown in recent years, notably in states and districts that have historically had large numbers of charter schools. In November 2016, Massachusetts rejected a ballot question that would have expanded the charter school sector (Seelye and Bidgood 2016). The current mayor of New York has taken steps to restrict the growth of charter schools (Shapiro 2019), and in the April 2019 mayoral election in Chicago, which hosts 125 charter schools, both candidates said they wanted to halt charter school expansion. Given that about half of American states regulate

7. Building on the seminal paper by Abadie, Diamond, and Hainmueller (2010) that investigates the effect of the California tobacco ban on tobacco consumption, our adaptation of the SC method could be well suited to look at the subsequent effect of the ban on health outcomes.

8. To build a counterfactual for Miami, Card (1990) collected outcome data for four other U.S. cities: Atlanta, Los Angeles, Houston, and Tampa–St. Petersburg. These cities were selected because they exhibited a pattern of economic growth similar to that in Miami during the late 1970s and early 1980s.

charter expansion by setting caps, we expect discussions on the benefits and costs of raising these caps to become more frequent in the future. This analysis will bring some evidence into that debate.

II. Background

A. The Massachusetts Charter School Sector

Since its origins in the mid 1990s, the charter school sector in Massachusetts has grown, with 80 schools serving more than 40,000 children in the 2016–2017 school year. Charter students represent about 4.2 percent of the PK–12 public school population. Charter schools in Massachusetts share a number of organizational features with charter schools in other states. Typically, charter schools are free to organize instruction around a philosophy or curricular theme and to adopt a longer school day and year than traditional public schools. Charter schools also have more discretion over staffing and compensation than traditional public schools, and most of the time are exempt from local collective bargaining agreements. All charter schools in Massachusetts are nonprofit.

Massachusetts charter schools face stringent state accountability requirements. Charter schools must file for renewal of their charter every five years and are held accountable annually via reports, financial audits, and site visits. Renewal applications must show that the school has been successful in terms of student achievement. As a result of this strict renewal process, since 1994, 29 approved charter schools have either never opened, closed, or surrendered their charters.

B. District Payments to Charter Schools

The vast majority of charter school funding—about 90 percent—comes from tuition paid by the sending district, which is the district where a charter school student resides. Sending districts calculate their total charter school tuition payment by multiplying the number of students attending a charter school by a per-charter-student tuition amount, roughly equal to average per pupil spending in the sending district. In this sense, when a student switches to a charter school, the budget follows the student.[9]

This funding rule has raised concerns about financial stress on districts. A significant amount of the costs of running a school are fixed, such as spending on building, maintenance, administration, or nursing. Thus, when traditional public schools lose students to charter schools, they might not be able to reduce spending as much as the revenue losses they experience. Over the short term, the money traditional public schools are losing (the average cost of educating a student) might be significantly larger than the money they save (the marginal cost of educating a student).

In line with this hypothesis, recent papers have found that charter schools have a negative effect on districts' financial health. Arsen and Ni (2012) find that revenues decline more rapidly than costs in districts that lose students to charter schools in

9. In Massachusetts, charter schools are not excluded from local funding sources, as is the case in states such as Arizona and Idaho, for instance (Weber 2021). The funds the district pays out to charter schools come from local, state, and federal sources.

Michigan. Bifulco and Reback (2014) find a negative impact between $804 and $905 per pupil enrolled in the Albany city district schools, New York, and between $723 and $736 per pupil in Buffalo, NY. Using the same method for five urban and nonurban districts in North Carolina, Ladd and Singleton (2020) find a similarly large negative fiscal impact. Finally, Cook (2018) documents that charter competition directly reduces districts' state and federal revenues.[10]

These results are important because under shrinking resources and financial stress, districts might not be able to adapt to charter school competition as much as they wish. Importantly, the negative fiscal spillover effect is likely to differ over the short and long term. Some costs that are fixed over the short term become variable over the long term, such as spending on teachers, which cannot be easily adjusted in a year. This time difference was a key motivation for the introduction of short-term transitional aid programs.

C. Massachusetts Temporary Refund for School Districts

Several states have adopted programs that temporarily offset the charter tuition paid by sending districts. Since 2007–2008, New York provides transitional aid to school districts. The state reimburses 80 percent of the charter payments for students in the first year of charter attendance, 60 percent for the second year, and 40 percent for the third year. Illinois, New Hampshire, and Pennsylvania also adopted transitional schemes (see Online Appendix A).

In Massachusetts, the state funds a charter reimbursement program called "Chapter 46 Aid." When tuition payments increase for a school district over the prior year, the state reimburses that district for 100 percent of the increased cost in the first year. The state then reimburses 25 percent of this first-year amount for each of the subsequent five years.[11] The state also fully reimburses the "facilities aid" that sending districts pay to charter schools to help pay for school buildings. In the 2016–2017 school year, state aid amounted to $39.6 million for the 100/25/25/25/25/25 formula and $34.4 million for the facilities aid.[12]

D. Fiscal and Education Spillovers When Transitional Aid Is Available

Temporary state aid programs mean that for several years after a student switches from a traditional public school to a charter school, the district is reimbursed for

10. Weber (2021) studies the relationship between the local market share of independent charter schools and the finances of host school districts for 21 states. In contrast to previous studies, they find that in most states, charter expansion leads to higher per pupil revenue and expenditures and higher per pupil spending on instruction. These results should be interpreted with caution as their estimates are descriptive rather than causal.

11. For details on the tuition formula, see Massachusetts Education Laws and Regulations (603 CMR 1.00) related to Charter schools, section 7: http://www.doe.mass.edu/lawsregs/603cmr1.html?section=07 (accessed December 19, 2024). Online Appendix Table A.1 presents an example that describes the timing of Chapter 46 Aid.

12. In recent years, the Massachusetts Legislature has not appropriated sufficient funding to provide sending districts with 100 percent of the reimbursements they should have received. In 2013 and 2014, districts received 96 percent and 97 percent of the total reimbursement. This rate dropped to 69 percent, 63 percent, and 58 percent in the years 2015, 2016, and 2017. 2015 is the most recent year we consider in this analysis, so insufficient funding will only impact the last year of our sample.

some of the amount it would have spent on that student. As a result, all else equal, the district's revenue per pupil enrolled mechanically increases. However, other sources of revenue may shrink when charter schools expand. Cook (2018), for instance, shows that charter competition decreases traditional public school revenues raised through property taxes by depressing district property values. The overall effect of charter school expansion on district per pupil revenue is therefore not clear.

Even when per pupil revenue does increase, it is not clear that this will translate into higher per pupil expenditures. Evidence of low government spending elasticities and flypaper effects suggest that state aid programs might not be directed fully towards higher per pupil expenditures (Feldstein 1975; Hines and Thaler 1995; Inman 2008; Fisher and Papke 2000; Gordon 2004; Dee and Levine 2004). In this paper, we therefore investigate if charter expansion changes overall district revenue and expenditure on noncharter students when temporary aid is available.

By reducing fiscal pressure on traditional public schools, temporary state aid might also help them adjust to charter school competition by reallocating some of their resources toward inputs that are perceived as more productive in terms of student achievement (Hoxby 2003). There is evidence that parents factor school achievement ratings information into student enrollment decisions (Cullen, Jacob, and Levitt 2006; Hanushek et al. 2007; Hastings and Weinstein 2008). Districts might therefore try to attract or retain students by spending more on instruction and teachers and less on school administration or other support services. On the other hand, if traditional public schools lose students, per pupil expenditure on fixed costs should mechanically go up.

Understanding how charter competition affects per pupil revenue, spending, and resource allocation in traditional public schools is important because these are likely determinants of students achievement. A large literature has linked per pupil expenditures and student achievement (Jackson, Johnson, and Persico 2016; Lafortune, Rothstein, and Whitmore Schanzenbach 2018; Hyman 2017; Card and Payne 2002; Hoxby 2001). How resources are spent might also matter for student achievement. Spending on fixed costs (typically building maintenance or debt interest) might not generate student progress to the same extent as spending on more variable costs, such as instruction, textbooks, or teachers' salaries.[13] We analyze how charter expansion affects noncharter student achievement in the second part of the paper.

III. Data and Descriptive Statistics

We use data from three sources. First, we use the Massachusetts Students Information Management System (SIMS) for the 2002–2003 through 2014–2015 school years. These files contain information on all Massachusetts public school students' race, sex, ethnicity, reduced-price lunch status, special education status, English language learner status, community of residence, and current school. We use students' current school to identify charter school students and to measure the percentage of

13. The literature is mixed regarding the effect of capital spending on student achievement. Martorell, Stange, and McFarlin (2016) find null effects, while Cellini, Ferreira, and Rothstein (2010) and Hong and Zimmer (2016) provide evidence that capital outlays can have positive achievement effects.

students enrolled in charter schools in each district. Then we use student identifiers to merge SIMS demographic data with test scores from the Massachusetts Comprehensive Assessment System (MCAS) database, covering the years 2003–2014. MCAS is administered each spring, typically in Grades 3–8 and 10. Its database contains raw scores for math and English language arts (ELA). We standardized scores by subject, grade, and year to have mean zero and unit variance in the population of students attending Massachusetts public schools.

Information on district revenue and expenditure comes from the Annual Survey of School System Finances collected annually by the U.S. Census Bureau (2017). All school districts that provide elementary or secondary education are included in the annual survey. The data include total district revenue, as well as revenue by sources (federal, state, and local). The temporary aid from the state is included in the category "All other state revenue."[14] The data also include information on total expenditure, as well as expenditure by object (instruction, support service functions, salaries, and capital outlay). Importantly, a separate section details district payments to charter schools. Online Appendix B provides a detailed presentation of the data and the variables we use in the analysis.

We build several outcomes to examine the fiscal spillovers of charter expansion. To measure effect on revenue, we look at the total revenue to which we subtract the district payments to charter schools. This provides a measure of a district's net revenue available to the district. We also use information on "other state revenue" to quantify the transitional aid received. To measure effect on districts expenditure, we use districts' (i) total expenditure, and their expenditure on (ii) fixed costs, (iii) instruction, (v) salaries, and (v) support services (further decomposed into pupil support, instructional support, and general administration). For each outcome, we divide the district expenditure on noncharter students by the total number of public school noncharter students in elementary, secondary, and high schools in that district. Expenditure on fixed costs is the sum of expenditures on plant operation and maintenance, student transportation, school administration and general administration, interest on debt, and some business-related support services (see Online Appendix B for more details). Expenditure on instruction corresponds to expenditure for interactions between teachers and students in the classroom, as well as cocurricular activities. These interactions can be activities of not only teachers but also of instructional aides or assistants engaged in regular instruction, special education, and vocational education programs.[15] Expenditure on salaries includes the salaries and wages paid by the district for all staff. Finally, expenditure on support services encompasses student support (such as counseling), teacher support (such as training or instruction supervision), and school administration.[16] To estimate charter

14. The form that schools fill in annually states that schools should report in this category amounts for specific programs including "enrollment increases and losses."

15. Spending on instruction includes salaries.

16. More specifically, expenditures on support services are the sum of several expense types. First are expenditures for administrative, guidance, health, and logistical support, including social work, student accounting, counseling, student appraisal, information, and placement services, as well as medical, dental, nursing, psychological, and speech services. Expenditures on support services also encompass expenditures for instruction supervision, curriculum development, instructional staff training, academic assessment, and media, library, and instruction-related technology services. Support tasks also relate to school administration, including expenditures for the principal and school office.

