Fiscal and Education Spillovers from Charter Expansion*

Camille Terrier† Matthew Ridley ‡

January 2018

Job Market Paper

Click here for the most recent version.

Abstract

The fiscal and educational consequences of charter expansion for non-charter students are central issues in the debate over charter schools. Does the charter sector drain resources and high-achieving peers from non-charter schools? This paper answers these questions using an empirical strategy that exploits a 2011 reform that lifted caps on charter schools for underperforming districts in Massachusetts. We use complementary synthetic control instrumental variable (IV-SC) and differences-in-differences instrumental variables (IV-DiD) estimators. The results suggest increased charter attendance encourages districts to shift expenditure in the traditional sector from support services to instruction and salaries. At the same time, charter expansion has a small positive effect on non-charter students' achievement.

*We are grateful to Alberto Abadie, Josh Angrist, Amy Finkelstein, Parag Pathak, and seminar participants at MIT Labor Lunch for their helpful comments. We are also grateful to Carrie Conaway, Alyssa Hopkins, Brenton T. Stewart, Hadley Brett Cabral, and the staff of the Massachusetts Department of Elementary and Secondary Education for data, suggestions, and assistance. Special thanks to Eryn Heying for excellent administrative support. Terrier acknowledges support from the Walton Family Foundation under grant 2015-1641.

†MIT, School Effectiveness and Inequality Initiative. Email: cterrier@mit.edu.

‡MIT. Email: mridley@mit.edu.

1 Introduction

Since its origins in the early 1990s, the charter school sector has grown to reach, as of 2016, more than 6,900 schools serving about 3.1 million children (National Alliance for Public Charter Schools, 2016). This rapid expansion has given rise to a large and rich literature on the effectiveness of charter schools (Abdulkadiroğlu et al., 2011; Dobbie and Fryer, 2011; Abdulkadiroğlu et al., 2016).[1] These publicly funded schools were initially conceived as a means to spur innovation in and competition for surrounding traditional public schools, yet growing concerns have emerged about charter schools' potential negative impact on non-charter students. Such concerns have had real-world effects in Massachusetts, where in November 2016 voters rejected a ballot initiative that would have added up to 12 new charter schools per year. This paper investigates the fiscal and educational impact of charter expansion on school districts by exploiting a 2011 reform that raised the cap on charter schools in Massachusetts.

Charter school policy debates often mention fiscal impacts on school districts, but causal evidence on this question is lacking (Epple et al., 2015). When a student switches from a traditional public school to a charter school, the funding follows the student. Charter schools are therefore criticized for draining resources from traditional public schools. Yet these schools often receive compensatory revenues from the state to smooth the lost revenue, which makes the net impact unclear (Arsen and Ni, 2012; Bifulco and Reback, 2014).[2] In addition, charter expansion might impact not only how much revenue traditional public schools receive but also how they spend their revenue. On one hand, if traditional public schools lose students, their per-pupil expenditures on fixed costs would mechanically go up, while per-pupil expenditures on variable costs might go down. On the other hand, increased competition generated by charter expansion might induce districts to shift resources toward productive inputs, such as spending on instruction and on teachers, while reducing spending on school administration or other support services (Hoxby, 2003).

The first objective of this paper is to isolate exogenous changes in the share of students who enroll in charter schools to assess the causal fiscal impact of charter school expansion on sending districts. By fiscal impact, we mean how charter expansion impacts (i) districts' average per-pupil expenditures on non-charter students, (ii) the share of these expenditures devoted to fixed and variable costs, and (iii) the share devoted to support services, instruction, and teacher salaries. Understanding the potential fiscal impact of charter expansion is fundamental for several reasons. First, per-pupil expenditure can be a determinant of student achievement (Jackson et al., 2016). Second, spending on fixed costs (typically building maintenance or debt interest) or support services might not generate student progress to the same extent as spending on instruction, textbooks, or teachers' salaries, which are more variable costs.

A second focal point in the debate on charter expansion relates to spillover effects on non-

[1] See also Epple et al. (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, New York. Arsen and Ni (2012) find that higher charter school enrollment levels in Michigan school districts are strongly associated with declining fund balances.

charter student achievement. (Cordes, 2017; Imberman, 2011; Booker et al., 2008). If charter schools drain resources and high-achieving peers from non-charter schools, this could harm student achievement in traditional public schools. Such a general equilibrium effect could bias estimates of charter school effectiveness. The second objective of this paper is therefore to revisit the question of charter expansion's impact on student achievement by using a novel identification strategy. As highlighted by Epple et al. (2015) in their review of the charter schools literature, studies of charter expansion's spillover effects face a number of methodological challenges that have rarely all been addressed in a single paper. Our study aims to fill that methodological gap.

A key challenge in isolating the impact of charter expansion is the non-random initial location and expansion of charter schools (Glomm et al., 2005; Bifulco and Buerger, 2015).[3] To deal with this endogeneity, we exploit a reform in Massachusetts that led to charter sector expansion. In 2011, the state raised the limit on district funding allocable to charter schools from 9% to 18% in districts with student performance in the lowest 10%.[4] In the four years following the reform, the share of students attending a charter school jumped from 7% to 12% in the districts that expanded their charter sector (hereafter termed expanding districts). The charter share remained relatively constant, at about 3%, in all other districts (hereafter termed nonexpanding districts). We use the charter sector's differential growth in expanding and nonexpanding districts as an instrument for districts' charter share.

To account for the fact that the expanding districts might be a non-random sample, we start by building a synthetic control group for these districts (Abadie and Gardeazabal, 2003; Abadie et al., 2010, 2015). This method creates a data-driven selection of control districts that closely match the expanding districts' outcome path in the years prior to the reform. One of our methodological contributions is to use the comparison between expanding districts and the synthetic control districts as an instrument for charter share. To do so, we show how to ensure that expanding and synthetic control districts have similar pre-reform trends in both expenditures and charter share.[5] Though the graphical analysis of the synthetic control method is compelling, this method has one important drawback: the small number of synthetic control districts, and the fact that only post-reform years are used to measure treatment effects, makes statistical inference difficult.

To address these limitations, we complement the synthetic control IV (IV-SC) approach by a differences-in-differences instrumental variable (IV-DiD) method. The instrumental variable is the interaction between the post-reform years and whether or not a district expands its charter segment. For nonexpanding districts, we start by re-using the group of districts identified and

[3]Charter schools tend to locate in districts where the population is diverse, expenditure per student is high, teacher costs are low, and public school achievement is relatively low (Glomm et al., 2005; Bifulco and Buerger, 2015).

[4]In addition, to ensure that only high-performing charter schools would open or extend, expansions in districts close to the 9% cap were limited to "proven providers", that is, existing charter schools or boards of governors with track records of high performance. Therefore, only a subgroup of low-performing districts, in which some proven providers submitted applications that were accepted, actually saw expansion after the reform.

[5]In other words, similar pre-reform trends are needed for both the first stage and the reduced form.

validated by the synthetic control method. We progressively enlarge this group to check the robustness of the results. In addition, combining IV-SC and IV-DiD gives us the opportunity to check that both methods yield similar results despite being based on different pre-trend assumptions. For DiD to be a viable instrument for charter share, the interaction between expanding district and post-reform years should be independent of potential outcomes, and this interaction should only affect student outcomes through its effects on the probability of charter enrollment.

Our results reveal that higher charter attendance shifts districts' expenditures towards seemingly more productive inputs. The IV-SC method shows that, after the reform, per-pupil expenditures on instruction and salaries increased more in expanding districts than in nonexpanding districts, by respectively 7.5% and 5.2%. On the other hand, per-pupil expenditures on support services drop by 4.4% more in expanding districts. The IV-DiD estimates confirm these results and provide evidence for the competition and fixed costs mechanisms. After the reform, the charter share in expanding districts increased from 7 to 12%. This rise results in a 23% increase in districts' per-pupil expenditures on fixed costs and a 12% increase on instructional costs. The competitive effects of charters are also borne out: the increase in instructional spending is accompanied by an almost equivalent (13%) reduction in per-pupil expenditures on support services.[6]

We then investigate the impact of charter expansion on student achievement. To do so, we use the same IV-SC and IV-DiD methodologies. However, looking at student achievement as an outcome raises additional challenges in terms of identification. Ample evidence shows that charter students differ from non-charter students and that this selection changes when charter schools expand (Epple et al., 2015; Baude et al., 2014; Cohodes et al., 2016). As a result, students in traditional public schools are also increasingly selected with charter expansion. Restricting achievement outcomes to non-charter students, as we do for expenditures outcomes, would bias estimations. Unlike most previous studies, we therefore estimate charter expansion's causal impact on the achievement of all students while controlling for individual charter enrollment (with time-varying effects).

Controlling for individual charter enrollment requires us to account for student selection into charter schools. We use an instrumental variable for individual charter enrollment (Angrist et al., 2010; Dobbie and Fryer, 2011, 2016; Abdulkadiroğlu et al., 2011; Angrist et al., 2013, 2016; Cohodes et al., 2016). Finally, a common concern is that charter schools locate in areas that have experienced increasing or decreasing trends in achievement (Imberman, 2011). We combine an IV-DiD, which assumes parallel pre-trends in outcomes, and an IV-SC, which imposes them, as an important robustness check.[7]

[6]These findings stand in contrast to studies that find charter expansion has negative fiscal impact on district spending (Arsen and Ni, 2012). The fiscal impact of charter schools probably depends on the existence of a state refund scheme. Massachusetts has one, but Michigan does not, which might explain the negative fiscal spillovers observed by the authors.

[7]An additional challenge to using the IV-SC method is that student achievement varies at the student level, while the SC methodology requires district-level variables to compute district weights. To move from individual to

Our results show that charter sector expansion has a positive impact on student achievement, although the effects are not always significant. In both math and English language arts (ELA), the IV-SC method indicates a small (but not significant) improvement in student achievement. The IV-DiD estimates reveal charter expansion makes a positive and significant impact on students' achievement. Our estimates suggest that moving from 10% to 15% of students attending charter schools would increase the achievement of non-charter students by 0.03 standard deviations in math and by 0.02 in ELA. These results confirm, to some extent, the findings of previous studies showing charter expansion has a limited impact on students' achievement (Bettinger, 2005; Imberman, 2011; Zimmer and Buddin, 2009; Davis, 2013; Sass, 2006; Winters, 2012).

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. In November 2016, a ballot question on whether or not to expand the charter school sector in Massachusetts drew national attention (The New York Times, 2016). Political heavyweights, such as U.S. Senator Bernie Sanders, President Barack Obama, and former Secretary of State Hillary Clinton weighed in on the initiative, and over $33 million from both sides came pouring into Massachusetts in what emerged as one of the most expensive ballot-question wars in the country. A majority of the state voted against the charter cap expansion in what is perceived nationally as a landmark decision. Given that about half of American states regulate 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.

2 Background

The Massachusetts Charter School Sector

The first Massachusetts charter schools opened in 1995, following the 1993 Massachusetts Education Reform Act, which allowed non-profit organizations, teachers, or other groups wishing to operate charter schools in Massachusetts to submit applications to the state's Board of Education. There are no for-profit charter schools in Massachusetts. Once authorized, 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. Most of the time, charter schools are exempt from local collective bargaining agreements.

district-level achievement, we extract the district-by-year variation by running a regression of student achievement on individual charter enrollment (instrumented), students' demographic characteristics, and a set of district-by-year effects that capture the remaining variation in achievement.

Massachusetts charter schools face stringent state accountability requirements. All charter schools operate under five-year charters granted to an independent board of trustees. Charter schools are therefore required to file for renewal 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, 28 approved charter schools have either closed or never even opened.

Charter school expansion

Since its origins in the mid 1990s, the charter school sector in Massachusetts has grown to 80 schools serving more than 40,000 children in the 2016-2017 school year. Charter students represent about 4.2% of the PK-12 public school population (Massachusetts Department of Elementary and Secondary Education, 2017).

This expansion has been facilitated by amendments to the numerical and net school funding caps set forth by the Massachusetts Legislature.[8] In 1997, the state adopted a 6% limit on district funding allocable to Commonwealth charter school tuition. That cap was raised to 9% in 2000.[9] In 2010, a legislative amendment to the charter school statute established the current funding cap provisions for charter schools. In districts with student performance in the lowest 10%, the 9% limit on district funding allocable to Commonwealth charter school was increased such that it would reach 18% by 2017. For all other districts, the 9% limit was unchanged. Districts are in the lowest 10% 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.

To ensure that only high-performing charter schools would open or expand, the state limited expansions in districts close to the 9% cap to proven providers, or existing operators with a strong track record.[10] 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.[11] With applications being limited to proven providers and the rigorous Massachusetts Board of Elementary and Secondary Education approval process, only a subgroup of low-performing dis-

[8] Like several other states in the U.S., Massachusetts regulates charter expansion by a system of caps. At the time of writing, 24 states have caps on the number of charter schools. Source: http://www.publiccharters.org/wp-content/uploads/2017/03/MODEL-Report_FINAL.pdf?x87663.