Table 1
Descriptive Statistics for Students and Districts

	All Districts (1)	High-Charter-Share Districts (2)	Low-Charter-Share Districts (3)	Expanding Districts (4)	Nonexpanding Districts (5)
Panel A: Students' Characteristics					
Female	0.492	0.492	0.491	0.491	0.492
Black	0.080	0.131	0.030	0.246	0.057
Hispanic	0.138	0.227	0.052	0.420	0.099
Asian	0.051	0.053	0.048	0.068	0.048
Subsidized lunch	0.337	0.496	0.183	0.772	0.277
Special education	0.176	0.188	0.164	0.198	0.173
Limited English proficient	0.049	0.086	0.013	0.154	0.034
Math test score	0.030	–0.162	0.216	–0.393	0.088
ELA test score	0.024	–0.190	0.230	–0.496	0.095
Panel B: Districts' per Pupil Revenue					
Total revenue (net of chart payments)	16,291	17,356	15,539	17,965	16,239
Federal revenue	860	1.063	733	1,964	825
State revenue (net of refund)	5,220	5,626	4,985	9,549	5,084
Local revenue	10,212	10,666	9,821	6,452	10,330
State temporary refund	58	123	17	145	56

(*continued*)

Table 1 (*continued*)

	All Districts (1)	High-Charter-Share Districts (2)	Low-Charter-Share Districts (3)	Expanding Districts (4)	Nonexpanding Districts (5)
Panel C: Districts' per Pupil Expenditures					
Total spending	15,917	16,898	15,235	17,492	15,867
Spending on instruction	9,075	9,857	8,534	9,848	9,050
Spending on fixed costs	3,519	3,732	3,374	3,490	3,520
Spending on support services	2,998	3,263	2,827	3,421	2,985
Spending on salaries	8,353	8,724	8,086	8,651	8,344
Charter payments	219	512	38	598	207
Capital outlays	651	588	700	1,378	628
Number of students	277,769	136,414	141,355	33,502	244,267
Enrollment (share)	1	0.49	0.51	0.12	0.88
Number of schools	1,185	625	601	204	1,009
Number of districts	293	113	180	9	284

Notes: The upper part of this table describes Massachusetts 5th–8th-graders in 2009–2010, the year before the cap reform. The bottom part of the table reports districts' per pupil expenditures. In Columns 2 and 3, districts' charter shares are higher (Column 2) and lower (Column 3), respectively, than the median value. Columns 4 and 5 are restricted to districts where the charter sector expanded (Column 4) and districts where the charter sector did not expand (Column 5) after the 2011 reform. Statistics include Massachusetts middle school students for whom we have baseline characteristics. The lower part of the table describes districts' expenditures for primary schools, secondary schools, and high schools.

effectiveness, we match the state administrative education data with admissions lotteries from 22 charter schools that enroll middle school students (in Grades 5–8) from the 2002–2003 to 2013–2014 school years. Online Appendix Table A.8 describes the charter schools that are eligible for the lottery instrument, as well as the years and grades of the lottery. We exclude charter schools that closed, declined to participate, had insufficient records, were not oversubscribed, or served alternative students (like students at risk of dropping out). The resulting lottery sample includes 13 charter schools in Boston and nine charter schools in other districts.

Panel A of Table 1 reports statistics on all middle school students in Massachusetts in the 2009–2010 school year, before the cap reform. In addition to being much more likely to be Black or Hispanic and to receive subsidized lunch, students in districts with a high share of charter schools have significantly lower math and ELA test scores than students in low-charter-share districts. These differences are even starker when comparing districts that expanded their charter sector after the cap reform and nonexpanding districts. In expanding districts, 66.6 percent of students are Black or Hispanic, as opposed to only 15.6 percent in nonexpanding districts. Students in expanding districts are also 49.5 percent more likely to have subsidized lunch, and they score 0.48 standard deviations lower in math and 0.59 lower in ELA. Such differences are not surprising, given that charter schools generally open in disadvantaged areas and that the 2011 reform only raised the cap on charter schools for the districts in the lowest tenth percentile of test scores.

Interestingly, the differences shown in Panel B, which reports districts' average per pupil expenditures in school year 2009–2010, are quite the opposite. High-charter-share districts spend on average $2,000 more per pupil than low-charter-share districts. Further, high-charter-share districts also spend more on instruction, fixed costs, support services, and salaries.

IV. Methodology

A key difficulty in analyzing the effect of charter expansion is charter schools' nonrandom initial location and expansion (Glomm, Harris, and Lo 2005; Bifulco and Buerger 2015).[17] To deal with this endogeneity, we exploit a 2010 cap reform in Massachusetts, which led to charter sector expansion. Like 24 other U.S. states, Massachusetts regulates charter expansion by a system of caps. In 1997, the state adopted a 6 percent limit on district funding allocable to commonwealth charter school tuition.[18] This cap was raised in 2000, to 9 percent, and raised again in 2010. The 2010 reform raised the 9 percent limit on district funding allocable to commonwealth charter schools to 18 percent, but only for districts with student performance in the lowest 10 percent of districts statewide. For all other districts, the 9 percent limit was

17. Charter schools tend to locate in districts where the population is diverse, per pupil expenditure is high, teacher costs are low, and public school achievement is relatively low (Glomm, Harris, and Lo 2005; Bifulco and Buerger 2015).

18. In 1997, the numerical cap was raised to 50. The funding cap only applies to commonwealth charter schools, which represent the vast majority of the charter sector. In 2016–2017, 71 of the 80 operating charter schools were commonwealth.

unchanged.[19] The former group therefore became *eligible* for expansion, while districts above the tenth percentile were *noneligible* for expansion. We exploit this 2010 reform to identify the spillover effects of charter school expansion on traditional public schools.

Only a subset of the eligible districts took advantage of the cap reform to expand their charter sector. This resulted in a low take-up among eligible districts. This prevents us from using the eligibility criterion—being in the lowest 10 percent of student achievement—as an instrument for the share of students who enroll in charter schools.

Among eligible districts, selection into expansion might be driven by several elements. First, before the reform, the cap was not equally binding for all districts. Districts for whom the cap was binding might be expected to take more advantage of the reform to expand their charter sector. Second, low-performing districts can only increase charter enrollment if nonprofit organizations, teachers, or parents decide to submit an application to either open a new charter school or expand an existing one. In addition, for districts that were close to the 9 percent limit on charter funding, only a proven provider may submit a new application, that is, an existing charter school operator with strong track records.[20] Finally, applications for new charter schools might be more likely in districts that have high per pupil expenditures because charter tuitions are mostly based on per pupil expenditures in the sending districts.[21] Each of these determinants might marginally affect the likelihood that an eligible district expands its charter sector, but none happens to be a strong predictor, which ruled out using these variables as instruments for expansion.

To address selection into expansion, our methodology consists of (i) identifying the (selected) group of expanding districts and then (ii) building a SC that experienced the same evolution of both its financial outcomes and charter share before the reform. We can then use the post-reform difference in charter share and financial outcomes to estimate the reduced-form and first-stage effect of the cap reform. Finally, to construct our main estimates of the causal effect of charter share on financial outcomes, we build an IV-DiD methodology that uses the districts from the synthetic control group (but without the weights). We detail each step in the next sections.

A. Identifying Expanding Districts

Some eligible districts took advantage of the reform to expand their charter sector (*expanding districts*). We identify an expanding district by looking at its charter sector growth

19. Districts are in the lowest 10 percent of achievement if their average math and ELA scores on the Massachusetts Comprehensive Assessment System have been in the lowest 10 percent statewide for two consecutive years.

20. Proven provider status was required for charter applications in districts with the lowest 10 percent of student performance whenever additional charter enrollment would cause tuition payments to exceed 9 percent of the district's net school spending. Criteria for proven provider classification include performance on state achievement tests, enrollment, attendance, retention, attrition rates, graduation rates, dropout rates, suspension numbers, effective governance, and competent financial management.

21. School districts' revenues are largely based on property taxes. This creates large variations in revenues and per pupil expenditures across districts.

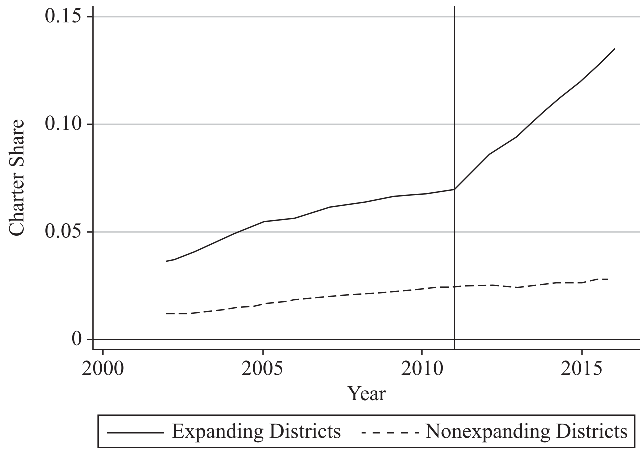

Figure 1

Charter Sector Expansion After the 2011 Reform

Notes: The figure plots the share of students attending a charter school over time. Figure 1 plots the share for elementary, secondary, and high school students, while Figure 2 is limited to middle school students. The plain line represents districts that saw an increase in the share of students attending a charter school after the 2011 reform (expanding districts), and the dotted line represents the districts that did not expand their charter sector after the reform (nonexpanding districts).

before and after the 2011 reform.[22] As displayed in Figure 1, for both the pre-reform and the post-reform periods, we calculate the average yearly increase in a district's charter share. We identify nine expanding districts whose slope is larger after the reform than before and that are in the lowest tenth percentile of student test scores: Boston, Chelsea, Malden, New Bedford, Lynn, Gill-Montague, Lawrence, Winchendon, and Salem.

Figure 1 shows a clearly accelerated charter expansion in these expanding districts after the 2011 reform. In the four years following the 2011 cap increase, the proportion of students attending a charter school jumped from 7 percent to 12 percent in the expanding districts. The charter share remained relatively constant at about 3 percent in all other nonexpanding districts.[23] Figure 2 reports the charter share evolution for middle schools only. Charter enrollment grew at the elementary and high school levels, though not as dramatically as in middle schools, where the proportion of students attending a charter school jumped from 10 percent to 15 percent in expanding districts. For that reason, we focus on middle school students when studying charter expansion effect on student achievement. However, we analyze fiscal spillovers for all levels—primary, middle, and high schools—as the expenditure variables are not decomposed by level.

22. We only look at expansions that are triggered by the 2011 reform. Charter schools also opened and expanded in other years but to a significantly lesser extent. Between one and five charter schools opened in each year from 2002 to 2010, while 13 charter schools opened in 2011.

23. We discard three districts from the group of expanding districts: Gateway, because it experienced a decreasing charter share before the cap reform, and Lowell and Chicopee, because they only had very marginal changes in slopes after the reform and idiosyncratic evolution patterns that do not seem related to the cap reform.

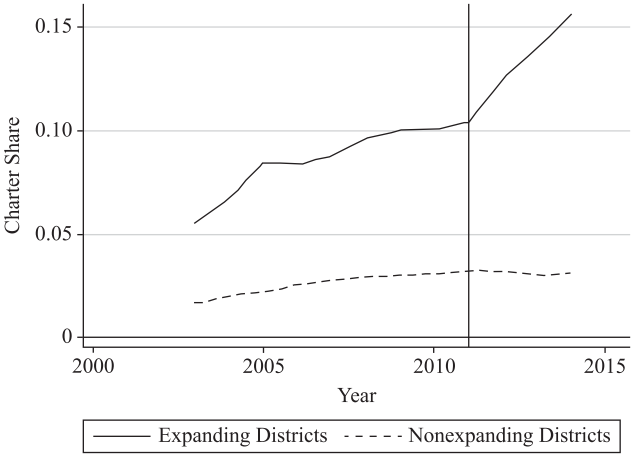

Figure 2

Charter Sector Expansion After the 2011 Reform—Middle Schools

Notes: The figure plots the share of students attending a charter school over time. Figure 1 plots the share for elementary, secondary, and high school students, while Figure 2 is limited to middle school students. The plain line represents districts that saw an increase in the share of students attending a charter school after the 2011 reform (expanding districts), and the dotted line represents the districts that did not expand their charter sector after the reform (nonexpanding districts).

B. Synthetic Control

Having identified the expanding districts, we build a synthetic control for them. We follow Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010, 2015), except for one key difference—it is vital that the SC match not only the evolution of the outcome variable in the expanding districts (as the "standard" SC method requires), but also the evolution of the charter share. We therefore modify the synthetic control method to estimate both a reduced-form effect and a first stage. The intuition is easy to illustrate with the seminal work by Abadie, Diamond, and Hainmueller (2010), which estimates the effect of the passage of Proposition 99 in California on per capita cigarette consumption. This can be seen as a first stage. The question they answer is: How should we build the SC if we want to estimate how the change in per capita cigarette consumption due to Proposition 99 subsequently affected health outcomes in California? Put differently, how should we construct the synthetic control to estimate both the first-stage effect of Proposition 99 on per capita cigarette consumption and its reduced-form effect on health outcomes in California? Similarly, we want to understand how the change in district charter share due to the charter cap reform subsequently affected district per pupil spending. We show how to modify the synthetic control method to estimate both the first-stage and the reduced-form effects of a reform. Online Appendix C details the methodology, which we summarize below.

Let's consider the following equation in which the charter share C_{jt} is the endogenous variable:

(1) $Y_{jt} = \gamma_1 + \rho C_{jt} + \upsilon_{jt}$

Because C_{jt} is potentially correlated with district-specific unobservables υ_{jt}, we instrument C_{jt} with a dummy for expanding districts, E_j, that takes the value one for expanding districts and zero for the synthetic control districts. The first-stage and reduced-form equations are:

(2) $C_{jt} = \gamma_2 + \beta E_j + u_{jt}$

(3) $Y_{jt} = \gamma_1 + \alpha E_j + \xi_{jt}$

We use the following synthetic control procedure to estimate the reduced-form treatment effect, α, and the first-stage coefficient, β. Consider a sample of $J + 1$ districts indexed by j and assume that district $j = 1$ will be the treated district (that is, the expanding district), while districts $j = 2$ to $j = J + 1$ are potential control districts. The sample includes T_0 pre-reform years, as well as T_1 post-reform years, with $T = T_0 + T_1$.