[9] 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 are Commonwealth.

[10] More specifically, a proven provider status was required for charter applications, in districts with the lowest 10% of student performance where additional charter enrollment would cause tuition payments to exceed 9% of the district's net school spending.

[11] For a complete definition of proven providers, see Massachusetts Education Laws and Regulations (603 CMR 1.00) related to Charter schools, section 4: http://www.doe.mass.edu/lawsregs/603cmr1.html?section=04.

tricts expanded their charter sectors after the reform.

The 9% cap was also not equally binding for all districts before the reform. At the state level, there was a large excess of demand for charter schools in 2010. At that time, almost as many students were on charter school waiting lists (26,708) as were actually enrolled in charter schools (28,422). Boston was the most seat-constrained district, as the Board of Education stopped accepting proposals for new Boston charters after expenditure reached the cap in 2008. Many districts in the lowest 10th percentile of student achievement where however far from the 9% cap.

The 2011 cap reform led to a significant increase in charter enrollment. Figure 1 shows that, in the four years following the 2011 cap increase, the proportion of students attending a charter school jumped from 7% to 12% in the low-performing districts that expanded their charter sectors after the reform (expanding districts). The charter share remained relatively constant, at about 3%, in all other districts (nonexpanding districts).[12]

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 with middle school. In middle schools, the proportion of students attending a charter school jumped from 10% to 15% in expanding districts. For that reason, we focus on middle school students when studying charter expansion effect on student achievement. We analyze fiscal spillovers for all levels however – that is primary, middle, and high schools – as the expenditure variables are not decomposed by level.

Districts Expenditures on Charter Schools and Traditional Public Schools

The vast majority of charter schools funding—about 90 percent—comes from tuition payments paid by the sending district, which is the district where a student resides. Policy debates often mention fiscal impacts of charter expansion on districts. When a student switches from a traditional public school to a charter school, the budget follows the student. Charter schools are therefore criticized for draining students and resources from traditional public schools.

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. In practice, tuition amounts are roughly equal to average per-pupil spending in the sending district. An important feature of charter school funding in Massachusetts is the availability of state programs that offset the charter tuition paid by sending districts. The state funds a charter reimbursement program called "Chapter 46 Aid" that pays a portion of district tuition costs in the six years after an increase in the number of students attending charter schools. Specifically, 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

[12] Not surprisingly, Boston is one of the districts that made the most of the raised cap to expand its charter sector. The percentage of students enrolled in a charter school went from 9% to 15% between academic year 2010-2011 and 2013-2014.

percent of this first-year amount for each of the subsequent five years. Reflecting this six-year reimbursement schedule, Chapter 46 Aid is sometimes referred to as the "100/25/25/25/25/25" formula.[13] Appendix Table A.1 presents an example that describes the timing of the Chapter 46 Aid. The second component of Charter 46 Aid is a refund for first year pupils entering public charter schools from private or home-schooled settings. To help defray this additional cost, the state fully reimburses this first year's tuition. In later years, financial responsibility for these charter students shifts back to the district. Finally, the state fully reimburses the "facilities aid" that sending districts pay to charter schools to help pay for school buildings. In the 2016-2017 school year, the total state aid amounted to \$80 million, decomposed into \$39.6 million for the 100/25/25/25/25/25 formula, \$34.4 million for the facilities aid, and \$6.9 million for former private or home-schooled students.[14]

The availability of state aid programs means that for several years after a student switches from a traditional public school to a charter school, the district's budget increases while the number of students remains the same. As a result, everything else being equal, the district's per-pupil revenue increases when students transition to a charter school. However, whether this translates into higher per-pupil expenditures is not clear. Evidence of low government spending elasticities and flypaper effects suggest that state aid programs might not translate into higher per-pupil expenditures (Feldstein, 1975; Hines and Thaler, 1995; Inman, 2008; Fisher and Papke, 2000; Gordon, 2004; Dee and Levine, 2004). Whether charter expansion increases districts' expenditures on non-charter students is therefore an open question.

Charter expansion might impact not only how much revenue districts devote to traditional public schools, but also how they spend such revenue. Here, two mechanisms might compensate each other. On one hand, if traditional public schools lose students, their per-pupil expenditures on fixed costs would mechanically go up, while per-pupil expenditures on variable costs might go down. Typical fixed costs are building maintenance or debt interest, while spending on textbooks, instruction, or teachers is usually considered to be more variable. On the other hand, increased competition generated by charter expansion might induce districts to shift resources toward inputs that are perceived as more productive in terms of student achievement (Hoxby, 2003). Typically, districts might increase spending on instruction and teachers, while reducing spending on school administration or other support services.

The first objective of this paper is to assess the causal fiscal impact of charter school expansion on sending districts. By fiscal impact, we mean how charter expansion impacts (i) districts' average per-pupil expenditures on non-charter students, (ii) the share of these expenditures devoted to fixed and variable costs, and (iii) the share devoted to support services, instruction, and

[13]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`.

[14]It should be noted that 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% and 97% of the total reimbursement. This rate dropped to 69%, 63%, and 58% in the years 2015, 2016, and 2017. 2015 is the most recent year we consider in this anaysis, so insufficient funding will only impact the last year of our sample.

teacher salaries.

Understanding the potential fiscal impact of charter expansion is fundamental for at least three reasons. First, per-pupil expenditure may be a determinant of student achievement (Jackson et al., 2016). If charter expansion has a fiscal impact, this might also affect non-charter students' achievement. Second, spending on fixed costs (typically building maintenance or debt interest) or support services might not generate student progress to the same extent as spending on more variable costs such as instruction, textbooks, or teachers' salaries. Finally, the question of charter expansion's fiscal impact on districts and traditional public schools has recently been at the center of a fierce debate. In November 2016, a ballot question to expand the presence of charter school in the state of Massachusetts drew national attention (The New York Times, 2016). Over $33 million came pouring into Massachusetts, from both sides of the debate, in what emerged as one of the most expensive ballot-question wars in the country. A majority voted against the charter cap expansion in what is perceived nationally as a landmark decision. Given that about half of American states regulate charter expansion by setting caps, we expect discussions on the potential fiscal effect of raising these caps to become more frequent in years to come.

3 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 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 districts' expenditures comes from the Annual Survey of School System Finances collected annually by the Census Bureau (U.S. Census Bureau, 2017). All school districts that provide elementary or secondary education are included in the annual survey. The data include revenue by source; expenditure by object (instruction, support service functions, salaries, and capital outlay); and information on indebtedness, cash, and investments. Importantly, a separate section indicates districts' payments to charter schools.[15] We can therefore

[15] Charter schools whose charters are held by non-governmental operators – typically all charter schools in Massachusetts – are considered beyond the scope of Census Bureau government finance statistics. In these cases,

isolate funds spent on non-charter students.

Using this dataset, we build five outcomes to examine the fiscal spillovers of charter expansion: per non-charter student districts' (1) total expenditures, (2) expenditures on fixed costs, (3) on instruction, (4) salaries, and (5) support services. For each outcome, we divide the district expenditure on non-charter students by the total number of public school non-charter students in elementary, secondary and high schools. Expenditures on fixed costs are the sum of expenditures on operation and maintenance of plants, student transportation, and interest on school debt. Expenditures on instruction correspond to expenditures for interactions between teachers and students in the classroom as well as co-curricular 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.[16] Expenditures on salaries include the salaries and wages paid by the district for all staff. These are gross salaries without deduction of withholdings for income tax, employee contributions to Social Security and retirement coverage. Finally, expenditures on support services are the sum of several sources of expenses. 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 ensompass expenditures for supervision of instruction, 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. We use district identifiers to match data on expenditures to state administrative education data for the years 2001-2002 to 2014-2015.

To estimate charter effectiveness, we match the state administrative education data with admissions lotteries from 22 charter schools that enroll middle school students (in grades 5 to 8) from the 2002-03 to 2013-14 school years. Appendix Table A.2 describes the charter schools that are eligible for the lottery instrument, as well as the years and grades of lottery. We exclude charter schools that closed, declined to participate, had insufficient records, where not oversubscribed, or served alternative students (like students at risk of dropping out). The resulting lottery sample includes 13 charter schools in Boston and 9 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 high charter share districts have significantly lower math and ELA test scores than students in low charter share districts. These differences are even starker when comparing expanding and nonexpanding districts. 66.6% of students in expanding districts are black or Hispanic, as opposed to only 15.6% in nonexpand-

school district payments to charter schools are included within school district expenditures, but the finances of the charter schools themselves are excluded from the statistics.

[16]Spending on instruction includes salaries.

ing districts. Students in expanding districts are also 49.5% 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 10th 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.

4 Methodology

One of the key difficulties in analyzing the impact of charter expansion is charter schools' non-random initial location and expansion (Glomm et al., 2005; Bifulco and Buerger, 2015).[17] To deal with this endogeneity, we exploit a 2011 cap reform in Massachusetts, which led to charter sector expansion. In the four years following the reform, the proportion of students attending a charter school jumped from 7% to 12% in the districts that expanded their charter sectors following the reform. The charter share remained relatively constant, at about 3%, in all other districts. For identification, we therefore exploit the charter sector's differential growth in expanding versus nonexpanding districts as an instrument for districts' charter share.

More formally, we identify an expanding district by looking at its charter sector growth before and after the 2011 reform. We examine how each district's charter share changes between 2002-2003 and 2014-2015. As displayed in Figure 1, we divide the full period into a pre-reform period that spans the years 2002-2003 to 2010-2011 and a post-reform period that spans the years 2011-2012 to 2014-2015. We note as $T1$ the first year of a period, TN the last year of a period, and N the number of years in the period. Then, for both the pre-reform and the post-reform periods, we calculate the slope of the charter share evolution (noted C) as follows: $\frac{(C_{TN}-C_{T1})}{N}$.

All districts for which the post-reform slope is larger than the pre-reform slope — that is, $Slope_{Post} - Slope_{Pre} > 0$ — and that are in the lowest 10th percentile of student test scores are considered to be expanding districts. The following nine districts are expanding: 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. We discard three districts from the group of expanding districts: Gateway, because it experienced a decreasing charter share before the cap reform, 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.

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

Low-performing districts that expanded their charter sectors after the reform might have different unobserved characteristics than low-performing districts that did not expand. Selection into expansion might be driven by several elements. First, low-performing districts can only increase charter enrollment if non-profit 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% limit on charter funding, only a proven provider may submit a new application.

Another factor in district decisions about whether or not to expand its charter sector is tuition. Charter schools' tuitions are determined by their sending districts' per-pupil expenditures. Given the relatively large variation in per-pupil expenditures across districts, applicants for a new charter school might consider the per-pupil expenditures of the potential sending districts when deciding where to locate the new charter school.[18] If, in addition, some districts faced increasing per-pupil expenditures before the reform, while others faced decreasing per-pupil expenditures, we might be concerned about selection on trends in unobserved characteristics. To mitigate omitted-variables bias in comparisons of expanding and nonexpanding districts, we develop a synthetic control instrumental variable approach (IV-SC), which we complement with a differences-in-differences instrumental variables (IV-DiD) estimator.

4.1 Synthetic Control Instrumental Variables (IV-SC)

We use Abadie et al. (2010) synthetic control strategy to construct a weighted average of control districts that matches the pre-charter-expansion outcome path in the expanding districts (Abadie and Gardeazabal, 2003; Abadie et al., 2010, 2015). Synthetic control outcomes in the post-reform-period provide a plausible counterfactual for the treatment group. This allows us to estimate a reduced form treatment effect. One of our methodological contributions is to adapt the synthetic control methodology to estimate a first stage as well. Our objective is to find a group of control units with a pre-reform path that is similar to that of the expanding districts, not only in terms of outcomes but also in terms of charter share (the endogenous variable). In other words, we estimate both a reduced form and a first stage.

More formally, let's consider the following structural equation in which the charter share C_{jt} is the endogenous variable:

$$Y_{jt} = \gamma_1 + \rho C_{jt} + v_{jt} \tag{1}$$

We want to estimate ρ, the effect of the charter share on our outcome of interest Y_{jt}. We cannot estimate it from equation (1) directly by OLS because C_{jt} is potentially correlated with unobserved district-specific trends v_{jt}. Therefore, we instrument C_{jt} with a dummy for expanding

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

districts, E_{jt}. The first stage and reduced-form equations are:

$$C_{jt} = \gamma_2 + \beta E_{jt} + u_{jt} \tag{2}$$
$$Y_{jt} = \gamma + \alpha E_{jt} + \xi_{jt} \tag{3}$$

where $\alpha = \beta\rho$. We use the following synthetic control procedure to estimate separately 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 $T = T_0 + T_1$.

$Y_{jt}(1)$ and $Y_{jt}(0)$ are the potential outcomes with and without treatment. The treatment effect for district j at time T_0 can be defined as:

$$\alpha_{jt} = Y_{jt}(1) - Y_{jt}(0) = Y_{jt} - Y_{jt}(0) \tag{4}$$

We are interested in estimating the vector $(\alpha_{j,T_0+1}, ..., \alpha_{i,T})$. This is the reduced form estimate of the IV-SC method. Abadie et al. (2010) show that we can identify the above treatment effects under the following model for the potential outcomes:

$$Y_{jt}(0) = \delta_t + Z_j\theta_t + \lambda_t\mu_j + \epsilon_{jt} \tag{5}$$
$$Y_{jt}(1) = \delta_t + Z_j\theta_t + \lambda_t\mu_j + \alpha_{jt} + \epsilon_{jt} \tag{6}$$

Potential outcomes depend on a common factor δ_t, a vector of observed covariates Z_j that are not affected by the intervention, a vector of time-specific parameters θ_t, a district-specific unobservable μ_j, and an unknown common factor λ_t. ϵ_{jt} is a transitory shock with zero mean. Finally, α_{jt} is a reduced-form year-specific treatment effect that is different from 0 only when $j = 1$ and $t > T_0$. The model allows the impact of unobservable district heterogeneity to vary with time, unlike standard differences-in-differences or fixed-effect specifications that impose λ_t to be constant over time. We can identify the first stage effect under the following model:

$$C_{jt} = \eta_t + Z_j\phi_t + \kappa_t\nu_j + \beta_{jt} + \xi_{jt} \tag{7}$$

The terms have the same interpretation as for the potential outcome. β_{jt} is a first-stage year-specific treatment effect that is different from 0 only when $j = 1$ and $t > T_0$.

Define a $(J \times 1)$ vector of weights $W = (w_2, ..., w_{J+1})$ such that $w_j \geq 0$ and $\sum w_j = 1$. Each possible choice of W corresponds to a potential synthetic control for the treated district.

The value of the outcome variable for each synthetic control (indexed by W) is:

$$\sum_{j=2}^{J+1} w_j Y_{jt} = \delta_t + \theta_t \sum_{j=2}^{J+1} w_j Z_j + \lambda_t \sum_{j=2}^{J+1} w_j \mu_j + \sum_{j=2}^{J+1} w_j \epsilon_{jt} \tag{8}$$

The value of the endogenous variable for each synthetic control is:

$$\sum_{j=2}^{J+1} w_j C_{jt} = \eta_t + \phi_t \sum_{j=2}^{J+1} w_j Z_j + \kappa_t \sum_{j=2}^{J+1} w_j \nu_j + \sum_{j=2}^{J+1} w_j \xi_{jt} \tag{9}$$

Finally, assume a vector of weights $(w_2^*, ..., w_{J+1}^*)$ that makes it possible, for each pre-reform year, to equalize three equations. First, the vector of weights equalizes the values of the pre-reform outcomes for the treated districts and the synthetic control. In addition, the vector of weights equalizes the values of the observed covariates Z_j of the reduced form equation for the treated districts and the synthetic control. Formally, for each period t:

$$\sum_{j=2}^{J+1} w_j^* Y_{jt} = Y_{1t} \quad \text{and} \quad \sum_{j=2}^{J+1} w_j^* Z_{jt} = Z_{1t} \tag{10}$$

The vector of weights also equalizes the values of the pre-reform endogenous variable for the treated districts and the synthetic control:

$$\sum_{j=2}^{J+1} w_j^* C_{jt} = C_{1t} \tag{11}$$

At that stage, it becomes clear that the pre-reform values of the endogenous variable should be identical (or as close as possible) in the treated districts and the synthetic control. To see why this is of first-order importance, imagine a situation where the vector of weights does not equalize the values of the endogenous variable for the treated and synthetic control. This could result in having a very good fit in outcomes between treated and synthetic control districts but a large difference in charter share. Given that charter share is a determinant of outcome, to obtain similar outcomes despite large differences in charter share, there must be other differences in unobserved predictors that compensate for the charter share gap. Finding weights that match not only the outcome variable but also the endogenous variable prevents differences in unobserved predictors between treated district and synthetic control districts. This is fundamental with respect to the independence assumption of the instrumental variable model.

If the vector of weights $(w_2^*, ..., w_{J+1}^*)$ exists, Abadie et al. (2010) show that the reduced form treatment effect α_{it} in equation 5 can be estimated by:

$$\hat{\alpha_{jt}} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt} \tag{12}$$

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

$$\hat{\beta_{jt}} = C_{1t} - \sum_{j=2}^{J+1} w_j^* C_{jt} \tag{13}$$

Considering a single treated unit and the effect of an intervention averaged over all post-intervention years allows us to omit the j and t subscripts. The IV-SC estimator of the parameter ρ in the structural equation 1 is the ratio of the reduced form estimate $\hat{\alpha}$ to the first stage $\hat{\beta}$[19]:

$$\rho_{IV-SC} = \frac{\hat{\alpha}}{\hat{\beta}} \tag{14}$$

In practice, this IV-SC estimator can be obtained either by estimating the first stage, the reduced form, and by taking the ratio of the two, or by running a weighted two-stage least squares (2SLS) regression of the post-intervention outcome variable on the post-intervention instrumented endogenous variable. In this regression, each control unit is weighted based on the synthetic control weights, while the treated unit has a weight of one.

Finally, it should be noted that the synthetic control reduced-form estimate $\hat{\alpha_{jt}}$ is unbiased whereas the IV-SC estimator suffers from the standard bias of the 2SLS estimator (although it is consistent). This bias, however, might be limited. The 2SLS bias is an increasing function of the number of instruments. By definition, when only one unit is treated, the IV-SC estimator relies on a single instrument (a dummy for treated and synthetic control districts), and the just-identified 2SLS estimator is median-unbiased.

Implementation

In practice, let X_1 be the vector of pre-reform characteristics for the treated district (that is, the expanding districts) and X_0 the matrix of the vectors of the untreated districts' pre-reform characteristics. A novelty with the IV-SC is that X_1 and X_0 should include the endogenous variable. The vector w^* is then chosen to minimize the distance $\|X_1 - X_0 w\|V = \sqrt{(X_1 - X_0 w)'V(X_1 - X_0 w)}$ where V is a $(k \times k)$ symmetric and positive semidefinite matrix that represents the weight of each predictor variable.

In practice, conditions 10 and 11 often only hold approximately. A perfect equality be-

[19]Note that this IV-SC estimator could also be interpreted as simply providing an appropriate scaling for the reduced-form treatment effect of expansion, α.

tween 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, it is standard practice to evaluate the discrepancy (or goodness of fit) by computing the root mean squared prediction error (RMSPE) as follows:

$$RMSPE = \left(\frac{1}{T0} \sum_{t=1}^{T0} (Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt})^2 \right)^{\frac{1}{2}} \tag{15}$$

Predictor Variables

We start by identifying outcome variable predictors, the most important of which is usually the lagged outcome variable because it accounts for the effects of any potentially unobserved predictor variables in pre-reform years. Including lagged outcome variable addresses concerns about omitting important unobserved predictors. Indeed, only units that are sufficiently similar in both observed and unobserved outcome variable determinants as well as in those determinants' effects on the outcome variable should produce similar outcome trajectories over extended periods of time. We therefore include five years of lagged outcomes and five years of lagged endogenous variable (charter share) as predictor variables. Concretely, for each expenditure outcome, the predictors are the expenditure variables in the years 2003, 2005, 2007, 2009, and 2011. We use the same years for the charter share variable. For the main specification, we chose not to include additional predictor variables because the synthetic districts chosen in this manner are well matched to the expanding districts' outcome. Section A presents sensitivity tests in which more and fewer lagged years are used as predictor variables, as well as when additional predictor variables are added.

Donor Pool

As a second step, we identify possible donor districts to create the synthetic control group. In order to avoid interpolation biases, it is very important to choose donor districts that have similar characteristics to the expanding districts. Specifically, since the expanding districts are all in the lowest 10th percentile of student test scores, we select similarly low-performing districts as donor districts. We therefore restrict the donor pool to districts in the lowest 10th percentile when using districts' expenditures as an outcome and districts in the lowest 25th percentile when using districts' average test score as an outcome. In addition, we drop districts that have an idiosyncratic shock to their endogenous variable (charter share). That condition is particularly important for IV-SC. 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.[20]

We must strike a fine balance between improving the match quality by enlarging the donor pool and avoiding the overfitting that often results from including too many districts in the donor pool that are dissimilar to expanding districts. Overfitting is detected when expanding districts are matched to a large number of donor districts, many of them having a very small weight. In that case, there is a high risk that some synthetic districts are part of the linear combination, despite having very different outcome determinants. Section A presents sensitivity tests in which we vary the size of the donor pool.

Predictor Variable Weights

Finally, the choice of the synthetic districts depends on the matrix of predictor variable weights V. We adopt a standard iterative optimization procedure that searches among all (diagonal) positive semidefinite V-matrices 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 et al. (2010)).

4.2 Identifying Assumptions

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 nonexpanding districts' outcomes. 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, only depends on the number of students enrolled in a charter school in its zone. As a result, interference should be limited. Competition effects from charter school expansion are also likely to be limited at the district level. Charter schools enroll the vast majority of their students from the district where they are 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 72 potential control districts. There is only a small probability that these synthetic districts are close enough to one of the expanding district to feel competitive pressure. Finally, even if expanding districts do have spillover effects on surrounding districts, this would most likely have an attenuating effect on our estimates.

Standard instrumental variable assumptions apply when using the difference in charter growth between expanding districts and synthetic control as an instrument. First, conditional on pre-reform charter expansion trends, charter share should increase more in expanding districts than in nonexpanding districts. We show evidence of this first stage in Figures 3 and 7.

[20]Formally, for each district in the donor pool, we calculate the post-reform charter expansion slope as previously defined: $\frac{(C_{TN}-C_{T1})}{N}$. We also compute all expanding districts' post-reform slope minimum. We discard two districts from the donor pool because they have larger post-reform slopes than the minimum slope of the expanding districts.

Second, conditional on districts' time-varying unobserved confounders, whether districts react to the reform by expanding their charter sector is independent of potential outcomes (independence assumption). Given the set of predictor variables we use to select the synthetic control, this independence assumption is equivalent to saying that, conditional on lagged achievement and lagged charter share, whether districts react to the reform by expanding their charter sector is independent of potential outcomes. In our context, given the risk of non-parallel trends in districts' expenditures in expanding versus nonexpanding districts, allowing time-varying district unobserved confounders is key to attenuating endogeneity from omitted variable bias. From that perspective, the IV-SC method rests on identification assumptions that are weaker than IV-DiD estimators. While the differences-in-differences model controls only for confounding factors that share a common trend, the synthetic control method allows the effect of unobservable confounding factors to vary with time.

Finally, the fact that districts reacted to the reform by expanding their charter sectors is assumed to only affect student outcomes through its effects on the probability of charter enrollment, not through any other factor or unobserved characteristic. This exclusion restriction would be verified if no other reform was adopted in 2011, or if the reforms adopted that year impacted students' 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 (Massachusetts State Legislature, 2010). We show that our results are not driven by the introduction of these new schools in the robustness checks section.

4.3 Inference

A caveat of the synthetic control method is that it does not allow us to assess the results' significance using standard (large-sample) inferential techniques. There are two reasons for that. First, our analysis is not based on a sample of the population, but on the entire population of districts in Massachusetts. Due to the absence of sampling variation, all the uncertainty surrounding the estimands is design-based. In other words, uncertainty in our context 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., 2017). Second, we only have a limited number of districts in the control pool and a limited number of post-reform periods covered by the sample. Typically, we usually have between five and ten districts in the synthetic control group and four post-reform years (from 2012 to 2015). These two reasons justify using permutation inference described in Abadie et al. (2010). Following their approach, we sequentially apply the synthetic control algorithm to every district in the pool of potential controls. Then, we compare the placebo effect with the treatment effect of the expanding district. Since none of the donor pool districts receive treatment, variation between the placebo district and its synthetic match occurs randomly. We can therefore assess the likelihood that the expanded districts' measured treatment effect is due to chance and whether the treatment effect measured for the expanding districts is larger than that for districts chosen at random.

We pay particular attention to the fit quality between each placebo district and its synthetic control. For each placebo district, the fit quality would only be good if its predictor variables' values belong to the convex hull of these predictors' values in its donor pool. In practice, the quality of the match 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 root mean square prediction error (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.

4.4 Differences-in-Differences Instrumental Variables (IV-DiD)

As a second research design, we instrument the charter share using the interaction between a post-reform-years dummy and whether a student lives in an expanding district. We control for post-reform-years and expanding districts main effects. Our strategy therefore uses a difference-in-differences instrument. The reduced-form estimate of the social return is calculated as the change in achievement between the pre- and post-reform cohorts in charter expanding districts, minus the change in achievement in nonexpanding districts.

The first-stage estimate corresponds to the charter sector growth differential between expanding and nonexpanding districts. Intuitively, students who applied to charter schools in expanding districts before the cap reform were significantly less likely to get a seat in a charter school than students who applied after the reform, when the number of seats had increased. This was not true in nonexpanding districts, where the number of seats in charter schools remained relatively constant.