Assume a vector of weights $\left(w_2^*, \ldots, w_{J+1}^*\right)$ that makes it possible to equalize, for each pre-reform year, (i) the values of the pre-reform outcomes Y_j for the treated districts and the synthetic control, (ii) the values of the endogenous variable C_j for these two groups, and (iii) the values of some predictor variables X_j of the outcome. Formally, for each pre-reform year t:

(4) $\sum_{j=2}^{J+1} w_j^* Y_{jt} = Y_{1t}$, and $\sum_{j=2}^{J+1} w_j^* C_{jt} = C_{1t}$, and $\sum_{j=2}^{J+1} w_j^* X_{jt} = X_{1t}$

If the vector of weights $\left(w_2^*, \ldots, w_{J+1}^*\right)$ exists, Abadie, Diamond, and Hainmueller (2010) show that, for each post- reform year t the reduced-form treatment effect α_t can be estimated by:[24]

(5) $\hat{\alpha}_t = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}$

Using the same proof, we show that the first-stage coefficient can be estimated by:

(6) $\hat{\beta}_t = C_{1t} - \sum_{j=2}^{J+1} w_j^* C_{jt}$

The key modification we introduce is that the weights used to estimate the reduced-form treatment effect match not only the values of the outcome variable, but also the values of the endogenous variable. Our modified SC uses the same weights to estimate both the first stage and the reduced form. This ensures that the control group is the same for both stages.[25]

24. Synthetic control weights are nonnegative and sum up to one. Doudchenko and Imbens (2016) propose a more general class of synthetic control estimators that allows researchers to relax some of the restrictions in the Abadie, Diamond, and Hainmueller (2010) method. They allow the weights to be negative, do not necessarily restrict the sum of the weights, and permit a permanent additive difference between the treated unit and the controls, similar to differences-in-differences procedures.

25. In practice, using the same weights might yield a poor fit for the first stage or reduced form. In that case, using different weights might be more appropriate, although this would change the estimator.

In practice, Condition 4 often only holds approximately. Perfect equality between treated districts and synthetic districts can only be obtained if the values of the predictor variables for the treated units fall within the convex hull of the values for the potential synthetic control districts' predictor variables. When the equality does not hold perfectly, we evaluate the discrepancy (or goodness of fit) by computing the root mean squared prediction error (RMSPE) as follows:

$$(7)\quad \begin{aligned} RMSPE_Y &= \left[\frac{1}{T_0}\sum_{t=1}^{T_0}\left(Y_{1t}-\sum_{j=2}^{J+1}w_j^{*}Y_{jt}\right)^2\right]^{\frac{1}{2}} \text{ and} \\ RMSPE_C &= \left[\frac{1}{T_0}\sum_{t=1}^{T_0}\left(C_{1t}-\sum_{j=2}^{J+1}w_j^{*}C_{jt}\right)^2\right]^{\frac{1}{2}} \end{aligned}$$

At that stage, it becomes clear why it is not satisfactory to construct the synthetic control by only matching the outcome variable (for instance, per pupil expenditure) in expanding districts and the synthetic control. Doing so could result in a very good fit in per pupil expenditures between expanding and synthetic control districts but a potentially large difference in charter share. Given that charter share is a determinant of spending, to obtain similar spending levels despite large differences in charter share, there must be other differences in unobserved predictors of spending that compensate for the charter share gap.

Similarly, if we construct the synthetic control by only matching the endogenous variable (charter share) in expanding districts and the synthetic control, then the synthetic control might not match financial outcomes very well. This would again suggest differences in unobserved predictors that justify the gap in spending despite identical evolutions of the charter share. As a result, it would be unclear whether the post-reform change in outcome is due to the post-reform change in charter share or to the post-reform change in other unobserved characteristics. Finding weights that match not only the outcome variable but also the charter share is fundamental with respect to the independence assumption of the instrumental variables model.[26]

We use cross-validation to construct predictor variable weights; that is, we split the pre-treatment sample in two and choose weights to minimize out-of-sample fit. When this procedure tends to produce large RMSPEs (possibly because splitting the sample introduces more noise with our limited number of years), we instead adopt a standard iterative optimization procedure that searches among all (diagonal) positive semidefinite V-matrixes and sets of w-weights for the best-fitting convex combination of the control units. Best-fitting refers to the fit between the outcome of the treated districts and that of its synthetic control before the reform takes place (see Abadie, Diamond, and Hainmueller 2010). Online Appendix E presents sensitivity tests in which we vary the method and the donor pool.

26. An alternative identification strategy is to identify the expanding districts, select a plausible control group "by hand," and use the interaction between an expanding-district dummy variable and a post-reform dummy as an instrument for the share of students who enroll in charter schools. Yet, using that differences-in-differences (DiD) as an instrument would require parallel pre-trends in both the fiscal outcomes and in the charter share, which is often not verified in our context. In addition, it is unclear what the appropriate control group should be. Instead of arbitrarily selecting a control group, we rely on a data-driven procedure to construct a suitable comparison group.

C. Identifying Assumptions

1. No Interference Between Expanding and Nonexpanding Districts

The synthetic control method assumes no interference between expanding and nonexpanding districts (Rosenbaum 2007). In other words, increased charter attendance in expanding districts is assumed to have no effect on outcomes in nonexpanding districts. This assumption is relatively plausible for fiscal spillovers. How much a district pays in charter tuition and how much aid it receives from the state depends exclusively on the number of students enrolled in charter schools in its zone. Competition effects from charter school expansion are also likely to be limited at the district level. In expanding districts, 91 percent of charter school students come from the district in which the school is located. For surrounding districts, then, fear of losing students should be limited. In addition, the synthetic control method selects a limited number of control districts (usually between five and ten) out of a sample of 32 potential control districts. There is only a small probability that these synthetic districts are close enough to one of the expanding districts to feel competitive pressure. Even if expanding districts had spillover effects on surrounding districts, this would most likely have an attenuating effect on our estimates.

2. Independence assumption

The independence assumption states that the instrument is independent of mean potential outcomes. In other words, unobserved determinants of both charter share and financial outcomes should not differ in the expanding and the SC districts. We impose several restrictions on the donor pool of the synthetic control to make this assumption plausible. First, we restrict the donor pool to districts that were not eligible to expand. These are districts with student achievement between the tenth and 20th percentile, hence just above the eligibility threshold. Using only noneligible districts ensures that the SC does not contain districts that were eligible for expansion but decided to not expand their charter sector for reasons that might be related to their financial situation. However, noneligible districts are not the ideal donor pool because students in these districts are slightly better performers than students in districts that are below the tenth percentile.[27] We therefore add to the donor pool districts that were eligible to expand and that received applications for new charter schools after the cap reform, but whose applications were rejected for reasons unrelated to potential fiscal or education outcomes. This group of districts is plausibly very similar to the group of expanding districts. Indeed, between 2011 and 2015, the state received 49 applications for new charter schools and rejected 19 of them. For each application, we collected information on the associated sending districts and on the rejection reason that is reported in the states meeting minutes. We then classified the rejection reasons as related or not related to the fiscal (or education) situation of a district.

27. Choosing donor districts that have similar characteristics to the expanding districts is important to avoid interpolation biases. Because the synthetic group is meant to reproduce the charter expansion that would have been observed for expanding districts without the 2011 cap reform, we discard from the donor pool two districts that experienced large increases in charter share after the reform, despite not being considered expanding districts, because their charter expansion did not accelerate after the reform.

Online Appendix D presents the 19 rejected applications, their corresponding sending districts, whether we classified the rejection as related to a potential fiscal or education outcome, and the rules we used to categorize the rejection reasons.[28] For instance, some applications were rejected because of the reviewer's skepticism on the ability of the suggested management team to lead the charter school effectively. This would typically be considered as a reason that is unrelated to a district's financial situation.

Finally, being an expanding district is assumed to only affect student outcomes through its effect on the probability of charter enrollment, not through any other factor or unobserved characteristic. This exclusion restriction would hold if no other reform was adopted in 2011 or if the reforms adopted that year impacted financial outcomes or student achievement equally in expanding and nonexpanding districts. Other reforms were indeed adopted in 2010. The Act Relative to the Achievement Gap included provisions for school turnarounds and the creation of innovation schools. We show that our results are not driven by the introduction of these new schools in the robustness checks section.

D. Inference

A caveat of the synthetic control method is that standard (sampling-based) inferential techniques have not been developed for this method.[29] This is one reason why our results mostly focus on the IV-DiD estimation described below. However, we do also apply the permutation inference that has become standard for synthetic controls, as described in Abadie, Diamond, and Hainmueller (2010). Following their approach, we sequentially apply the synthetic control algorithm to a random selection of nine districts drawn from the pool of potential controls.[30] Then, we compare the placebo treatment effect so estimated with the actual treatment effect for the expanding districts. Since none of the donor pool districts receive treatment, variation between each combination of nine placebo districts and its synthetic match occurs randomly. We can therefore assess whether the treatment effect measured for the expanding districts is larger than that for districts chosen at random.[31]

28. We identified six criteria out of 93 as potentially related to a potential fiscal or education outcome. Three applications were rejected for reasons unrelated to potential outcomes. These applications cover three potential sending districts that we drop from the donor pool to construct the synthetic control.

29. There are two reasons for this. First, synthetic control analysis is typically not based on a sample from a population aggregate, but rather on an entire population, such as all districts in Spain or states in the United States. Due to the absence of sampling variation, all uncertainty surrounding the estimands is design-based and stems from unobservability of potential outcomes, in particular, unobservability of what the treated districts' potential outcome would be in the absence of the treatment (Abadie et al. 2020). Second, we only have a limited number of districts in the control pool (between five and ten) and a limited number of post-reform periods covered by the sample.

30. Our treated unit is an aggregate of nine expanding districts. To match the size of the treated unit, we also run the permutation inference on combinations of nine districts.

31. We pay particular attention to the fit quality between each placebo district and its synthetic control. Fit quality might be poor if some placebo districts have very low (or high) achievement or very low (or high) charter share. We therefore only consider placebo districts for which the RMSPE is not more than three times larger than the RMSPE of the expanding districts. We apply this rule for the RMSPE of both the charter share and the outcome (Ferman and Pinto 2017).

We do inference separately for the first-stage and the reduced-form estimates. We cannot do placebo inference on the IV estimand because, by definition, the first-stage estimates are close to zero for the placebo units. This implies that their IV estimand (the ratio reduced form over first stage) tends to infinity.

E. Differences-in-Differences Instrumental Variables (IV-DiD)

We complement the SC method with an IV-DiD method in which we instrument the charter share using the interaction between a post-reform dummy and whether a student lives in an expanding district (while controlling for post-reform year and expanding district main effects). This alternative identification allows us to assess the results' significance using standard inferential techniques. It also provides a robustness check for the results. The reduced-form effect of the charter cap reform is calculated as the change in outcome between the pre- and post-reform years in charter expanding districts minus the change in achievement in nonexpanding districts. The first-stage estimate corresponds to the difference in charter sector growth between expanding and nonexpanding districts.

The IV-DiD rests on the idea that the pre-reform expanding districts' outcomes provide a good counterfactual for what would have happened to post-reform expanding districts' outcomes in the absence of the reform. Subtracting the changes in nonexpanding districts' outcomes adjusts for any pre-post variation that affected expanding and nonexpanding districts equally over the period. This DiD successfully identifies charter school spillover under the standard parallel trends assumption that, absent the reform, the change in outcome over this period would have been the same in expanding and nonexpanding districts. We show graphical evidence of the first-stage and parallel trends in the "Results" section below.[32]

Finally, because both the expanding districts and the control group are partially treated before the reform (in the sense that they have a nonzero charter share), the fuzzy-DiD environment requires two additional assumptions (de Chaisemartin and D'HaultfŒuille 2018). First, in both expanding and nonexpanding districts, the average treatment effect among districts that had a positive pre-reform charter share should remain stable over time. This assumption is relatively plausible because the institutional features accompanying the charter expansion are the same before and after the 2011 reform. In particular, the refund scheme does not change. Second, Online Appendix Figure A.2 shows that the charter share evolution for each of the control groups we use is relatively stable post-reform. Treatment intensity is therefore unlikely to change in the control group after the reform. Biases generated by differential treatment effects between the treatment and control groups should be limited.