The IV-DiD rests on the idea that the pre-reform expanding districts' cohorts provide a good counterfactual for what would have happened to post-reform expanding districts' cohorts 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 our Results section below.

Two additional identifying assumptions underlie the IV-DiD method. First, the interaction between living in an expanding district and post-reform years should be independent of potential outcomes. This would typically not hold if expanding and nonexpanding districts did not experience parallel pre-trends in expenditures or achievement before the reform. Due to concerns about such trends, we adopt a conservative approach by systematically controlling for districts' time trends in our IV-DiD specifications. Finally, the interaction between living in an expanding district and post-reform years should only affect student outcomes through its effects on the probability of charter enrollment, not through any other factor or unobserved characteristic. We have discussed that exclusion restriction in the section on IV-SC.

Using a DiD after the synthetic control method has two advantages. First, to construct a control group of nonexpanding districts, we start by re-using the group of districts identified and validated by the synthetic control method. We use these districts as a standard control group, without the district weights computed by the synthetic control method. Then, we enlarge this control group to include all the nonexpanding districts that are in the lowest 10th percentile of the test scores distribution. In addition, combining IV-SC and IV-DiD gives us the opportunity to check that both methods yield similar results despite being based on different pre-trends assumptions.

The second-stage equation for the spillover analysis is:

$$Y_{dt} = \alpha_2 + \delta_2 P_{dt} + \theta_2 E_d + \lambda C_{dt} + \epsilon_{dt} \tag{16}$$

where Y_{dt} is per-pupil expenditure or achievement in district d in year t, δ_2 is the coefficient of the a post-reform dummy P_{dt}, θ_2 is the coefficient of a dummy for expanding districts E_d, and ϵ_{dt} is an error term. Our treatment variable, C_{dt}, measures the share of students enrolled in a charter school.

To instrument charter share, we combine a just-identified model and an over-identified model in which we use three instrumental variables. In the just-identified model, we use a dummy variable Z_{dt} as an instrumental variable for charter share. Z_{dt} is the interaction between the post-reform dummy P_{dt} and the dummy for expanding districts E_d. The first stage for this two-stage least squares (2SLS) procedure is:

$$C_{dt} = \alpha_1 + \delta_1 P_{dt} + \theta_1 E_d + \gamma Z_{dt} + \nu_{dt} \tag{17}$$

where γ is the effect of post-reform expanding districts on charter share. As in the second-stage equation, the first stage includes a post-reform dummy P_{dt}, a dummy for expanding districts E_d, and district time effects. In the over-identified model, we use three dummy variables Z_{1dt}, Z_{2dt}, and Z_{3dt} as instrumental variables for charter share. The dummy variable for expanding districts is decomposed into three sub-categories which are Boston, other urban expanding districts, and nonurban expanding districts. Z_{1dt} is the interaction between a post-reform dummy and a Boston dummy. Z_{2dt} is the interaction between a post-reform dummy and a dummy for other urban expanding districts. Z_{3dt} is the interaction between a post-reform dummy and a dummy for nonurban expanding districts. In the over-identified model, the first stage for this two-stage least squares (2SLS) procedure becomes:

$$C_{dt} = \alpha'_1 + \delta'_1 P_{dt} + \theta'_1 E_d + \gamma_1 Z_{1dt} + \gamma_2 Z_{2dt} + \gamma_3 Z_{3dt} + \nu'_{dt} \tag{18}$$

For standard errors, we use the White estimator of variance.

5 Results on Fiscal Spillovers of Charter Expansion

Our results reveal that increased charter attendance shifts districts' expenditures towards seemingly more productive inputs. For each expenditure variable, we use the log of the variable as an outcome. We use the synthetic control method separately for each of the five expenditure outcomes in which we are interested. Table 2 lists the selected synthetic control districts and associated weights. Of the 75 districts in the donor pool, between five and 11 districts have been selected as synthetic control districts. For districts' total per-pupil expenditure (column 1), Worcester is the most heavily weighted (44.3 percent), followed by Southbridge, Athol-Royalston, and Somerville, which receive weights of 19.4, 19.2, and 14.9 percent, respectively. North Adams has the smallest weight at 2.3 percent. The next four columns report districts' weights for per-pupil expenditures on fixed costs, instruction, salaries, and support services.

The top left plot of Figure 3 shows the first stage estimate of the synthetic control method, that is, the charter share evolution in expanding districts and the synthetic control.[21] Annual charter share in the synthetic group closely follows the charter share in expanding districts until 2011. The next five figures show the reduced form estimates, i.e. the evolution of our five measures of districts' expenditures in expanding districts and synthetic control. Here again, the synthetic control appears to replicate very well the path of expanding districts' per-pupil expenditures before the 2011 reform. The different synthetic groups appear to be good controls for the expanding districts. After 2011, however, the curves clearly diverge, and by 2015, charter share in expanding districts has increased by more than three percentage points compared to the synthetic control, going from five percent to more than eight.[22] Similarly, total per-pupil expenditures as well as per-pupil expenditures on fixed costs, instruction, and salaries increase in expanding districts. By 2015, total per-pupil expenditures has increased by, on average, 4.9% in expanding districts compared to the synthetic control group; per-pupil expenditures on fixed costs have increased by 6.4%; per-pupil expenditures on instruction by 7.5%; and per-pupil expenditures on salaries by 5.1%. These increases are accompanied by a 3.9% reduction in per-pupil expenditures on support services.[23]

We use placebo inference to evaluate the probability that these effects are due to chance. Figure 4 reports the estimated treatment effect for expanding districts (line "TREATMENT"), and the placebo effect for each placebo district. We only keep the placebo districts that have a RMSPE that is no larger than three times the RMSPE of the expanding districts. We discard

[21]We run the synthetic control algorithm separately for each outcome. As reported in Table 2, the group of synthetic control districts differs for each outcome, which also implies that we have a different first stage for each outcome. In practice, the first stage figures are relatively similar. Due to space restrictions, Figure 3 only shows the first stage for the per-pupil expenditure on instruction.

[22]The magnitude of the charter share increase differs from the one in Figure 1 because in Figure 3, the charter share is an average of district level charter share in expanding and nonexpanding districts. Figure 1 plots charter share that is directly calculated from student level data.

[23]The value of the coefficients is displayed in Figure 4. The reported coefficient corresponds to the mean difference, over the years 2012 to 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 increase or percentage reduction in the absolute value of the outcomes.

the control districts with a high RMSPE because they might bias the inference by creating spuriously large treatment effects. This explains why the number of placebo districts varies by outcome. The top left figure shows placebo inference for the first stage, in other words the comparison between the estimated changes in charter share for expanding districts and placebo districts. Expanding districts have very significantly higher charter expansion than any other placebo district. The p-value, which calculates the probability that a randomly selected placebo district has a significantly lower coefficient than the expanding districts, is equal to one. Changes in instruction, salaries, and support services also appear to be large, with p-values of 0.92, 0.88, and 0.11.[24]

The IV-DiD estimates confirm these results. When moving to the IV-DiD specifications, we successively use as a control group the synthetic control districts and the districts in the lowest 10th percentile of test scores that did not expand. When we use the synthetic control districts, unlike in the IV-SC method, we do not use the districts' weights. All synthetic control districts have a weight of one in the IV-DiD. Figure 5 reports pre-trends in both charter share and district expenditures when the control group comprises synthetic control districts. All trends look very parallel, although trends in fixed costs are slightly noisier. Appendix Figure A.1 reports trends when we enlarge the control group to all nonexpanding districts in the lowest 10th percentile of test scores. Again, pre-trends look very similar, except for trends in per-pupil expenditures on fixed costs. This confirms the importance of controlling for districts' time trends in all IV-DiD specifications.

Table 3 reports two-stage least squares (2SLS) estimates of charter school expansion's fiscal spillovers for the over-identified model. The coefficients suggest that overall per-pupil expenditures do not change significantly. However, as expected, per-pupil expenditure on fixed costs goes up. Moving from 7% to 12% of students attending charter schools (which is the average jump for expanding districts after the reform) would increase per-pupil expenditures on fixed costs by 23%. At the same time, per-pupil expenditures on instruction would also increase by 12%, meaning that increased per-pupil expenditures are not solely driven by fixed costs. With regard to the competition effect, districts seem to shift resources towards more productive inputs. For a five percentage point increase in charter share, we find that the aforementioned positive impact on instruction expenditures would be compensated by an 13% drop in support services expenditures.

Appendix Table A.3 shows that the first-stage coefficients for each of the three instruments are significant. The first stage is significantly larger in Boston, where post-reform charter share goes up by five percentage points more than in nonexpanding districts. In other urban districts and in nonurban districts, charter share goes up by 1.6 and 1.4 percentage points, respectively. Finally, Appendix Table A.4 shows results for the just-identified model. Standard errors are significantly larger when using a single instrument, which makes it more difficult to detect significant effects.

Our findings on the fiscal spillovers effect of charter expansion stand in contrast to stud-

[24]The standard significance level of 10 percent corresponds to a p-value larger than 0.9 or smaller than 0.1.

ies that show charter expansion has negative fiscal impact on district spending. We suspect that charter schools' fiscal impact probably depends on whether or not a state has a refund scheme. Massachusetts has one, but Michigan does not, which might explain the negative fiscal spillovers observed by, for example, Arsen and Ni (2012). On the other hand, Bifulco and Reback (2014) present case studies of the financial adaptation of traditional public schools to enrollment declines in Albany and in Buffalo New York, where an aid program similar to the one in Massachusetts exists.[25] Yet their estimates, made under different scenarios, suggest a negative impact of charter expansion. Ladd and Singleton (2017) apply the same methodological approach to six urban and nonurban districts in North Carolina, where no transitional aid program exists. For all districts, they find a negative net fiscal impact of charter schools.

A large literature has linked per-pupil expenditures and student achievement. Guryan (2003) used a policy-induced regression discontinuity in Massachusetts to estimate that a per-pupil expenditures increase of 1 standard deviation increases test scores by approximately 0.5 standard deviation. More recently, Jackson et al. (2016) find that a 10 percent increase in per-pupil spending each year for all 12 years of public school leads to 0.27 more completed years of education, 7.25 percent higher wages, and a 3.67 percent reduction in the annual incidence of adult poverty. The shift of resources we observe towards seemingly more productive inputs raises questions about the charter school expansion's subsequent impact on student achievement.

6 Education Spillover of Charter School Expansion

To investigate the impact of charter school expansion on student achievement, we use the same IV-SC and IV-DiD methodologies detailed above. However, looking at student achievement as an outcome raises two additional challenges in terms of identification.

6.1 Change in Student Selection and Charter Effectiveness When the Charter Sector Expands

First, students' selection into charter schools might change with charter expansion. This raises the question of what student sample is appropriate for estimating the spillover effect of charter school expansion? If both charter and non-charter students are considered, the effect of charter expansion on average outcomes includes both the charter treatment effect for those enrolling in

[25]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 program reimburses the districts for a portion of their charter school payments that are attributable to recent increases in charter school enrollment. The award amounts are 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." This aid is very similar to the one in place in Massachusetts.

a charter school and the spillover effects for students in traditional public schools. Restricting the sample to non-charter students, as we do for expenditures outcomes, does not solve the issue and would actually bias estimations.

Ample evidence shows that student selection into charter schools changes when charter schools expand. Using data from National Alliance for Public Charter Schools, Epple et al. (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% in 2001 to 50% 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. Finally, looking at charter schools that expanded in Boston after the 2011 cap reform, Cohodes et al. (2016) observe that expanded charters attracted a more disadvantaged, lower-achieving population. As suggested by the authors, "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".

We also look at this in our data. As shown in Figure 6, before the reform, expanding districts were already growing in terms of charter share, while nonexpanding districts experienced no growth. We use this fact to show how, before the reform, charter students' characteristics changed in expanding districts compared to nonexpanding districts. Figure 6 reports that the share of black charter school students diminishes with expansion, while the share of Asian charter students increases with expansion. This provides suggestive evidence that students' selection into charter schools might change when charter schools expand.

The dynamic selection of students into charter schools implies that students in traditional public schools are also increasingly selected under charter expansion. Unlike most previous studies, we therefore estimate the causal impact of charter expansion on all students while also accounting for individual charter enrollment. Specifically, our regressions include both charter share and individual charter enrollment as explanatory variables. Controlling for individual charter enrollment requires to account for student selection into charter schools. A large body of literature shows that charter enrollment is endogenous. Charter applicants have different observed and unobserved characteristics than non-charter applicants. We therefore use a lottery instrument to recover unbiased estimates of charter effectiveness.[26]

The second challenge for identification is the potential correlation between charter expansion and charter effectiveness. The charter schools that expand are likely to be the best ones. As detailed above, after Massachusetts' 2011 reform, expansions in districts close to the 9% limit on charter funding were limited to proven providers, that is, existing charter schools or

[26]For references on charter school effectiveness and use of lotteries to identify it, see Hoxby and Murarka (2007), Angrist et al. (2010), Dobbie and Fryer (2011), Dobbie and Fryer (2016), Abdulkadiroğlu et al. (2011), Angrist et al. (2013), Abdulkadiroğlu et al. (2016), Carlson and Lavertu (2016), Angrist et al. (2016), and Cohodes et al. (2016).).
The model that controls for individual charter enrollment has both individual and aggregate charter enrollment as endogenous instrumented variables. This is a typical peer-effect specification as in Acemoglu and Angrist (2000) and Angrist (2014). A standard identification assumption of this model is that the private return of charter attendance with the reform instrument is the same as the private return with the lottery instrument.

boards of governors with track records of high performance. Consistent with this, Cohodes et al. (2016) 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.[27] Figure 6 also shows that charter students' test scores in both math and ELA have increased over time in expanding districts. Although this is not a measure of charter schools' value-added, this evolution suggests there may be a correlation between charter expansion and charter effectiveness. Without accounting for that correlation, our estimate of charter expansions' spillover effect would capture both the spillover effect and the effect of the increased charter quality when charter schools expand. We account for changing charter effectiveness by allowing the charter effect to be time-varying. 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.

A last concern for identification arises if 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 IV-SC (that imposes parallel pre-trends) more valuable. Combining the two methods gives us the opportunity to check that they yield similar results despite being based on different pre-trends assumptions.

6.2 IV-SC: From Student-Level to District-Level Achievement

While spending outcomes vary at the district-by-year level, student achievement varies at the student level. Yet the synthetic control methodology requires district-level variables to compute the district weights. We therefore need to aggregate our outcomes at the district level. In order to both control for individual-level confounders (charter enrollment and demographics) and aggregate the dataset at the district-by-year level, we start by running the following regression:

$$Y_{idt} = \alpha + \beta_0 C_i + \beta_1 C_i * Urb_i + \beta_2 C_i * P_i + \gamma' X_i + \mu_{dt} + \epsilon_{idt} \tag{19}$$

where Y_{idt} is the test score of student i in district d and year t. C_i is a dummy for individual charter enrolment, $C_i * Urb_i$ is a dummy for enrolment in an urban charter school, and $C_i * P_i$ is a dummy for enrollment in a charter school after the 2011 reform. X_i is a vector of student demographic characteristics (sex, race, special education, limited English proficiency, subsidized lunch status, and a female-minority interaction term). μ_{dt} is a full set of districts-by-year fixed effects that capture the remaining variation in achievement we are interested in. More specifically, the districts-by-year fixed effects estimate the districts-by-year level variation in test scores, once we have accounted for charter effectiveness and students' demographics. This is the outcome variable we use to identify synthetic control districts.

When controlling for the charter effect, we allow this effect to vary along two dimensions:

[27] Baude et al. (2014) also find that charter school quality has improved over time in Texas.

whether the charter school is located in an urban area and whether the charter effect is estimated before or after the 2011 reform.[28] In the second step, we simply apply the synthetic control algorithm on $\hat{\mu}_{dt}$ as our dependent variable rather than the average Y_{idt}. The rest of the IV-SC methodology is identical as explained in section 4.

To estimate equation 19, we instrument individual charter enrollment, enrollment in an urban charter school, and enrollment in a charter school after the reform. We use as instrumental variables a dummy indicating if a student wins a charter lottery, a dummy indicating if a student wins a lottery for an urban charter school, and a dummy indicating if a student wins a lottery for a charter school after the 2011 reform. Table 4 reports first stage and second stage estimates of the private return to charter schools. Columns 1, 2, and 3 show first stage coefficients for each of the three instrumental variables. They all have a positive and significant impact on student probability to enroll in a charter school. Coefficients in math and ELA differ slightly because of differences in student samples. The second stage coefficients confirm that charter schools produce larger gains in urban areas than in nonurban areas. They also appear to be more effective after the cap reform than before. This is consistent with what Cohodes et al. (2016) find in Boston.

6.3 IV-DiD on Student-Level Achievement

Unlike the IV-SC that requires aggregate level outcomes, we run the IV-DiD regressions on student level achievement. To make sure that the achievement variable we use is as similar as possible for the IV-SC and the IV-DiD, we use the same residualization process. In other words, we use as an outcome the student level achievement once accounted for individual-level confounders (charter enrollment and demographics). We run the following regression to get the residualized test scores:

$$Y_{idt} = \alpha + \beta_0 C_i + \beta_1 C_i * Urb_i + \beta_2 C_i * P_i + \gamma' X_i + \epsilon_{idt} \tag{20}$$

The only difference between this equation and equation 19 is the absence of the districts-by-year fixed effects μ_{dt} in this equation. All other terms are identical. For the IV-DiD, we use $\hat{\epsilon}_{idt}$ as the dependent variable rather than the average Y_{idt}. The second-stage equation for the achievement spillover analysis is:

$$\hat{\epsilon}_{idt} = \alpha_2 + \delta_2 P_{idt} + \theta_2 E_{id} + \lambda C_{idt} + \epsilon_{idt} \tag{21}$$

where $\hat{\epsilon}_{idt}$ is the residualized test score of student i in district d and year t, δ_2 is the coefficient of the a post-reform dummy P_{idt}, θ_2 is the coefficient of a dummy for expanding districts

[28] 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). We tested other sources of heterogeneity (including gender) and only kept the variables that were significant sources of heterogeneity.

E_{id}, and ϵ_{idt} is an error term. Our treatment variable, C_{idt}, measures the share of students enrolled in a charter school.

For the analysis of achievement spillover, we restrict the sample of expanding districts to Boston. We adopt that restriction because Boston is the district with the largest charter expansion after the reform, and the expanding district with the highest number of students in our sample. When testing the over-identified model with three instruments, the first stage coefficients of two instruments were not significant. In urban expanding districts excluding Boston and nonurban expanding districts, the charter share did not increase significantly more than in nonexpanding distircts. We therefore prefer to focus the analysis on Boston.[29]

We use a dummy variable Z_{idt} as an instrumental variable for charter share. Z_{idt} is the interaction between the post-reform dummy P_{idt} and a dummy for expanding districts E_{id}. The first stage for this two-stage least squares (2SLS) procedure is:

$$C_{idt} = \alpha_1 + \delta_1 P_{idt} + \theta_1 E_{id} + \gamma Z_{idt} + \nu_{idt} \tag{22}$$

where γ is the effect of post-reform expanding districts on charter share. As in the second-stage equation, the first stage includes a post-reform dummy P_{idt}, a dummy for expanding districts E_{id}, and district time effects. We cluster standard errors at the individual and district level.

7 Results on Education Spillover of Charter Expansion

Our results show that increased charter attendance has a positive impact on student achievement, although the effect is not always significant. The synthetic control method reveals a small improvement in student achievement in both math and ELA, though none of the changes markedly differ from placebo tests. Figure 7 shows the evolution of the charter share and math and ELA test scores in expanding and synthetic expanding districts. The top figure shows that charter share increases more in expanding districts than in synthetic control districts. The additional two figures demonstrate that the synthetic control very effectively replicates expanding districts' achievement path before the 2011 reform. After 2011, the curves diverge, and by 2015, achievement in expanding districts has increased by 0.028 standard deviations in math and by 0.019 standard deviations in ELA, as compared to the synthetic control.[30]

However, placebo inference shows that these test score gains are not statistically different from placebo districts' gains. Figure 8 reports the estimate of expanding districts' treatment effect as well as the placebo effects.[31] This figure also shows the p-value, which is the proba-

[29]The fact that the first stage coefficients in the over-identified model are significant for the spending outcomes but not for the achievement outcomes is likely due to the different aggregation levels. The first stage coefficients are estimated on district level variables for the fiscal spillovers and on individual level variables for the achievement spillovers.

[30]The value of the coefficients can be found in Figure 8.

[31]As for expenditure outcomes, we only keep the placebo districts with a RMSPE that is no larger than three

bility that one of the placebo coefficients is higher than or equal to the estimated coefficient for the expanding districts. If p-values for the first stage all show that charter share in expanding districts increased notably more than in nonexpanding districts, the p-values also show that the corresponding test score gains are not statistically significant. We run the synthetic control algorithm separately for math and ELA test scores. As reported in Table 2, the group of synthetic control districts differs for each outcome. This explains why we also have a different first stage for math and ELA test scores.

When moving to the IV-DiD specifications, we successively use as a control group the synthetic control districts and the nonexpanding districts with test scores in the lowest 10th percentile. Figure 9 reports pre-trends in student achievement when the control group comprises synthetic control districts (top two figures) and bottom 10th percentile districts (bottom two figures). Some pre-trends look more parallel than others, which confirms that it is critical to control for districts' time trends in all IV-DiD specifications.

Table 5 reports two-stage least squares (2SLS) estimates of charter school expansion's effect on student achievement. In all regressions, we use the interaction between post-reform cohort and living in Boston as an instrument. The IV-DiD estimates show that charter expansion had a positive and significant impact on students' achievement. Moving from 10% to 15% of students attending charter schools, which is the average post-reform increase in middle school, would raise non-charter students' achievement by 0.033 standard deviations in math and 0.023 in ELA. These estimates are remarkably similar to those we obtain we the IV-SC method. Using the districts in the lowest 10th percentile as a control group yields much smaller (and insignificant) coefficients. This is likely due to the fact that the synthetic control districts more effectively reproduce expanding districts' achievement pre-trends than do the districts in the bottom 10th percentile.

Broadly speaking, 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, 2017) and Florida (Sass, 2006) suggests mildly positive and sometimes significant effects on achievement. In contrast, Bettinger (2005) finds a negative and significant, but very small, effect of charter expansion 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.[32]

It is also worth noting that the negative results in the literature tend to occur in settingss with little funding available to compensate public schools when the charter sector expands. This was the case for Bettinger's 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, 2017).

times the expanding districts' RMSPE. This explains why the number of placebo districts varies by outcome.

[32]For additional references on the effect of charter schools on non-charter students, see Hoxby (2003), Booker et al. (2008), Zimmer and Buddin (2009), Davis (2013), Jinnai (2014), Mehta (2012), Cremata and Raymond (2014), Zimmer et al. (2009), Sass (2006), Bifulco and Ladd (2006).

8 Robustness Checks

Sensitivity Tests for Synthetic Control Specifications

The identification of a group of synthetic control districts is the result of three successive choices regarding (1) the predictor variables, (2) the method used to compute predictor variable weights, and (3) the districts included in the donor pool. To mitigate potential concerns about specification-searching and cherry-picking, we run seven robustness checks that test our main results' sensitivity to changes in each of the three aforementioned choices (Ferman et al., 2016; Kaul et al., 2017). Results are presented in Appendix A. Table A.6 details the specification used for each robustness check. For purposes of comparison, the first row presents the baseline specification used through the paper.[33] 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, for predictor variables, reducing the number of lagged values of the outcome variable and charter share or including their entire pre-reform path systematically yields a worse fit on outcomes. Tests on the size of the donor pool confirm that we must strike a balance between having a sufficiently large donor pool, in order to have enough donor districts similar to the expanding districts, and not having too large a donor pool, to avoid overfitting. Most importantly, for most sensitivity tests we run, the 2011 reform's reduced form effect is notably consistent across specifications. This is particularly true for districts' expenditures, with the exception of per-pupil expenditures on fixed costs.

Innovation and Turnaround Schools

In 2010, the law that raised the cap on charter schools also included provisions for school turnarounds and the creation of innovation schools (Massachusetts State Legislature, 2010). The Innovation Schools initiative provided educators and other stakeholders in Massachusetts with the opportunity to create new schools that operate with increased autonomy and flexibility in terms of curriculum, budget, staffing, and school schedule and calendar. While the innovation school model aimed to be cost-neutral for districts, one-time competitive grants were introduced to support the development of these schools.

School turnarounds is the second initiative adopted in 2011. To better target assistance to underperforming districts, the Massachusetts Department of Elementary and Secondary Education introduced a new five-level district classification system. The state's most struggling

[33] In our main specification, to identify the set of synthetic control districts, we used five lagged values of the outcome and charter share as predictor variables. We used an iterative optimization procedure to compute the predictor variable weights. For the set of donor pool districts, we use districts in the lowest 10th and 25th percentiles of student test scores for districts' expenditures and districts' test score, respectively.

schools are designated level 4 or 5 based on an analysis of four-year trends in absolute achievement, student growth, and improvement trends.[34] Districts with one or more level 4 or 5 schools are required by state law to develop Turnaround Plans that support the accelerated improvement of student achievement within three years. Plans may include nominating a new leader, called a receiver (in level 5 districts only); coaching activities for teachers, administrators, and district leaders; and developing work teams and professional communities of practice.[35] In addition, newly identified level 4 schools are eligible to apply for federal funding through the Massachusetts School Redesign Grants program.

The instrumental variable exclusion restriction would not be verified if the introduction of innovation and turnaround schools was unbalanced between expanding and nonexpanding districts. To address this, for all years after the 2011 reform, we collected data on innovation schools, recipients of innovation schools grants, and the amounts received. We have also collected data on level 4 and 5 schools, as well as recipients of school redesign grants, and amounts received. As a robustness check, we re-analyze achievement spillovers by controlling for innovation schools, level 4 and 5 schools, and recipients of each grant type. This ensures that post-reform differences in achievement between expanding and nonexpanding districts are not driven by differences in the prevalence of innovation or turnaround schools or differences in grants amounts.