The second-stage equation for the spillover analysis is:

$$(8)\quad Y_{dt} = \alpha_2 + \delta_2 Post_t + \theta_2 E_d + \lambda C_{dt} + \epsilon_{dt}$$

where Y_{dt} is per pupil expenditure or achievement in district d in year t, δ_2 is the coefficient on a post-reform dummy $Post_t$, θ_2 is the coefficient of a dummy for expanding

32. See Hudson, Hull, and Liebersohn (2017) for a discussion of the assumptions underlying the IV-DiD.

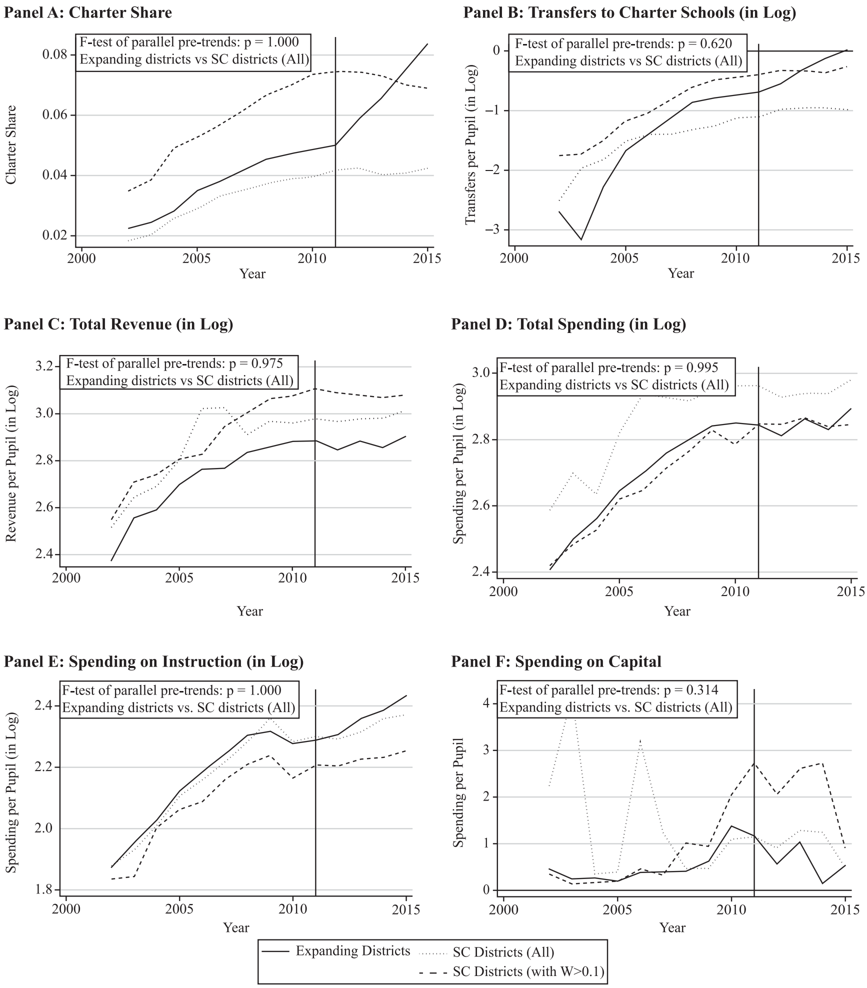

Figure 3

Trends in Charter Share and Districts' per Pupil Revenues and Expenditures

Notes: This figure plots the share of students attending a charter school (Panel A), districts' payments to charter schools (Panel B), per pupil revenue (Panel C), per pupil expenditures (Panel D), and per pupil expenditures on instruction (Panel D) and on capital (Panel F). For all expenditure variables (except spending on capital), we use the log of the variable. The plain lines represent expanding districts. The gray dotted lines represent synthetic control districts, and the black dotted lines represent synthetic control districts whose weight is larger than 0.1. Each panel also reports the *p*-value from an *F*-test of parallel pre-trends between expanding districts and unweighted synthetic control districts. To perform this test, we run an event-study regression of each outcome on the interactions between expanding districts and each year. We test if the coefficients of the expanding-by-year effects are jointly equal to zero in the pre-reform years.

districts E_d, and ϵ_{dt} is an error term. Our treatment variable, C_{dt}, measures the share of students enrolled in charter schools.

To instrument charter share, we use an overidentified model in which we use three instrumental variables (Z_{1dt}, Z_{2dt}, and Z_{3dt}), which are the interaction between a post-reform dummy and, respectively, a dummy for Boston, for other urban expanding districts, and for nonurban expanding districts. The first stage for this two-stage least squares (2SLS) procedure is:

$$(9) \quad C_{dt} = \alpha_1' + \delta_1' Post_t + \theta_1' E_d + \gamma_1 Z_{1dt} + \gamma_2 Z_{2dt} + \gamma_3 Z_{3dt} + \nu_{dt}'$$

Finally, to construct the control group of nonexpanding districts, we reuse the group of districts identified by the synthetic control method. Figure 3 reports pre-trends in charter share and outcomes in expanding and synthetic control districts. The gray dotted line plots trends for SC districts as a standard control group, without the district weights computed by the synthetic control method.[33] There is variation across outcomes in how parallel pre-trends are. For instance, the trends for the "transfers to charter schools" outcome is worse than for the other outcomes, which is primarily due to SC districts with a very small weight (smaller than 0.1). Omitting these districts yields better pre-trends, as shown by the black dotted line in Figure 3. In the rest of the analysis, we will systematically report IV-DiD results with and without SC weights to ensure that SC districts with a small weight do not drive the results.[34]

V. Results on Fiscal Spillovers of Charter Expansion

A. Effect of Charter School Expansion on District Revenue

Figure 4 shows estimates of the first-stage (Panel A) and reduced-form effects (Panels B–F) of lifting the cap on charter schools in 2011.[35] Between 2011 and 2015, charter share in expanding districts increased by more than three percentage points compared to the synthetic control, going from 5 percent to more than 8 percent. A first direct consequence of that expansion is that districts' transfers to charter schools increased by 66.5 percent over the same period (Figure 4B). These transfers were partially compensated by the temporary refund, which also went up significantly

33. Each figure also reports the p-value from an F-test of parallel pre-trends. To perform this test, we run an event-study regression of each outcome on the interactions between expanding districts and each year. We test if the coefficients of the expanding-by-year effects are jointly equal to zero in the pre-reform years. The large p-values we obtain suggest that the pre-reform coefficients are jointly equal to zero for all outcomes.

34. We do not include group-specific time trends in our specification, following evidence from Borusyak, Jaravel, and Spiess (2024) that group-specific trends introduce underidentification problems. DiD specifications that include group-specific trends estimate an average of the dynamic treatment effects that severely overweighs short-run effects and weighs long-run effects negatively. This is a particular concern in our setting because the long-run effects are larger than the short-run effects.

35. Because we run the synthetic control algorithm separately for each outcome, the group of synthetic control districts and the first-stage estimates differ for each outcome. For each outcome, Online Appendix Table A.4 lists the selected synthetic control districts and associated weights. Figure 4 shows the first stage using weights computed for the total per pupil expenditure outcome. Online Appendix Figure A.1 summarizes the first stage across all outcomes.

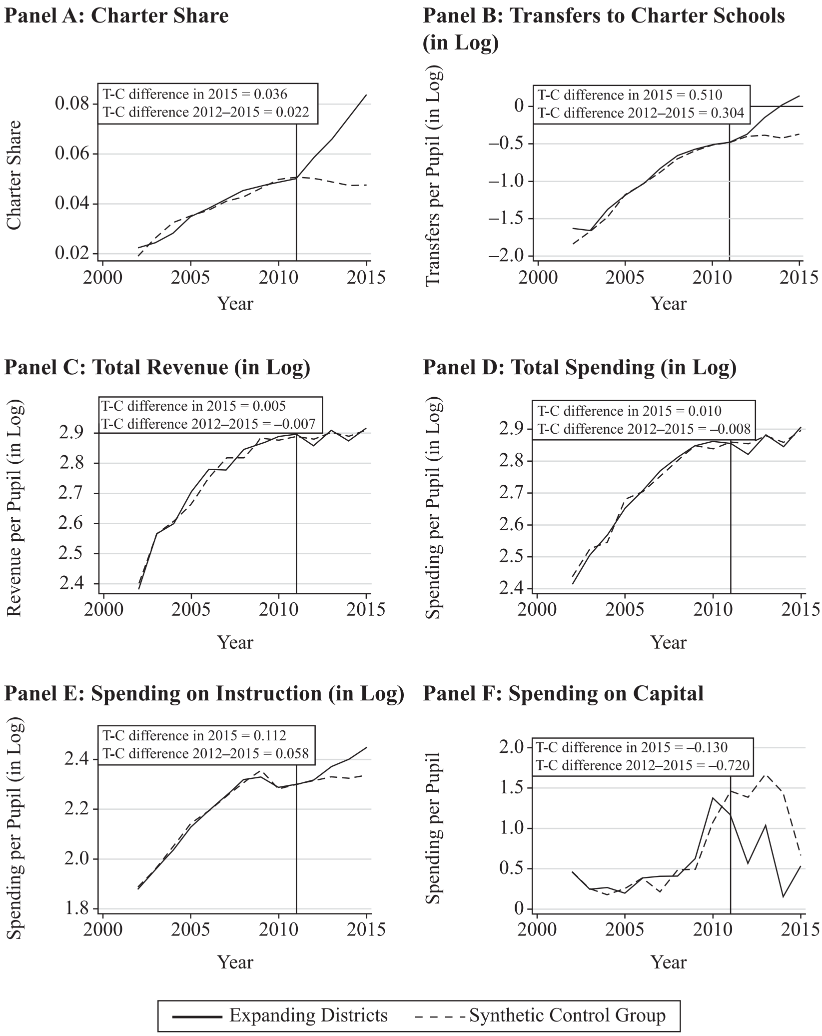

Figure 4

Charter Share and Districts' per Pupil Expenditures in Expanding Districts and Synthetic Control Districts

Notes: This figure plots the share of students attending a charter school (Panel A), districts' transfers to charter schools (Panel B), per pupil net revenue (Panel C), per pupil expenditures (Panel D), and per pupil expenditures on instruction (Panel E), and capital outlay (Panel F). For all expenditure variables (except capital), we use the log of the variable. The plain lines represent districts that saw an increase in the share of students attending a charter school after the 2011 reform (expanding districts), and the dotted lines represent the synthetic control districts. For expanding districts, we plot the average charter share and expenditures. For synthetic control districts, we plot the weighted average of the charter share and expenditures, using the weights defined by the synthetic control method. The coefficients reported in the top left side of each panel correspond to the difference in outcome between expanding districts and the synthetic control group in 2015 (top coefficient) and the average difference over the period 2012–2015 (bottom coefficient).

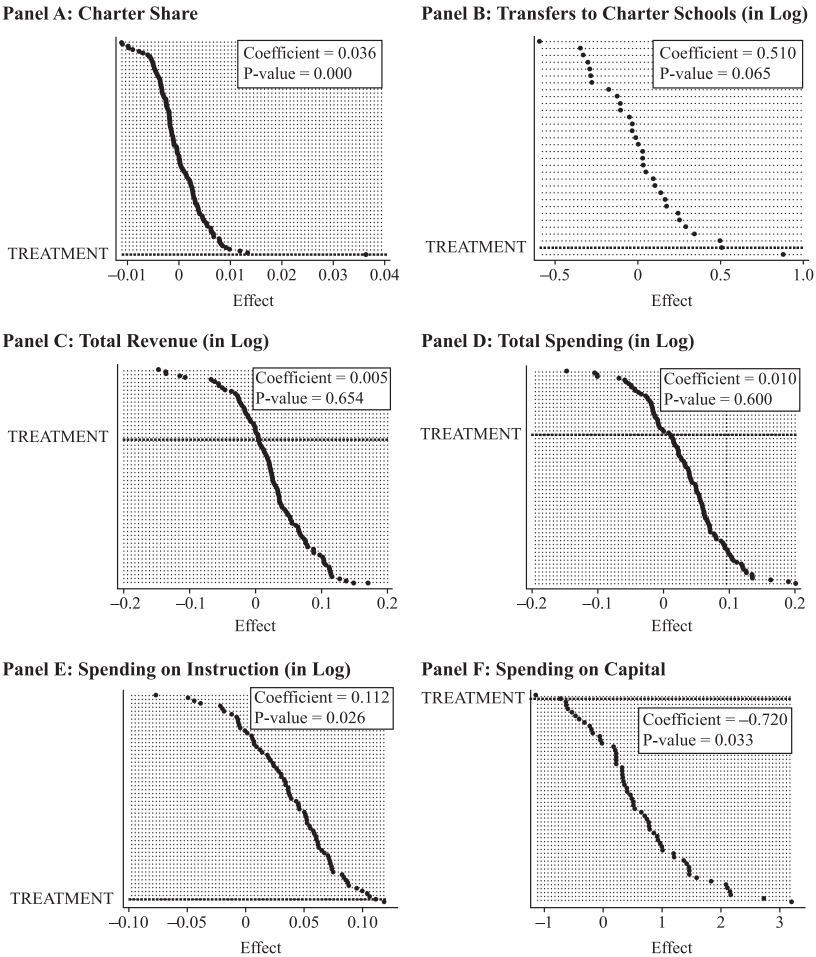

Figure 5

Placebo Inference for the Fiscal Impact of Charter School Expansion

Notes: This figure plots the distribution of the charter expansion's effect on districts' per pupil revenue and expenditures in 2015, as measured by the synthetic control method. The "treatment" lines report the coefficients when expanding districts are compared to their synthetic control districts. The exact value of each coefficient is reported in the top right corner of each figure. For all expenditure variables (except capital), we use the log of the variable. The other lines in the figures report the coefficients when a placebo group of non-expanding districts is compared to its identified group of synthetic control districts. The p-value is calculated as the probability of obtaining a placebo estimate that is greater than the actual estimated treatment effect (less than it when the effect is negative), multiplied by two to approximate a two-tailed test. For spending on capital, the "treatment" line and the placebos correspond to the average charter expansion effect on districts' per pupil capital expenditures between 2012 and 2015.