We re-analyze fiscal spillovers by accounting for differences in grants received by expanding and nonexpanding districts. We subtract each district's grants for innovation schools or school redesigns from its total expenditures. For other sub-expenditures (on fixed costs, instruction, support services, and salaries), we calculate what share of the total expenditure they represent, and we use that share to subtract the grant received. For instance, if spending on instruction represents 60 percent of a district's total spending, we would subtract 60% of the received grants from expenditures on instruction.

Results in Table 6 indicate that our results on charter expansion's fiscal spillovers are not driven by innovation and school redesign grants. Similarly, the addition of controls for innovation and turnaround schools hardly changes estimates of charter expansion's achievement spillovers.

9 Conclusion

The charter sector has grown rapidly since its introduction in the early 1990's. Yet growing concerns have emerged about charter schools' potential negative impact on non-charter students. The charter sector is claimed to drain resources and high-achieving peers from non-charter

[34]By statute, the state can have no more than 4% of all public, non-charter schools identified as Level 4 and Level 5 at one time. No more than 2.5% of the total number of districts can be designated Level 5 at any one time.

[35]This includes nomination of a new leader, called a receiver (in level 5 districts only), as well as coaching activities for teachers, administrators and district leaders, and development of work teams and professional communities of practice.

schools. Concerns that the charter sector drains resources and high-achieving peers from non-charter schools prevented charter expansion in Massachusetts in November 2016, when voters rejected a ballot initiative that would have added up to 12 new charter schools. This paper investigates the fiscal and educational impact of charter expansion on school districts by exploiting a 2011 reform that raised the cap on charter schools in Massachusetts.

Our results reveal that increased charter attendance shifts districts' expenditures towards seemingly more productive inputs. The IV-SC method shows that, after the reform, per-pupil expenditures on instruction and salaries increased by respectively 7.5% and 5.2%, respectively, in expanding districts compared to nonexpanding districts. This is accompanied by a 4.4% reduction in per-pupil expenditures on support services. Further, our results indicate that charter expansion has a positive impact on student achievement, although the effects are small and not always significant. Our estimates suggest that moving from 10% to 15% of middle school students attending charter schools would increase non-charter student achievement by 0.033 standard deviations in math, and by 0.023 in ELA. These results parallel previous studies that found charter expansion had a limited impact on students' achievement (Bettinger, 2005; Imberman, 2011).

It is worth noting some caveats to our analysis. If charter schools have any spillover effect on student achievement in traditional public schools, we might expect the impact to be larger in traditional public schools that are geographically close to a charter school. As highlighted by Cordes (2017), examining spillover effects over large distances, as we do in this analysis, might underestimate the impact of charter schools on the performance of those students attending traditional public schools in the same neighborhoods where charter schools locate. Going forward, we plan to measure local effects of charter expansion using applications to open new charter schools. Since many applications are rejected, students in areas where a charter school application was rejected can be used as a plausible counterfactual for students in areas where a charter school application has been accepted. Finally, this study focuses on the fiscal and educational spillover effects of charter expansion. Future work might also investigate the charter expansion effect on traditional public schools' class size and teacher quality.

References

Abadie, Alberto, Alexis Diamond, and Jens Hainmueller, "Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program," *Journal of the American Statistical Association*, Jun 2010, *105* (490), 493–505.

__ , __ , and __ , "Comparative Politics and the Synthetic Control Method," *American Journal of Political Science*, Feb 2015, *59* (2), 495–510.

__ and Javier Gardeazabal, "The Economic Costs of Conflict: A Case Study of the Basque Country," *American Economic Review*, Feb 2003, *93* (1), 113–132.

__ , Susan Athey, Guido W. Imbens, and Jeffrey M. Wooldridge, "Sampling-based vs. Design-based Uncertainty in Regression Analysis," *Working Paper*, June 2017.

Abdulkadiroğlu, Atila, Joshua D. Angrist, Peter D. Hull, and Parag A. Pathak, "Charters without lotteries: Testing takeovers in New Orleans and Boston," *American Economic Review*, Jul 2016, *106* (7), 1878–1920.

__ , __ , Susan M. Dynarski, Thomas J. Kane, and Parag A. Pathak, "Accountability and Flexibility in Public Schools: Evidence from Boston's Charters and Pilots," *Quarterly Journal of Economics*, May 2011, *126* (2), 699–748.

Acemoglu, Daron and Joshua D. Angrist, "How Large Are Human-Capital Externalities? Evidence from Compulsory Schooling Laws," *NBER Macroeconomics Annual*, Jan 2000, *15*, 9–59.

Angrist, Joshua D., "The Perils of Peer Effects," *Labour Economics*, Oct 2014, *30*, 98–108.

__ , Parag A. Pathak, and Christopher R. Walters, "Explaining charter school effectiveness," *American Economic Journal: Applied Economics*, Oct 2013, *5* (4), 1–27.

__ , Sarah R. Cohodes, Susan M. Dynarski, Parag A. Pathak, and Christopher R. Walters, "Stand and Deliver: Effects of Boston's Charter High Schools on College Preparation, Entry, and Choice," *Journal of Labor Economics*, Apr 2016, *34* (2), 275–318.

__ , Susan M. Dynarski, Thomas J. Kane, Parag A. Pathak, and Christopher R. Walters, "Inputs and Impacts in Charter Schools: KIPP Lynn," *American Economic Review: Papers & Proceedings*, 2010, *100* (2), 239–243.

Arsen, David and Yongmei Ni, "The Effects of Charter School Competition on School District Resource Allocation," *Educational Administration Quarterly*, Feb 2012, *48* (1), 3–38.

Baude, Patrick, Marcus Casey, Eric A. Hanushek, and Steven G. Rivkin, "The Evolution of Charter School Quality," NBER Working Paper 20645, Oct 2014.

Bettinger, Eric P., "The Effect of Charter Schools on Charter Students and Public Schools," *Economics of Education Review*, Apr 2005, *24* (2), 133–147.

Bifulco, Robert and Christian Buerger, "The Influence of Finance and Accountability Policies on Location of New York State Charter Schools," *Journal of Education Finance*, 2015, *40* (3), 193–221.

__ and Helen F. Ladd, "The Impacts of Charter Schools on Student Achievement: Evidence from North Carolina," *Education Finance and Policy*, Mar 2006, *1* (1), 50–90.

— **and Randall Reback**, "Fiscal Impacts of Charter Schools: Lessons from New York," *Education Finance and Policy*, Jan 2014, *9* (1), 86–107.

Booker, Kevin, Scott M. Gilpatric, Timothy Gronberg, and Dennis Jansen, "The effect of charter schools on traditional public school students in Texas: Are children who stay behind left behind?," *Journal of Urban Economics*, Jul 2008, *64* (1), 123–145.

Carlson, Deven and Stéphane Lavertu, "Charter school closure and student achievement: Evidence from Ohio," *Journal of Urban Economics*, Sep 2016, *95*, 31–48.

Cohodes, Sarah R., Elizabeth M. Setren, and Christopher R. Walters, "Can Successful Schools Replicate? Scaling Up Boston's Charter School Sector," SEII Discussion Paper 06, 2016.

Cordes, Sarah A., "In Pursuit of the Common Good: The Spillover Effects of Charter Schools on Public School Students in New York City," *Education Finance and Policy*, 2017, *forthcoming.*

Cremata, Edward J. and Margaret E. Raymond, "The Competitive Effects of Charter Schools: Evidence from the District of Columbia," *Working paper*, 2014.

Davis, Tomeka M., "Charter School Competition, Organization, and Achievement in Traditional Public Schools," *Education Policy Analysis Archives*, Dec 2013, *21* (88).

Dee, Thomas S. and Jeffrey Levine, "The Fate of New Funding: Evidence from Massachusetts' Education Finance Reforms," *Educational Evaluation and Policy Analysis*, 2004, *26* (3), 199–215.

Dobbie, Will S. and Roland G. Fryer, "Are high-quality schools enough to increase achievement among the poor? Evidence from the Harlem Children's Zone," *American Economic Journal: Applied Economics*, Jul 2011, *3* (3), 158–187.

— **and** — , "Charter Schools and Labor Market Outcomes," NBER Working Paper 22502, Aug 2016.

Epple, Dennis, Richard Romano, and Ron Zimmer, "Charter Schools: A Survey of Research on Their Characteristics and Effectiveness," NBER Working Paper 21256, June 2015.

Feldstein, Martin S., "Wealth Neutrality and Local Choice in Public Education," *American Economic Review*, 1975, *65* (1), 75–89.

Ferman, Bruno, Cristine Campos de Xavier Pinto, and Vitor Augusto Possebom, "Cherry picking with synthetic controls," Working Paper 420, São Paulo School of Economics, June 2016.

Fisher, Ronald C. and Leslie E. Papke, "Local Government Responses to Education Grants," *National Tax Journal*, 2000, *53*, 153–168.

Glomm, Gerhard, Douglas Harris, and Te Fen Lo, "Charter School Location," *Economics of Education Review*, Aug 2005, *24* (4), 451–457.

Gordon, Nora, "Do Federal Grants Boost School Spending? Evidence from Title I," *Journal of Public Economics*, Aug 2004, *88* (9-10), 1771–1792.

Guryan, Jonathan, "Does Money Matter? Estimates from Education Finance Reform in Massachusetts," NBER Working Paper 8269, May 2003.

Hines, James R. and Richard H. Thaler, "Anomalies: The Flypaper Effect," *Journal of Economic Perspectives*, 1995, *9* (4), 217–226.

Hoxby, Caroline M., "School choice and school productivity: Could school choice be a tide that lifts all boats?," in C.M. Hoxby, ed., *The Economics of School Choice*, Chicago, IL: University of Chicago Press, January 2003, pp. 289–342.

__ **and Sonali Murarka**, "Charter Schools In New York: Who Enrolls and How They Affect Their Students' Achievement," NBER Working Paper 14852, Apr 2007.

Imberman, Scott A., "The Effect of Charter Schools on Achievement and Behavior of Public School Students," *Journal of Public Economics*, Aug 2011, *95* (7-8), 850–863.

Inman, Robert P., "The Flypaper Effect," NBER Working Paper 14579, Dec 2008.

Jackson, C. Kirabo, Rucker C. Johnson, and Claudia Persico, "The Effects of School Spending on Educational and Economic Outcomes: Evidence from School Finance Reforms," *Quarterly Journal of Economics*, Feb 2016, *131* (1), 157–218.

Jinnai, Yusuke, "Direct and indirect impact of charter schools' entry on traditional public schools: New evidence from North Carolina," *Economics Letters*, Sep 2014, *124* (3), 452–456.

Kaul, Ashok, Stefan Klößner, Gregor Pfeifer, and Manuel Schieler, "Synthetic Control Methods: Never Use All Pre-Intervention Outcomes Together With Covariates," *Working paper*, July 2017.

Ladd, Helen F. and John D. Singleton, "The Fiscal Externalities of Charter Schools: Evidence from North Carolina," *Working paper*, 2017.

Massachusetts Department of Elementary and Secondary Education, "Charter School Enrollment Data Annual Report (2016-2017)," Technical Report 2017.

Mehta, Nirav, "Competition in Public School Districts: Charter School Entry, Student Sorting, and School Input Determination," *Working paper*, 2012.

National Alliance for Public Charter Schools, "2016 Annual Report," 2016.

Rosenbaum, Paul R., "Interference Between Units in Randomized Experiments," *Journal of the American Statistical Association*, Mar 2007, *102* (477), 191–200.

Sass, Tim R., "Charter Schools and Student Achievement in Florida," *Education Finance and Policy*, Mar 2006, *1* (1), 91–122.

The New York Times, "Trump-Clinton? Charter Schools Are the Big Issue on Massachusetts' Ballot," November 5th, 2016.

U.S. Census Bureau, "Public Education Finances: 2015," Technical Report, U.S. Census Bureau 2017.

Winters, Marcus A., "Measuring the Effect of Charter Schools on Public School Student Achievement in an Urban Environment: Evidence from New York City," *Economics of Education Review*, Apr 2012, *31* (2), 293–301.

Zimmer, Ron and Richard Buddin, "Is Charter School Competition in California Improving the Performance of Traditional Public Schools?," *Public Administration Review*, Sep 2009, *69* (5), 831–845.

__ **, Brian Gill, Kevin Booker, Stephane Lavertu, Tim R. Sass, and John Witte**, *Charter schools in eight states: Effects on achievement, attainment, integration, and competition*, RAND, 2009.