Table 2
2SLS Estimates of Fiscal Spillovers

		Per Pupil Revenue					Per Pupil Expenditures on:						
	Charter Payments (log) (1)	Temp Refund (log) (2)	Fed Rev (log) (3)	State Net Rev (log) (4)	Local Rev (log) (5)	Total Net Rev (log) (6)	Total Exp (log) (7)	Capital (8)	Fixed Costs (log) (9)	Instruction (log) (10)	Salaries (log) (11)	Support (log) (12)	Student-to-Teacher Ratio (13)
Charter share	15.04**	30.71**	0.46	–3.79***	0.39	–0.36	–0.21	–24.1**	1.18	2.27***	1.23***	–1.48*	–9.69
	(5.96)	(10.05)	(2.42)	(1.28)	(1.51)	(0.89)	(0.75)	(10.73)	(0.89)	(0.74)	(0.45)	(0.83)	(6.91)
N	209	220	238	252	224	196	238	280	210	238	238	210	165
R^2	0.819	0.561	0.771	0.734	0.908	0.844	0.845	0.211	0.804	0.907	0.926	0.930	0.819
First-stage F-stat	17.5	12.8	22.2	21.8	17.6	20.0	23.1	21.5	17.8	17.5	23.9	21.1	26.9
Mean Y ($)	598.2	144.6	1964.2	10002.5	6452.3	17965.4	17491.6	1378.1	3489.9	9848.5	8650.8	3421.4	
Scaled effect (%)	75.2	153.5	2.3	–19.0	1.9	–1.8	–1.0	–87.2	5.9	11.4	6.2	–7.4	
Scaled effect ($)	450.0	222.1	46.1	–1899.3	124.9	–325.1	–183.6	–1202.6	206.0	1118.2	532.2	–253.8	

Notes: This table reports 2SLS estimates of the charter expansion's effect on districts' per pupil revenues and expenditures. Observations are weighted using the SC weights. For all outcomes, we use the log of the variable, except for the spending on capital, and student-to-teacher ratio. "State Net Rev" refers to the state revenue minus this the temporary refund. "Total Net Rev" refers to total revenue minus payments to charter schools. The endogenous variable is the charter share, which is a continuous variable that ranges from zero to one. The specification is given by Equations 8 and 9 in the text. It uses three instruments for charter share: (i) the interaction between a post-reform years dummy and a Boston dummy (Z_{1dt}), (ii) the interaction between a post-reform years dummy and a dummy for other urban expanding districts (Z_{2dt}), and (iii) the interaction between a post-reform years dummy and a dummy for nonurban expanding districts (Z_{3dt}). All regressions control for districts fixed effects, and post-reform years. For standard errors, we use the White estimator of variance. Mean Y ($) shows the mean value of the outcome (expressed in dollars) in expanding districts in 2010 (the last pre-reform year). Scaled effect (%) reports the scaled effect, that is, the change in outcome (expressed in percent) if charter share goes up by five percentage points. This corresponds to the coefficient reported in the top row divided by 20 (and multiplied by 100 to read the statistic directly as a percentage). Scaled effect ($) shows the change in outcome (expressed in dollars) if charter share goes up by five percentage points. This statistic corresponds to the mean value of the outcome multiplied by the row "Scaled effect (%)" and divided by 100 to read the statistic directly as a percentage change in dollars. Significance: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

(as discussed below), so that districts' total per pupil revenue (net of payments to charter schools) did not go down between 2011 and 2015 (Figure 4C).[36]

We use placebo inference to evaluate the probability that these effects are due to chance. Figure 5 reports the estimated first-stage and reduced-form effects for expanding districts (line "Treatment") and the placebo effect for each placebo district. The *p*-value reported on each graph calculates the probability of obtaining an estimate at least as large as the one obtained for the expanding districts when the treatment is reassigned at random. Figure 5A shows that charter expansion was significantly larger in expanding districts than in any other group of placebo districts. Similarly, the increase in payments to charter schools has a *p*-value of 0.06, suggesting that the positive effect is significant.

The IV-DiD estimates, reported in Table 2, show similar results. This table shows IV-DiD estimates in which observations are weighted using the SC weights, while Online Appendix Table A.5 shows unweighted results. As expected, the weighted and unweighted results are very similar for almost all outcomes that have good parallel pre-trends (Figure 3). The results differ more when pre-trends do not look as good in the unweighted comparisons of expanding versus SC districts, for instance, for the transfers to charter schools and spending on capital. Due to the better trends when applying the SC weights, we consider the weighted IV-DiD results as more reliable and therefore focus the discussion on these results.

B. Role of the Temporary Refund

Columns 1 and 2 suggest that increasing the charter share by five points, or a 100 percent rise from the pre-expansion value, would (to a linear approximation) increase districts' transfers to charter schools by 75.2 percent (or $450 per student) and the "other state revenue" category, which includes the temporary refund, by 153.5 percent (or $222 per student), both on a per pupil basis.[37] These estimates suggest that the aid offsets about 49 percent of districts' transfers to charter schools. We also find no effect on per pupil revenue net of transfers to charter schools (Column 6).[38] These results confirm that, as expected in Massachusetts, charter expansion triggers larger payments to charter schools but also additional aid from the state.

Our results are in line with previous findings from Bifulco and Reback (2014), who present case studies of the financial adaptation of traditional public schools to enrollment declines in Albany and Buffalo, NY, where an aid program similar to the

36. The value of the coefficients is displayed in Figure 4. The reported coefficient corresponds to the mean difference, in 2015, of the log of per pupil expenditures in expanding districts and in synthetic control districts. Taking the exponential of these coefficients gives us the percentage change in the absolute value of the outcomes. The increasing trends in per pupil expenditures between 2000 and 2010 are partly due to declining enrollment trends during that period.

37. We should be very cautious when comparing changes in revenue and expenditures expressed in dollars per student. The gains and losses we document might not add up to zero because the synthetic control group varies across outcome.

38. Online Appendix Table A.6 shows that the first-stage coefficients for each of the three instruments are significant.

one in Massachusetts exists.[39] They find that the transitional aid offsets between 18 and 88 percent of the estimated negative impact, depending on the district and the scenario. Our approach complements Bifulco and Reback (2014) by capturing both direct and indirect fiscal effects. By indirect, we mean that beyond the direct effect of charter expansion on district enrollment and revenue, districts can also adjust and react to changes in enrollment, for instance, by reducing some of their costs or by experiencing changes in where their revenue is coming from. These adjustments are important to account for when looking at medium- and long-term effects of charter school expansion as we do in this paper.

C. Effect of Charter Expansion on Sources of Revenue

While the existence of a refund should mechanically increase per pupil revenue (as revenue remains constant, at least for the first year, whereas the number of students goes down), the overall null effect of charter school expansion we find on districts' per pupil revenue suggests that other sources of revenue may be adjusting to charter expansion. This has been found in other contexts, for example, local revenue decreasing because of a drop in property prices when a charter school enters a market (Cook 2018).

This leads us to document next how different sources of revenue respond to charter expansion. Table 2 shows that charter share has no significant effect on federal and local revenues, but has a significant effect on state revenue net of the temporary refund. A five point increase in charter share would lead to a 19 percent reduction in state revenue net of the temporary refund. Local revenue is by far districts' main source of revenue, as shown in Table 1. Total per pupil revenue is equal to $16,000 in Massachusetts, decomposed as $10,000 from the local sources, $5,000 from the state, and less than $1,000 from federal sources. The null effect on local revenue is interesting, as it contrasts with the finding of Cook (2018) that charter competition decreases traditional public school revenues raised through property taxes by depressing district property values.[40] The reduction in the state revenue that is unrelated to the temporary refund suggests that states might reduce some of their noncharter related funding as a result of charter expansion. We cannot, however, investigate the source of this reduction due to a lack of more detailed data.

D. Effect of Charter School Expansion on Per pupil Expenditures

Constant per pupil revenue could still be consistent with fiscal stress if districts' per pupil expenditures are increasing more than their revenue. This typically happens

39. Bifulco and Reback (2014) explain that New York State provides districts with increasing charter school enrollments transitional aid meant to reduce fiscal impacts on the district. The aid is computed as "80 percent of the payments attributed to increased charter school enrollment during the last year, 60 percent of payments attributed to increases in charter school enrollments two years earlier, and 20 percent of the payments attributed to increases in charter school enrollments three years earlier."

40. Although nonsignificant, the small positive effect on per pupil federal revenue is in line with concerns about charter schools not enrolling difficult-to-educate students, such as students needing special education or English learners (Bergman and McFarlin 2020; Setren 2019). Traditional public schools receive additional Federal revenue to help educate these students (such as Title I for low-income students and IDEA for special education students), which might boost per pupil revenue when enrollment drops.

when districts cannot reduce their expenditures proportionally to the revenue they transfer to charter schools. As soon as districts face fixed costs that are difficult to adjust over the short term, the amount that districts pay to charter schools (the average cost of educating a student) would be larger than the amount they save by losing a student (the marginal or variable cost of educating a student).

This is not what our next result suggests. We find that total per pupil expenditure remained constant over the 2011–2015 period. The fact that per pupil expenditure did not increase more than per pupil revenue suggests that charter expansion did not have a negative fiscal impact on district finances, a result that stands in contrast with prior studies and might be partially due to the temporary refund scheme (Arsen and Ni 2012; Ladd and Singleton 2020; Cook 2018).

E. Effect of Charter School Expansion on Resource Allocation

After having looked at *whether* total per pupil expenditures change, we now investigate *how* resources are allocated by looking at schools' per pupil spending on capital outlay, fixed costs, instruction, salaries, and support services.

Charter expansion might lead to a reallocation of per pupil spending in two ways. First, faced with competition for their students, schools might change how they spend limited resources in order to retain existing students and attract new ones. Because parents factor school ratings information into student enrollment decisions (Hastings and Weinstein 2008), charter competition could create incentives for traditional public schools to reallocate resources in a way that boosts student achievement (Hoxby 2003). The existence of transitional aid might give some traditional public schools more leeway to adjust to competition in this way. Similarly, districts that face large charter school expansion and uncertainty on student enrollment in future years might be more reluctant to invest in capital, spend on construction, or purchase new land.

Second, several sources of expenditures are fixed over the short term, so that a reduction in the number of students would mechanically increase these per pupil expenditures. For instance, teachers are not easy to dismiss from one year to the next. Even over the medium run, a large share of teachers are unionized, which can hamper schools' ability to reduce their salaries. Finding that increased spending on fixed costs is the only reason why per pupil expenditure rises would be a concerning result. It would imply that schools' inability to scale down fixed expenses forces them to cut down on variable costs to maintain their total expenses constant. Our results only partly support this story.

Consistent with the reallocation story, we find that the constant per pupil expenditure is not divided proportionally between all types of expenditures. Moving from 7 percent to 12 percent of students attending charter schools (which is the average jump for expanding districts four years after the reform) would increase per pupil expenditure on instruction by 11.4 percent (or $1,118), on salaries by 6.2 percent (or $532), and on fixed costs by 5.9 percent (although this last effect is not statistically significant). In contrast, per pupil expenditure on capital and on support services would go down (by, respectively, 87.2 percent—equivalent to $1,202—and 7.4 percent).

The large negative effect on capital spending is striking. These expenses correspond to districts' spending on construction and on purchase of land and of existing structures. Districts' reluctance to invest in capital might be a plausible reaction to lower expected future enrollment or more uncertain future enrollment caused by charter expansion.

In contrast, the positive effect on spending on instruction is also interesting. The $1,118 per student increase almost compensates for the $1,202 drop in capital outlay expenditures. Spending on instruction corresponds to expenditures for interactions between teachers and students in the classroom, as well as cocurricular activities. They cover activities of teachers but also of instructional aides or assistants. While increasing these expenditures might be a source of student progress, cutting spending on support services might be detrimental to students' attainment, in particular, when cuts target pupils' support, such as guidance, health, and psychological support, or social work (Carrell and Hoekstra 2014; Carrell and Carrell 2006; Reback 2010).[41]

Our finding that per pupil spending on fixed costs does not increase is slightly surprising. Two reasons could explain this. First, we used a fairly restrictive definition of fixed costs, which omits sources of expenses that might be sticky over the short or medium run (such as teacher salaries).[42] Including part of teacher salaries in the fixed costs would most likely lead to a positive effect of charter expansion on fixed costs.