Figure 1: Charter Sector Expansion after the 2011 Reform

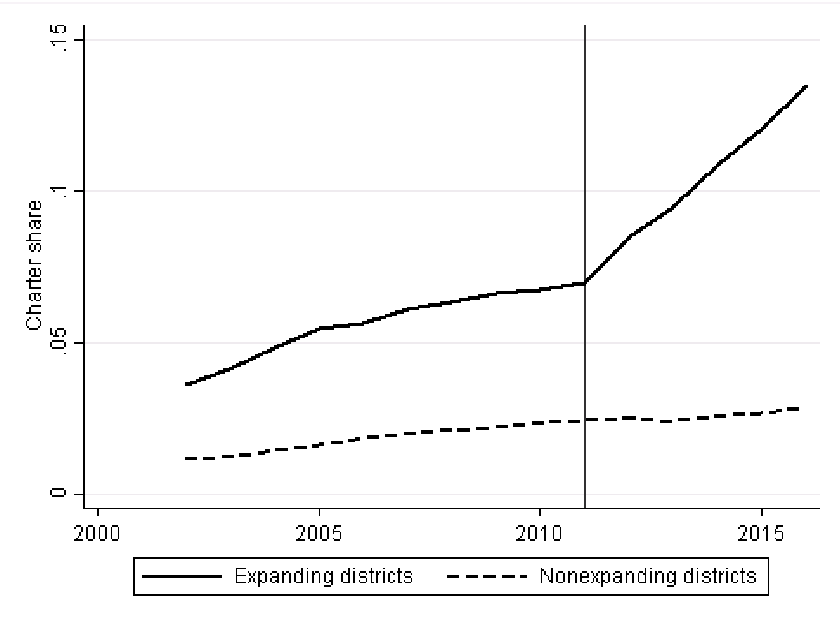
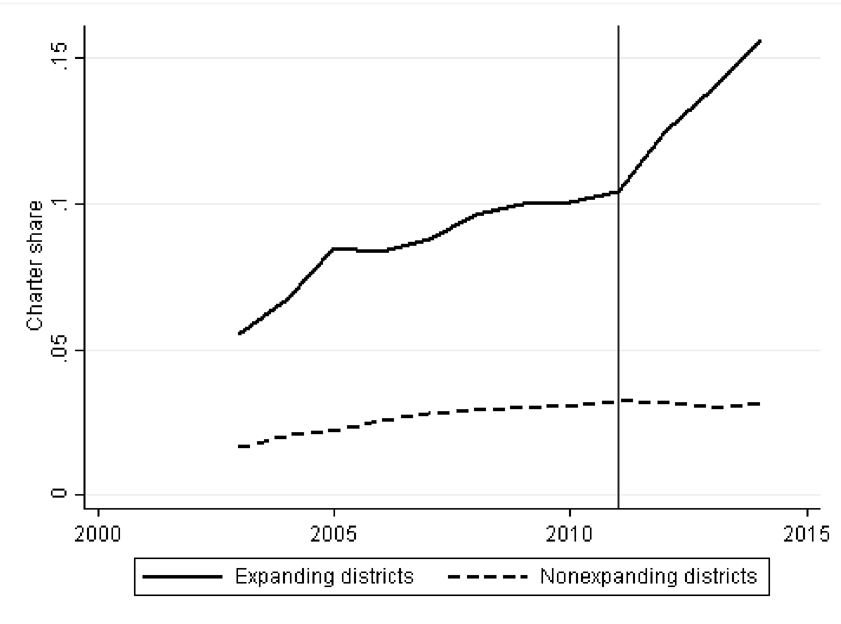


Figure 2: Charter Sector Expansion after the 2011 Reform - Middle Schools



Notes: These figures plot the evolution of the share of students attending a charter school. Figure 1 plots the evolution for elementary, secondary, and high school students, while Figure 2 is limited to middle school students. The plain lines represent districts that experienced an increase in the share of students attending a charter school after the 2011 reform (expanding districts), and the dotted lines represent the districts that did not expand their charter sector after the reform (nonexpanding districts).

Table 1: Descriptive Statistics for Students and Districts

	All districts	High charter share districts	Low charter share districts	Expanding districts	Non expanding districts
	(1)	(2)	(3)	(4)	(5)
			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
			B. Districts' per-pupil expenditures		
Total spending	14,402	15,614	13,573	15,817	14,357
Spending on instruction	9,075	9,857	8,534	9,848	9,050
Spending on fixed costs	2,275	2,363	2,207	2,264	2,275
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
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 respectively higher (column 2) and lower (column 3) 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.

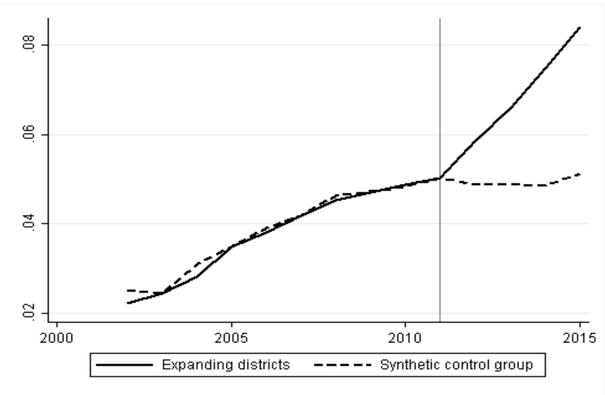
Table 2: Synthetic Control Districts' Weights

	Districts' per-pupil expenditures					Students' test scores	
		Fixed			Support		
	Total	costs	Instruction	Salaries	Services	Math	ELA
District	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Brockton				0.034			
Cambridge						0.167	
Chicopee			0.225				
Easthampton						0.044	
Erving		0.079				0.039	
Everett		0.116					
Fall River				0.161	0.122	0.138	
Greenfield		0.069		0.092	0.127		
Leominster		0.002					
Medford		0.209					0.006
North Adams	0.023		0.088	0.076	0.024	0.175	0.179
Northampton							0.213
Oxford		0.075					
Randolph					0.075		
Somerville	0.149			0.100	0.235	0.234	0.084
Southbridge	0.194		0.012	0.258			
Springfield		0.111		0.081	0.060		0.413
Webster					0.357		
Winthrop		0.253					
Worcester	0.443	0.011	0.521	0.153		0.203	0.104
Adams-Cheshire		0.053					
Athol-Royalston	0.192		0.154	0.045			
Pioneer Valley		0.022					

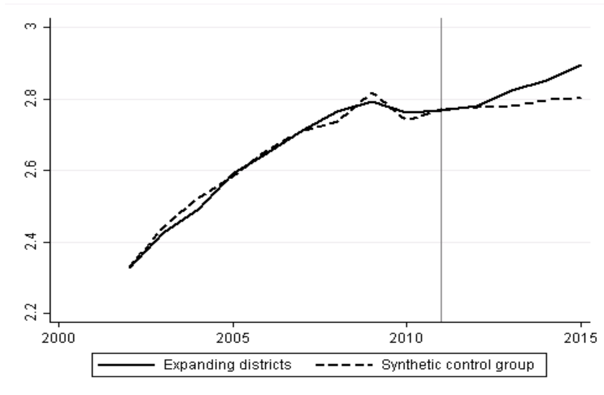
† Notes: This table reports the district weights assigned by the synthetic control method. Columns 1 to 7 report weights computed when the outcome variable is, respectively, districts' per-pupil expenditures (column 1), districts' per-pupil expenditures on fixed costs (column 2), on instruction (column 3), salaries (column 4), support services (column 5), student achievement in math (column 6), and student achievement in ELA (column 7). For all expenditure variables, we use the log of the variable as an outcome variable.

Figure 3: Charter Share and Districts' Per-Pupil Expenditures in Expanding Districts and Synthetic Control Districts

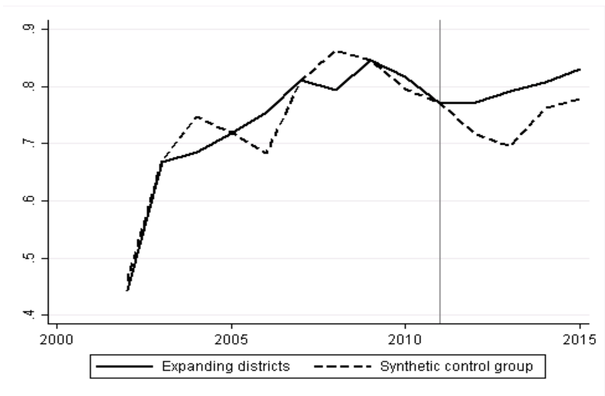
(a) Charter share



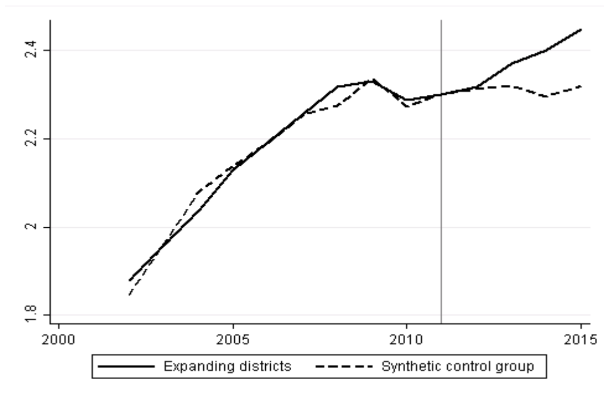
(b) Total spending



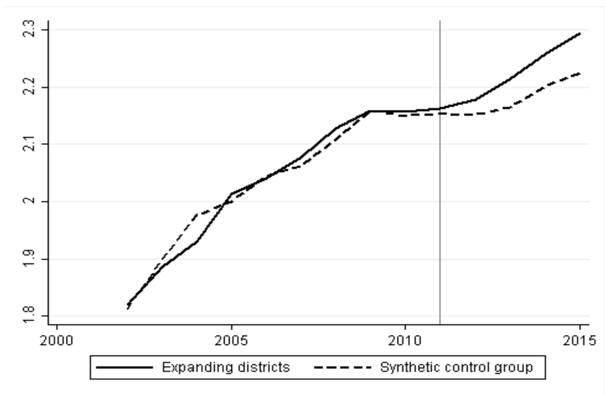
(c) Spending on fixed costs



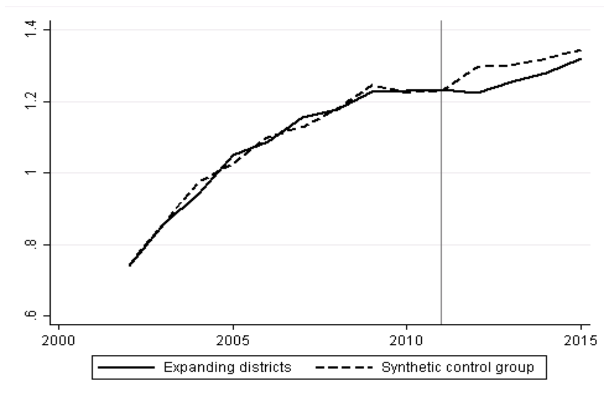
(d) Spending on instruction



(e) Spending on salaries



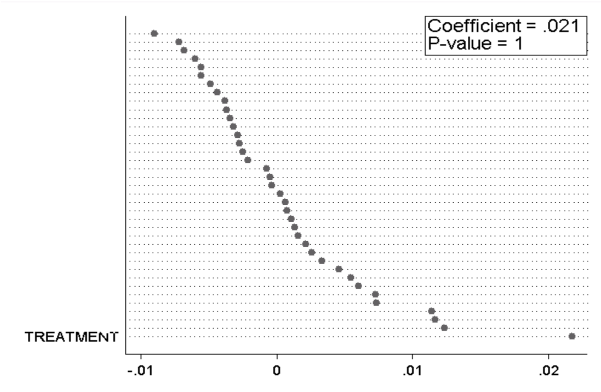
(f) Spending on support services



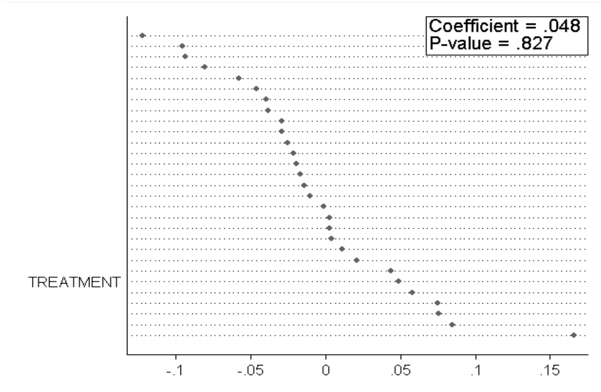
Notes: This figure plots the share of students attending a charter school (plot a), districts' per-pupil expenditures (plot b), their per-pupil expenditures on fixed costs (plot c), instruction (plot d), salaries (plot e), and support services (plot f). For all expenditure variables, we use the log of the variable. The plain lines represent districts that experienced 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. We use the weights defined by the synthetic control method.