We cannot fully isolate what share of the resource reallocation is due to schools' strategic reaction to competition and what share is due to the semimechanical increase in per pupil spending on fixed costs, but the latter effect is unlikely to drive all our results, as we find that schools do cut down on some expenditures, such as support services and capital. This suggests that some schools actively reallocate their resources instead of passively experiencing mechanical changes. Furthermore, even if schools' inability to reduce fixed costs (including teachers) does drive part of our results, this mechanism is interesting, as it suggests that the number of students per teacher, one of the determinants of student achievement, would go down.[43] We test this by looking at the effect of charter school expansion on the student-to-teacher ratio and find that it falls in expanding districts, but not enough for the effect to be statistically significant.

Given these effects on per pupil spending, a natural question is whether they translate into achievement consequences for noncharter students. In fact, a large literature has linked per pupil expenditures and student achievement in other contexts (Jackson, Johnson, and Persico 2016; Lafortune, Rothstein, and Whitmore Schanzenbach 2018; Hyman 2017; Card and Payne 2002; Hoxby 2001). We therefore turn now to the achievement spillovers of charter school expansion.

41. Carrell and Hoekstra (2014) use within-school variation in the number of counselors in elementary school in Florida. They find that an additional counselor significantly increases achievement (particularly for boys) and reduces misbehavior by 20 percent for boys and 29 percent for girls. Using a similar identification, Carrell and Carrell (2006) find that lower student-to-counselor ratios reduce student disciplinary problems, in particular, for minority and low-income students. Reback (2010) exploit discontinuities in Alabama's elementary school counselor subsidies to show that additional counselor subsidies reduce the likelihood of disciplinary incidents (such as weapon-related incidents and student suspensions). They do not find any effect on student educational outcome, however.

42. Our definition of fixed costs contains expenditures on plant operation and maintenance, student transportation, school administration and general administration, interest on debt, and some business-related support services (see Online Appendix B for more details).

43. Numerous studies have demonstrated the positive effect of smaller class sizes on student achievement (Krueger 1999; Angrist and Lavy 1999; Hoxby 2000; Urquiola 2006; Fredriksson, Öckert, and Oosterbeek 2013) and on longer-term outcomes, such as the probability of taking the ACT and SAT exams or being enrolled in college (Krueger and Whitmore 2001; Chetty et al. 2011).

VI. Education Spillover of Charter School Expansion

We now turn to estimating the spillover effects of charter expansion on student achievement in traditional public schools. This presents additional challenges due to selection bias in who attends charter schools. In this section we describe how we overcome this problem using admissions lotteries as instruments for charter attendance and how we combine this with our SC and IV-DiD approach to estimate the spillover effects. The next section presents our results.

A. Identifying Spillovers in the Presence of Selection Effects

We start from the following potential outcomes framework. Let the test score Y_{idt} of a student i in district d at time t be given by

$$(10)\quad Y_{idt} = c_{idt}Y^1_{idt} + (1 - c_{idt})Y^0_{idt}$$

where $c_{idt} = 1$ if the student attends a charter school and zero otherwise.Y^1_{idt} and Y^0_{idt} are the student's potential outcomes from attending a charter and noncharter school, which we model as:

$$(11)\quad Y^1_{idt} = \alpha + \beta_{it} + \gamma' X_{idt} + u_{dt} + \epsilon^1_{idt}$$

$$(12)\quad Y^0_{idt} = \alpha + \rho C_{dt} + \gamma' X_{idt} + u_{dt} + \epsilon^0_{idt}$$

where C_{dt} is the proportion of students in d who attend a charter school at t, that is, the district-by-time average value of c_{idt}. X_{idt} is a vector of demographic characteristics. u_{dt} and ϵ_{idt} are error terms that reflect the influence of unobserved district-level and individual-level factors, respectively, on achievement. In this framework, charter expansion has two effects on achievement: a spillover effect ρ on noncharter students, through raising C_{dt}, and a direct effect β_{it} for those additional students who attend charter schools as a result of the expansion.

Estimating ρ as the effect of expansion (C_{dt}) on the average test scores of noncharter students would suffer from selection bias because nonrandom student selection into charter schools changes the average potential achievement of those left behind. For example, if charter schools enrolled the least able students in a district first, then noncharter students would become more positively selected as C_{dt} increases (the charter sector expands), and average noncharter test scores would be positively correlated with C_{dt} even when $\rho = 0$. In our potential outcomes framework, we can represent this selection bias as ϵ^0_{idt} being correlated with C_{dt} conditional on $c_{dt} = 0$.

Ample evidence confirms that charter school students are in fact selected on achievement and that this selection changes as charter schools expand. Looking at charter schools that expanded in Boston after the 2011 cap reform, Cohodes, Setren, and Walters (2021) observed that expanded charters attracted a more disadvantaged, lower-achieving population. The authors suggest that this pattern may reflect the changes in recruitment practices required by the 2010 Achievement

Gap Act, which mandated that charter schools take steps to enroll higher-need students.[44]

Such dynamic selection of students into charter schools implies that students in traditional public schools are also an increasingly selected sample as charter schools expand, creating the identification problem above.

We develop a novel approach to address this identification problem, by estimating a spillover effect of charter expansion on *all* students while controlling for the private returns to charter schools. Specifically, by substituting Equations 11 and 12 into Equation 10, we get

$$(13)\quad Y_{idt} = \alpha + \rho C_{dt} + \tilde{\beta}_{idt} c_{idt} + \gamma' X_{idt} + u_{dt} + \epsilon^0_{idt}$$

where $\tilde{\beta}_{idt} := \beta_{idt} - \rho C_{dt}$, and $\epsilon_{idt} = c_{idt}\epsilon^1_{idt} + (1 - c_{idt})\epsilon^0_{idt}$.

This demonstrates that a spillover effect only on noncharter students (ρC_{dt}) can be modeled as a spillover effect on all students after controlling for a heterogenous direct effect of charter attendance $\tilde{\beta}_{idt}$. We then aim to estimate ρ by estimating Equation 13 using sources of exogenous variation in c_{idt} and C_{dt}.

B. Estimation Approach

Equation 13 is a typical peer effects specification, as in Angrist (2014) or Acemoglu and Angrist (2000), where one regressor (C_{dt}) is the group average of another (c_{idt}). There are three important requirements to consistently estimate the effect of C_{dt} in this context. First, we must deal with endogeneity arising from the correlation between C_{dt} and district-specific trends in achievement u_{dt}. To do so, we use the 2011 expansion reform and the SC and IV-DiD methods developed above. We describe further below how we adapt them to this context.

Second, we must accurately estimate the causal effect of charter attendance $\tilde{\beta}_{idt}$ for the students affected by expansion. We use an instrumental variable strategy in which our instrument is whether a student received an offer in any charter school's admissions lottery. Receiving such an offer is random conditional on the portfolio of schools applied to and the year of application.[45]

Third, we must control for *heterogeneity* in the causal effect of charter attendance $\tilde{\beta}_{idt}$ for two reasons. When the spillover ρ affects only noncharter students, this effectively introduces district-by-time-level heterogeneity related to charter expansion into the causal effect of charter school attendance, as Equation 13 above shows. In addition, because district-level charter enrollment (C_{dt}) is the mean of individual enrollment (c_{idt}) at the district-by-year level, the instrument we use for C_{dt} is likely to impact both

44. In other contexts, using data from National Alliance for Public Charter Schools, Epple, Romano, and Zimmer (2015) find that the proportion of students eligible for free or reduced-price lunch (FRL) in charter schools has grown markedly over time, from roughly 30 percent in 2001 to 50 percent in 2010. Baude et al. (2014) use data from Texas and find that student selection into charter schools moved from being negative in 2001 in mathematics and reading to roughly neutral in 2011.

45. A substantial prior literature has used these admissions lotteries to estimate charter school effectiveness (Hoxby and Murarka 2007; Angrist et al. 2010, 2016; Dobbie and Fryer 2011, 2016; Abdulkadiroğlu et al. 2011, 2016; Angrist, Pathak, and Walters 2013; Carlson and Lavertu 2016; and Cohodes, Setren, and Walters 2021), and we refer the reader to this literature for further details of the approach.

the individual and the aggregate enrollment into a charter school, a potential violation of the exclusion restriction, unless the "lottery compliers" have the same local average treatment effect (LATE) of charter attendance as the "expansion compliers" (Acemoglu and Angrist 2000). In other words, students who are induced to attend charter schools by winning the charter lottery must have a similar treatment effect of enrolling in a charter school to students who are induced to attend charter schools as a result of charter expansion in their district. This might not be true if charter school effects are heterogeneous. In particular, charter effectiveness may have increased following the 2011 reform, so that the treatment effect for the expansion compliers would be larger than for others. Expansions in districts close to the 9 percent limit on charter funding were limited to proven providers, that is, existing charter schools or boards of governors with track records of high performance. Consistent with this, Cohodes, Setren, and Walters (2021) find that, despite attracting a more disadvantaged, lower-achieving population, post-expansion charter schools in Boston produced larger effects than other charter schools before the reform.

To address concerns about heterogeneous treatment effects, our specification allows for the effectiveness of charter schools to differ after the reform (relative to before). We also allow for urban charter schools to have different treatment effects to nonurban ones.[46] As far as we know, this is the first paper to account for both changing student selection into charter schools and higher-performing charter schools' selection into expansion.

1. Synthetic control estimation

Achievement and charter attendance are individual-level variables, but our SC approach is designed for district-level outcomes. This motivates a two-step approach to estimate Equation 13 and isolate district-level achievement variables. In the first step, we run the following regression at the student level:

$$(14)\quad Y_{idt} = \alpha + \beta_0 c_{idt} + \beta_1 c_{idt} \cdot Urb_{it} + \beta_2 c_{idt} \cdot Post_t + \gamma X_{it} + \mu_{dt} + e_{idt}$$

where Y_{idt} is the test score of student i in district d and year t, c_{idt} is a dummy for individual charter enrollment, and e_{idt} is an error term. $c_{idt} \cdot Urb_{it}$ and $c_{idt} \cdot Post_t$ are dummies for enrollment in an urban charter school and enrollment in a charter school after the 2011 reform. We instrument the endogenous variables c_{idt}, $c_{idt} \cdot Urb_{it}$ and $c_{idt} \cdot Post_t$ by O_{idt}, $O_{idt} \cdot Urb_{it}$ and $O_{idt} \cdot Post_t$, where O_{idt} denotes receipt of an offer in any charter school's admissions lottery. X_{it} is a vector of controls, including student demographic characteristics (sex, race, special education, limited English proficiency, subsidized lunch status, and a female–minority interaction term), and dummy variables for students' application portfolios interacted with year of application. μ_{dt} is a full set of district-by-year fixed effects that capture the remaining variation in test scores, once we have accounted for charter effectiveness and student demographics.

46. A large body of evidence suggests that urban charter schools generate large academic gains for lottery applicants (Hoxby and Murarka 2007; Dobbie and Fryer 2011; Abdulkadiroğlu et al. 2011; Angrist et al. 2016).

In the second step, we take the estimated fixed effects $\hat{\mu}_{dt}$ from Equation 14 and use these as the outcome variable in our SC analysis. The SC approach exactly parallels that in the estimation of fiscal spillovers, so we do not describe it again here.

Online Appendix Table A.9 reports first-stage and second-stage estimates of the private return to charter schools. The second-stage coefficients confirm that charter schools produce larger gains in urban areas than in nonurban areas. Charters also appear to be more effective after the cap reform than before. This is consistent with what Cohodes, Setren, and Walters (2021) find in Boston.

2. IV-DiD estimation

As we did for the fiscal spillover, we complement the SC approach with an IV-DiD method. One might be concerned that charter schools locate in districts that have experienced decreasing or increasing achievement trends (Imberman 2011). This makes the complementarity between IV-DiD (that assumes parallel pre-trends in outcomes) and SC (that imposes parallel pre-trends) particularly valuable. Combining the two methods gives us the opportunity to check that they yield similar results despite being based on different pre-trends assumptions.

Our IV-DiD regressions use the same outcome variable as our SC analysis above, that is, the estimated district-by-time fixed effects $\hat{\mu}_{dt}$ from Equation 14. The IV-DiD estimation approach with this outcome variable then exactly parallels the approach used in the estimation of fiscal spillovers described in Section IV.E, so we do not describe it again here. As with the fiscal spillovers, we report results both weighted and unweighted with the SC weights.

3. Comparison with alternative approaches and robustness

An alternative approach to identification in this context would be to make a selection-on-observables assumption that the increased selection into charter schools induced by charter expansion is a function of observable characteristics, for instance, pre-expansion test scores. With this assumption, we could then estimate the spillover effect as the effect of charter expansion on average noncharter achievement controlling for these factors.

The key advantage of our approach relative to this alternative is that we do not need to assume selection on observables. If, as may be likely, charter students are selected on unobservable characteristics that affect achievement, then the alternative selection-on-observables approach would not remove selection bias from our estimates. In contrast, our lottery instrument is robust to selection on unobservables.

The key disadvantage of our approach is that it may not capture all relevant heterogeneity in the individual causal effect of charter enrollment. In particular, students attending nonlottery charters may have different effects of attendance, as some previous studies suggest (Angrist, Pathak, and Walters 2013), which we cannot directly estimate with our lottery-based approach. One may also be concerned that the dimensions of heterogeneity controlled for above might not capture all relevant dimensions within the lottery sample.

To address these concerns, in Online Appendix E we consider two alternative estimation approaches as robustness checks. The first approach allows for the direct causal effects of charter attendance to differ along more and different dimensions of heterogeneity but is otherwise the same. In the second approach, we instead adopt a more standard selection-on-observables assumption as described above. Specifically, we estimate the effect of charter expansion on average noncharter sector achievement after controlling for students' pre-expansion test scores. Both approaches produce similar results. We view the similar results from the second approach in particular, which relies on very different identifying assumptions, as especially reassuring.

VII. Results on Education Spillover of Charter Expansion

A. Results

We start by reporting results from the SC method—that estimates the effect of the cap lift on students achievement in expanding districts compared to the synthetic control districts. We then report results from the 2SLS specification that estimates the effect of a 1 percent increase in the charter share on noncharter students achievement.

Figure 6 summarizes the SC results. By 2015, achievement in expanding districts has increased by 0.099 standard deviations in math and by 0.035 standard deviations in ELA, as compared to the synthetic control. Placebo inference, shown in the last row of Figure 6, shows that these test score gains are not statistically different from gains in placebo districts.

Table 3 reports the IV-DiD results (weighted with the SC weights). They show very similar results that are statistically significant. We find that moving from 10 percent to 15 percent of students attending charter schools, which is the average post-reform increase in middle school, would raise noncharter student achievement by 0.11 standard deviations in math and 0.05 in ELA, although this last effect is not statistically significant. Figure 7 reports pre-trends in student achievement. Although some look more parallel than others, as for the fiscal outcomes, we prefer not to include group-specific time trends in our specification, following evidence from Borusyak et al. (2024) that group-specific trends introduce underidentification problems. In addition, the results unweighted by the SC weights, reported in Online Appendix Table A.7, are extremely similar for both math and ELA.[47]

B. Consistency with Previous Studies

Our results accord with previous studies showing charter expansion has limited impact on traditional public school achievement. Evidence from New York City (Winters 2012; Cordes 2018) and Florida (Sass 2006) suggests mildly positive and sometimes significant effects on achievement. In contrast, Bettinger (2005) finds that charter expansion has a negative and significant, but very small, effect in Michigan. However, this paper primarily focuses on elementary schools rather than middle schools, which may explain the discrepancy. Imberman (2011) also finds significant negative effects for elementary schools but an insignificant positive effect, of similar size to our own results, for middle and high schools.[48] Interestingly, the negative results in the literature tend to occur in

47. A potential concern with the weighted result for math is that the first-stage *F*-statistic falls below 10 (it is 8.8). Reassuringly, the *F*-statistic is higher for the unweighted result (10.7) and the coefficient estimate is almost identical (2.22 vs. 2.21).

48. For additional references on charter schools' effect on noncharter students, see Hoxby (2003), Booker et al. (2008), Zimmer and Buddin (2009), Davis (2013), Jinnai (2014), Mehta (2017), Cremata and Raymond (2014), Zimmer et al. (2009), Sass (2006), Bifulco and Ladd (2006).

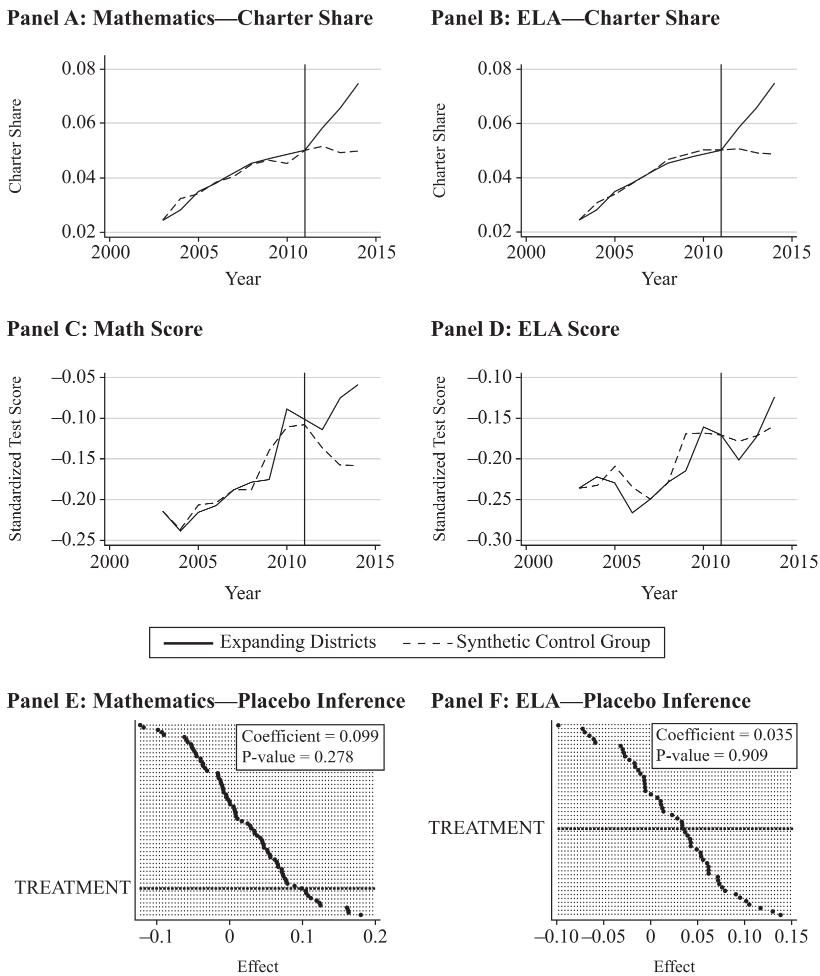

Figure 6

Charter Share and Students' Achievement

Notes: This figure plots the share of students attending a charter school (Panels A and B) and students' average math and ELA test scores (Panels C and D). The plain lines represent districts that saw an increased share of students attending a charter school after the 2011 reform (expanding districts), and the dotted lines represent the synthetic control districts. To control for student selection into charter schools, the test-score outcome variables are the district–time fixed effects from a regression of students' raw test scores on a set of students' demographic characteristics and a dummy for individual charter enrollment (instrumented by receiving a charter lottery offer). Panels E and F plot the distribution of charter expansion's impact on student achievement, as measured by the synthetic control method. The "treatment" lines report the coefficients when expanding districts are compared to their synthetic control districts. The exact value of each coefficient is reported in the top right corner of each figure. The other lines in the figures report the coefficients when a placebo group of nonexpanding districts is compared to its identified group of synthetic control districts. The *p*-value is calculated as the probability of obtaining a placebo estimate greater than the actual estimated treatment effect (less than it when the effect is negative), multiplied by two to approximate a two-tailed test.

Table 3
2SLS Estimates of Charter School Expansion's Impact on Achievement

	Math		ELA	
	First Stage (1)	2SLS (2)	First Stage (3)	2SLS (4)
Charter share		2.21**		1.06
		(0.93)		(0.84)
Post-reform * Boston	0.0411***		0.0427***	
	(0.0100)		(0.0099)	
Post-reform * Other urban	0.0136***		0.0151***	
	(0.0039)		(0.0038)	
Post-reform * Nonurban	0.0114**		0.0130**	
	(0.0052)		(0.0052)	
N	204	204	228	228
R^2		0.582		0.763
First-stage F-stat	8.8		10.5	

Notes: This table reports first-stage and 2SLS estimates of charter expansion's effect on student achievement. To control for student selection into charter schools, the outcome variable is the district–time fixed effects from a regression of students' test scores on a set of students' demographic characteristics and a dummy for individual charter enrollment (instrumented by receiving a charter lottery offer). We then use 2SLS to estimate the effect on this outcome of the charter share, which is a continuous variable that ranges from zero to one. Regressions are weighted using the synthetic control weights identified for these outcomes. The specification is given by Equations 8 and 9 in the text. It uses three instruments for charter share: (i) the interaction between a post-reform years dummy and a Boston dummy (Z_{1dt}), (ii) the interaction between a post-reform years dummy and a dummy for other urban expanding districts (Z_{2dt}), and (iii) the interaction between a post-reform years dummy and a dummy for nonurban expanding districts (Z_{3dt}). Columns 1 and 3 show the first-stage effects of these instruments on the charter share, and Columns 2 and 4 show the effect of the charter share on residualized test scores as estimated by 2SLS. All regressions control for district fixed effects and post-reform years. For standard errors, we use the White estimator of variance. Significance: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

settings with little funding available to compensate public schools when the charter sector expands. This was the case for Bettinger's (2005) setting in Michigan and could explain why his findings differ from ours. The positive effects in New York, meanwhile, come from a context similar to our own of increasing per pupil funding as charters expand (Cordes 2018). Similarly, using data from North Carolina and Massachusetts, Slungaard Mumma (2022) finds no effect of charters on the achievement of students in the traditional public schools, but a positive effect on test scores for academically focused charters.

C. Discussion of Potential Mechanisms

At first sight, the somewhat limited effect of charter school expansion on student achievement might be slightly surprising given the increased per pupil spending on

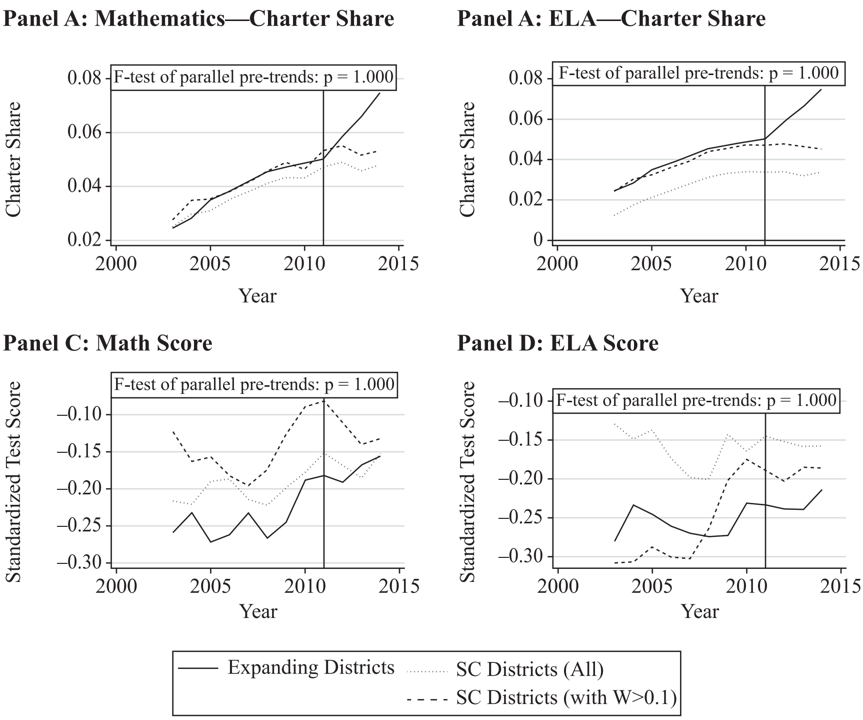

Figure 7

Trends in Student Achievement

Notes: This figure plots the share of students attending a charter school (Panels A and B) and students' average math and ELA test scores (Panels C and D). The plain lines represent the expanding districts, and the dotted lines represent synthetic control districts. The lines plot the average test score in these districts without using the weights defined by the synthetic control method.

instruction observed. Yet, the evidence on the effects of school spending on academic outcomes is mixed.[49] In addition, it often takes time before additional spending starts to have an effect on student achievement. Jackson, Johnson, and Persico (2016) find that, for low-income children, a 10 percent increase in per pupil spending each year for all 12 years of public school is associated with 0.46 additional years of completed education. Similarly, Lafortune, Rothstein, and Whitmore Schanzenbach (2018) find that, following the adoption of school finance reforms, changes in achievement trends cumulate over subsequent years, so that "ten years after a

49. Early observational studies found additional funding had small or zero effects (Coleman et al. 1966; Hanushek 2003). Numerous papers later documented the relationship between student achievement and school spending by exploiting school finance reforms (SFRs). Card and Payne (2002) find that court mandated SFRs reduce SAT score gaps between low- and high-income students. Hoxby (2001) finds increased spending due to SFRs has mixed effects on high school dropout rates. Guryan (2003), Papke (2005), and Hyman (2017) find that reforms improved test scores and college attendance in low-income districts in Massachusetts and Michigan.

reform, relative achievement of students in low-income districts has risen by roughly 0.1 standard deviation." Our identification only allows us to measure outcomes up to four years after the 2011 reform, but consistent with the two aforementioned studies, we find larger long-term effects than short-term effects in the next section.

Teacher transition from traditional public schools to charter schools is unlikely to drive the effect on student achievement because teacher transitions from traditional public schools to charter schools are fairly rare, in part because salaries are higher in traditional public schools than in charters. Cohodes, Setren, and Walters (2021) examine the composition of Boston charter schools before and after the 2011 expansion. They show that 66 percent of the expansion charter teachers in Boston have less than one year of experience teaching in Massachusetts public schools. Finally, given the small and insignificant results we find on class size, this mechanism is unlikely to have large effects on student achievement.

VIII. Spillover Effects After the End of the Temporary Aid

Because the Massachusetts reimbursement funding scheme is only temporary, a natural question is what happens after the end of the refund period? We used charter school openings prior to 2011 to investigate charter expansion's long-term, post-reimbursement consequences. Distinguishing the short-term and long-term effect is also important because the short-term effect of expansion that we have discussed so far captures both any competitive effect of charter schools on traditional public schools and the effect of a short-term increase in funding due to Massachusetts' refund scheme. Separating these effects is useful from an external validity perspective—what would we expect in states without refunds?

To provide some evidence on these mechanisms, we analyze whether the spillover effects of expansion persist after districts' temporary aid ends. The 2011 reform we use is too recent to measure post-refund effects of expansion. Therefore, we instead exploit the fact that many districts in our sample saw charter openings pre-2011 that led to large and persistent increases in the share of students attending charter schools. We identify openings that happened between 2004 and 2009 to observe the outcome for at least six years after the opening (and two years before).[50] We then estimate dynamic treatment effects of charter school openings on districts' current and future charter share and outcome variables. Because charter openings are staggered across districts, we use the estimator developed by de Chaisemartin and D'HaultfŒuille (2020) that accounts for the potential heterogeneity of the treatment effects across districts or over time.[51]

50. We discard districts that had two or more consecutive openings to isolate the post-refund effect of a single opening. None of the charter schools considered closed.

51. The results we obtain with a standard event-study specification are almost identical, but several recent papers have raised concerns on the bias of staggered-DiD estimates when treatment effects are heterogenous over time or across treated units (de Chaisemartin and D'HaultfŒuille 2020; Athey and Imbens 2022; Goodman-Bacon 2021; Sun and Abraham 2021).

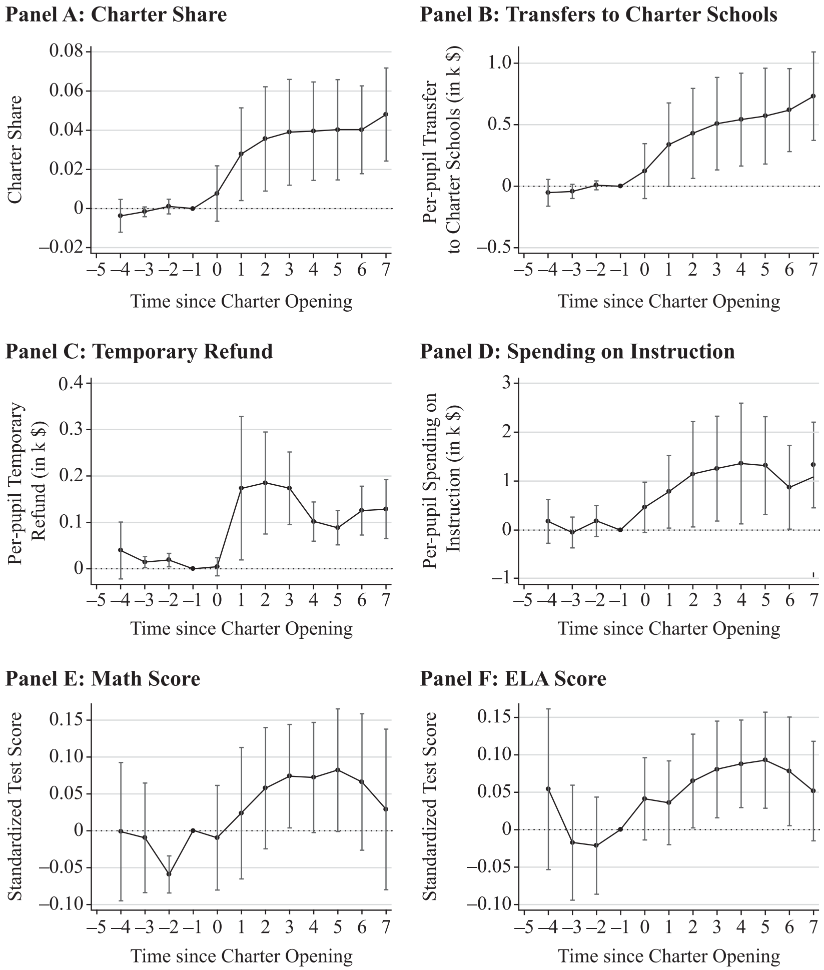

Figure 8

Dynamic Spillover Effects of Charter Openings

Notes: This figure plots the estimated coefficients of dynamic spillover effects of charter openings. The dependent variable is either the charter share (Panel A), districts' transfers to charter schools (Panel B), the temporary refund (Panel C), per pupil spending on instruction (Panel D), or students' test score in math (Panel E) or ELA (Panel F). We use the estimator developed by de Chaisemartin and D'HaultfŒuille (2020) and implemented using the did-multiplegt stata command. For each post-opening year t, the estimator compares the evolution of the outcomes in districts that saw a charter school open for the first time ("first-time switchers") and districts in which a charter school has not opened yet. The sample contains districts that saw a charter school opening between 2004 and 2009. We discard districts that had two or more consecutive openings. The effect at $t = -1$ (the last pre-opening year) is normalized to zero. Each figure plots 95 percent confidence intervals calculated from standard errors clustered at the district level. Achievement is measured as MCAS scores standardized to have mean zero and variance one in each year.

For each post-opening year t, the estimator compares the evolution of the outcomes in districts that saw a charter school open for the first time ("first-time switchers") and districts in which a charter school has not opened yet. We have two reasons to believe the results might be a good guide to longer-run causal effects. First, by including four leads of charter opening ($t = -1, \ldots, -4$), our results show no clear pre-trend for most outcomes. Second, our estimates turn out to replicate the synthetic control estimates surprisingly well over the three- to four-year time span. These two factors give us some confidence that our estimates provide useful information on the longer-run effect of charter expansion.

Figure 8 plots the estimated effect of an opening at $t = 0$ on the charter share and on outcomes at $t + k$ for $k = -4, \ldots, 7$.[52] A first interesting fact emerges. Panel A shows that charter schools increase their capacity progressively in the years that follow the opening.

It takes about four years for the charter share to rise by about four percentage points, before remaining subsequently largely flat. This means that the transfers to charter schools and the temporary refund will also be scaled over time, two results that are confirmed in Panels B and C. The temporary refund peaks in the three years that follow a charter school opening (that is, during the fast expansion years) before moving down afterwards. This pattern matches the 100/25/25/25/25/25 refund scheme that exists in Massachusetts.

A natural question is whether district spending reacts to this refund dry-off. The results in Panel D show that, in the years following the opening of a charter school, districts progressively increase per pupil spending on instruction, corresponding well to our SC and IV-DiD results. The long-term effects are directionally similar to the short-term effects, though always smaller. This suggests that the largely positive effects of charter expansion on districts tend to persist, but more modestly, or vanish past the end of the temporary refund. The distinction between the short-term and long-term effects hence partially reconciles our results with the negative fiscal spillover effect identified by previous studies, which use data from states where districts are not refunded for their charter tuition expenditures (Arsen and Ni 2012; Ladd and Singleton 2020; Cook 2018). Overall, the small long-run effect suggests that the refund cushions the short-run financial blow to districts in order to give them time to adjust to lower student numbers.

We find similar effects on achievement (reported in Panels C and D of Figure 8). The effects are positive in the short run, again corresponding well to our previous results, and largest over the three- to five-year time horizon. This fits with previous research suggesting that it takes several years for increased spending to impact achievement (Jackson, Johnson, and Persico 2016; Lafortune, Rothstein, and Whitmore Schanzenbach 2018). Interestingly, the achievement effects vanish in Years 6 and 7, after the reduction of the temporary refund. This provides suggestive evidence that the effect of charter expansion on student achievement is partly driven by the temporary injection of extra funding into the school system, in addition to any competitive effect.

52. The effect at $t = -1$ is normalized to zero.

IX. Sensitivity Tests for Synthetic Control Specifications

Identifying a group of synthetic control districts is the result of three successive choices regarding (i) the predictor variables, (ii) the method used to compute predictor variable weights, and (iii) the districts included in the donor pool. To mitigate potential concerns about specification-searching and cherry-picking, we run six robustness checks that test our main results' sensitivity to changes in each of these three choices (Ferman, de Xavier Pinto, and Possebom 2017; Kaul et al. 2017). Online Appendix E, Table A.11 details the specification used for each robustness check. For comparison, the first two rows present the baseline specification used throughout the paper.[53] Each robustness check departs from the main specification and changes one element at a time. We compare the different specifications in terms of the number of synthetic control districts identified, quality of the pre-reform fit for outcome variables and charter share (as measured by the RMSPE), and the reduced-form treatment effect estimate.

The sensitivity tests reveal that using the cross-validation method often produces lower-quality fit (as measured by larger RMSPEs) for outcome variables than the nested optimization procedure. This might be because splitting the sample introduces more noise with our limited number of years. As a result, we only used the cross-validation method when we considered that the cost in terms of fit quality was not too large, for instance, for per pupil expenditures and per pupil expenditures on instruction. Results for the other outcomes rely on the standard optimization method with four lagged values of both the outcome variable and the charter share (three for achievement and student-to-teacher ratio).

Tests on the size of the donor pool provide very similar results. If anything, enlarging the donor pool to districts whose achievement is in the lowest 25th percentile (instead of 20th for our main specification) yields larger point estimates than the ones we report in the paper. This is true for results on total spending, spending on salaries, support services, and student-to-teacher ratio. Most importantly, for most sensitivity tests we run, the effect of the reform is notably consistent across specifications.[54]

X. Conclusion

Concerns that the charter sector drains resources and high-achieving peers from traditional public schools are common. This paper provides evidence of charter expansion's fiscal and academic spillover effects in an environment in which the state temporarily compensates districts for the revenue they lose when students

53. We use the specification in the first row for results on total expenditures and expenditure on instruction. For the other fiscal outcomes and results on student-to-teacher ratio, we use the specification in the second row.

54. The sign of the effect changes a bit more for results on support services, but the three specifications that report a zero or positive effect (Equations 1, 3, and 6) have significantly worse fits for the per pupil expenditures and charter enrollment variables. We therefore would not trust these results as much as the ones we report in the paper.

move to charter schools. Our results reveal that higher charter attendance does not reduce per pupil expenditures in district schools but induces districts to shift expenditures towards instruction and away from capital (which encompasses spending on construction or purchase of land and existing buildings). We also find that the large charter expansion generated modest positive effects on achievement among students who remain in district schools.

In the longer run, and particularly after the reimbursement period ends, we find that charter expansion's positive effects on both expenditures and achievement tend to disappear (though without becoming negative). The reimbursement scheme seems to insulate districts from the short-term financial shock of charter sector expansion, allowing them to adjust over time and avoid negative effects. Taken together, our pattern of results on expenditures and test scores suggest that it is not only *how much* districts spend that matters for student achievement (Jackson, Johnson, and Persico 2016; Lafortune, Rothstein, and Whitmore Schanzenbach 2018; Hyman 2017; Card and Payne 2002; Hoxby 2001), it is also *how* they spend their money (Martorell, Stange, and McFarlin 2016; Cellini, Ferreira, and Rothstein 2010; Hong and Zimmer 2016).

Given that about half of American states have caps on charter expansion and that a growing number of states are getting close to their caps, debates over the benefits and costs of charter expansion are likely to become more frequent. While our study focuses on Massachusetts, a state with a highly regulated charter sector and a functioning and relatively generous reimbursement aid scheme, several states, including Illinois, New Hampshire, New York, and Pennsylvania, have also adopted temporary refund schemes. The Massachusetts experience is particularly relevant when evaluating charter expansion in these settings. Our findings are also relevant for states seeking to ensure that traditional public schools and their students are not harmed by the expansion of the charter sector.

Although we focus on the spillover effect of charter school expansion on traditional public schools, a more general cost–benefit analysis for both charter and noncharter schools might be useful from a policy perspective. Because of the additional expenses required by the temporary refund schemes, one might worry that charter expansion will be very costly to the state. However, this cost is partially compensated for by the fact that charter schools' revenue is on average 17 percent lower than in traditional public schools in Boston (Wolf et al. 2017). A similar reasoning applies for student achievement. Besides the small positive spillover effect we find, several papers show that urban charter schools in Massachusetts significantly boost their students' test scores, while nonurban charter schools seem to reduce student achievement (Abdulkadiroğlu et al. 2011, 2016; Angrist, Pathak, and Walters 2013). This means that a student's transition from a traditional public school to an urban charter school can potentially have a positive effect not only on their achievement but also on the achievement of students who stay in traditional public schools. The overall effect is more uncertain when students transition to nonurban charter schools.

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