Figure 4: Placebo Inference for the Fiscal Impact of Charter School Expansion

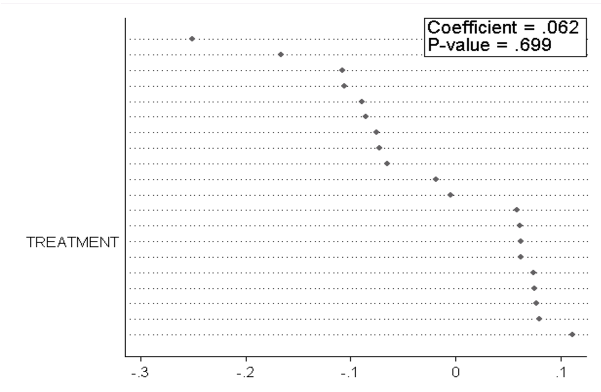
(a) Charter share



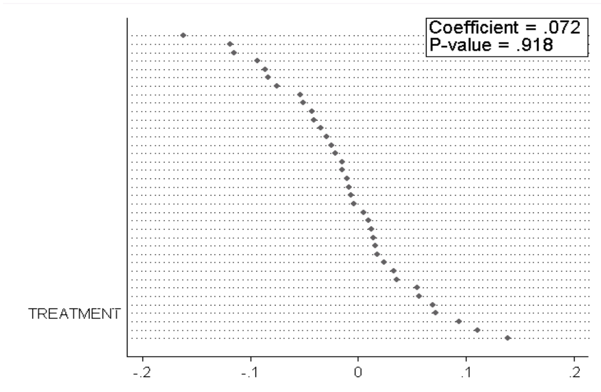
(b) Total spending



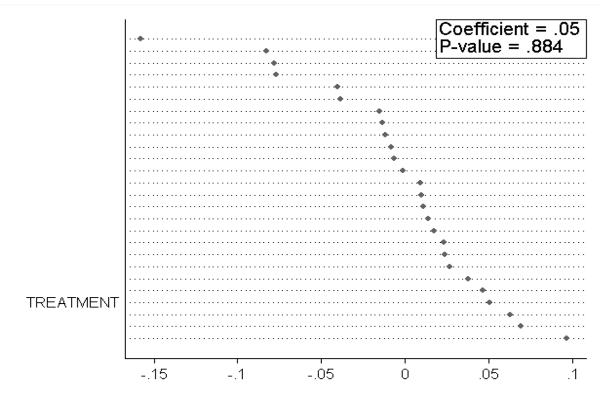
(c) Spending on fixed costs



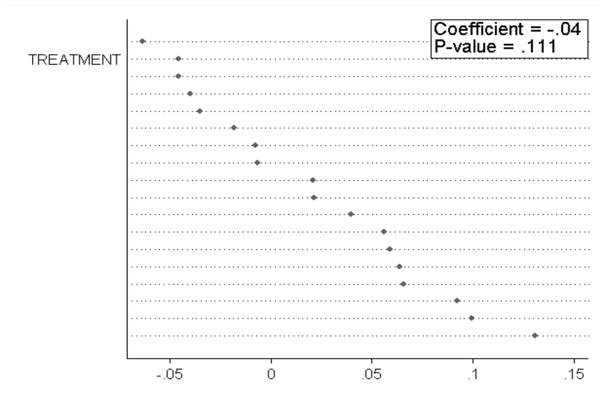
(d) Spending on instruction



(e) Spending on salaries



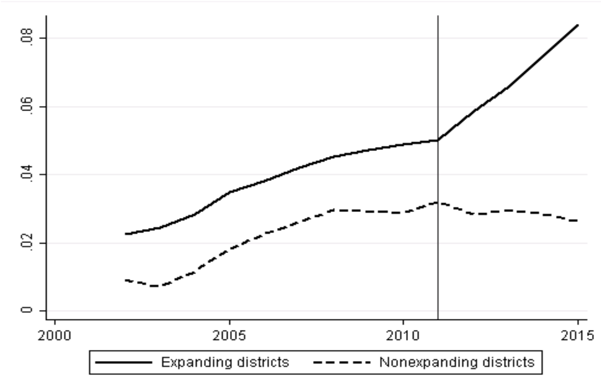
(f) Spending on support services



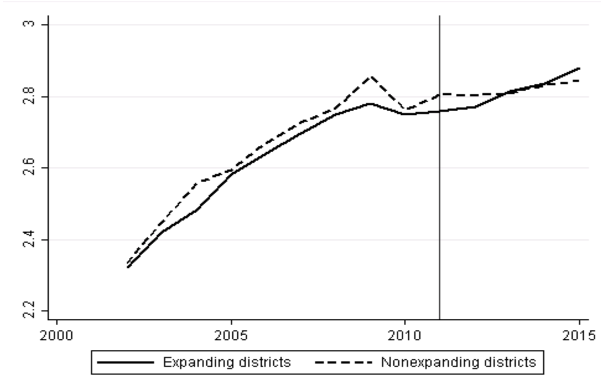
Notes: This figure plots the distribution of the charter expansion's impact on districts' per-pupil expenditures, as measured by the synthetic control method. The lines "TREATMENT" report the coefficients when expanding districts are compared to their synthetic control districts. The exact value of each coefficient is reported in the top right corner of each figure. For all expenditure variables, we use the log of the variable. The other lines in the figures report the coefficients when a placebo district is compared to its identified group of synthetic control districts. The p-values report the probability that one of the placebo coefficients is higher than or equal to the estimated coefficient for the expanding districts.

Figure 5: Pre trends in Charter Share and Districts' Per-Pupil Expenditures

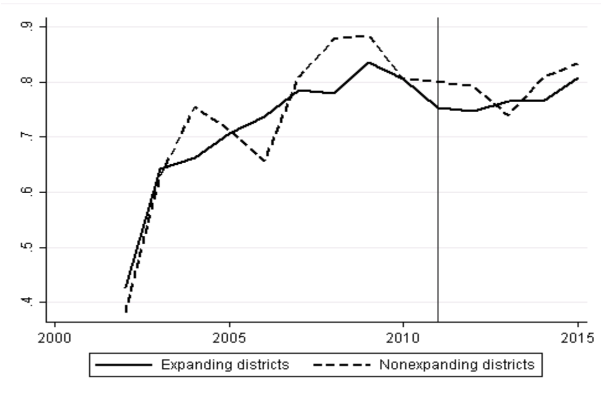
(a) Charter share



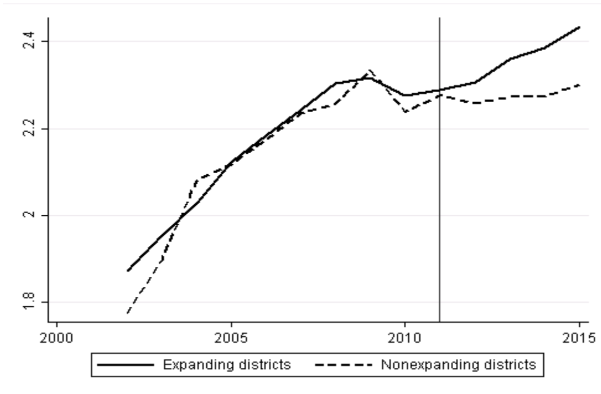
(b) Total spending



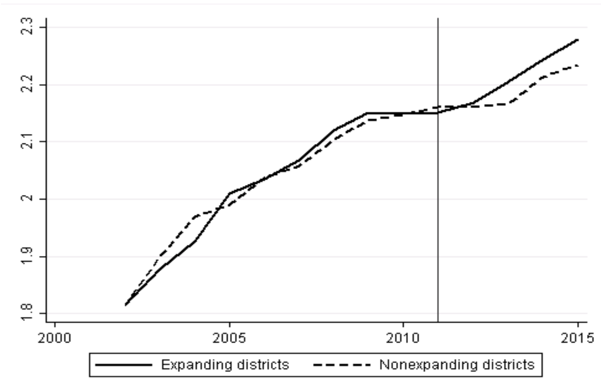
(c) Spending on fixed costs



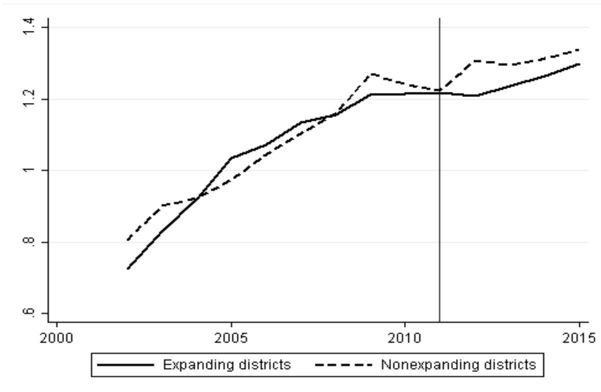
(d) Spending on instruction



(e) Spending on salaries



(f) Spending on support services



Notes: This figure plots the share of students attending a charter school (plot a), districts' per-pupil expenditures (plot b), their per-pupil expenditures on fixed costs (plot c), instruction (plot d), salaries (plot e), and support services (plot f). For all expenditure variables, we use the log of the variable. The plain lines represent expanding districts and the dotted lines represent synthetic control districts. For both expanding districts and synthetic districts, we plot the average charter share and expenditures without using the weights defined by the synthetic control method.

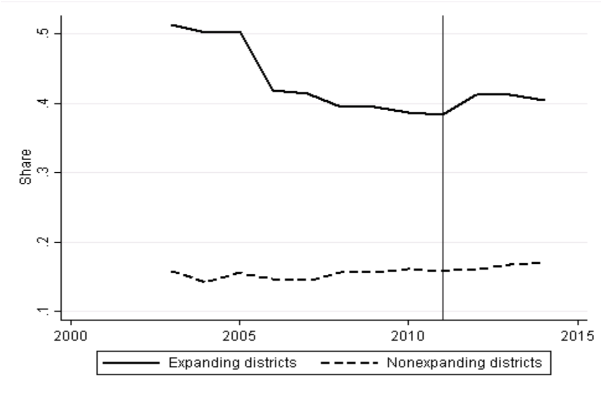
Table 3: 2SLS Estimates of Fiscal Spillovers

	Per-pupil expenditures on:				
	Total (1)	Instruction (2)	Fixed costs (3)	Support services (4)	Salaries (5)
	Control group: Synthetic control districts				
Charter share	1.0980	2.3540**	4.6303***	-2.6167**	0.9285
	(0.8633)	(0.9485)	(1.5172)	(1.2673)	(0.5827)
N	196	196	182	224	252
R2	0.999	0.999	0.968	0.988	0.999
F-Stat	9.5	9.9	10.9	12.4	11.9
	Control group: Districts in the lowest 10th percentile				
Charter share	0.6311	1.6312*	4.3034***	-2.7500**	0.4498
	(0.8042)	(0.8248)	(1.4815)	(1.1339)	(0.5752)
N	392	392	392	392	392
R2	0.999	0.999	0.968	0.988	0.999
F-Stat	10	10	10	10	10

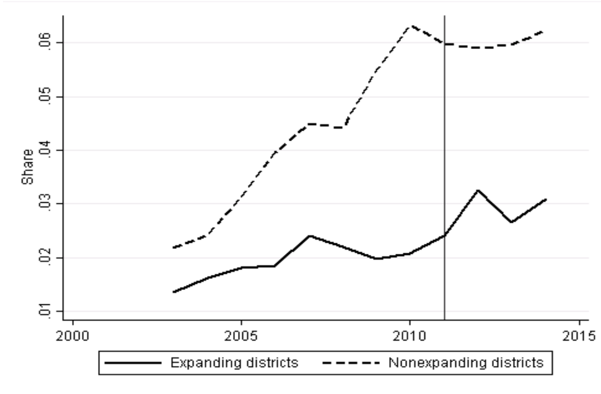
† Notes: This table reports 2SLS estimates of the charter expansion's effect on districts' per-pupil expenditures. For all expenditure variables, we use the log of the variable. The endogenous variable is the charter share, which is a continuous variable that ranges from 0 to 1. In this over-identified model, we use three instruments: (i) the interaction between a post-reform years dummy and a Boston dummy, (ii) the interaction between a post-reform years dummy and a dummy for other urban expanding districts, and (iii) the interaction between a post-reform years dummy and a dummy for nonurban expanding districts. All regressions control for expanding districts, post-reform years, and district time trends. For standard errors, we use the White estimator of variance. When using the synthetic control districts as a control group, the number of observations varies for each outcome depending on how many synthetic control districts were identified.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Figure 6: Charter Students' Characteristics and Achievement

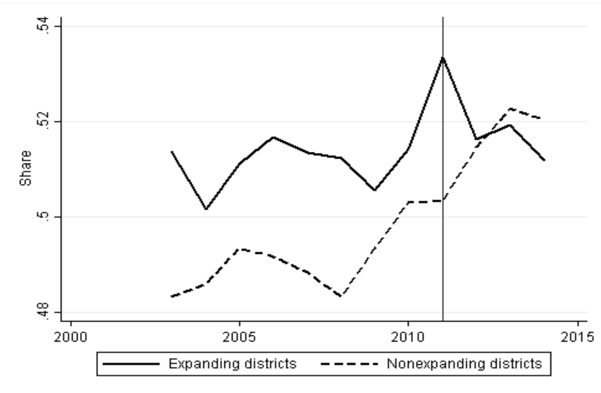
(a) Black



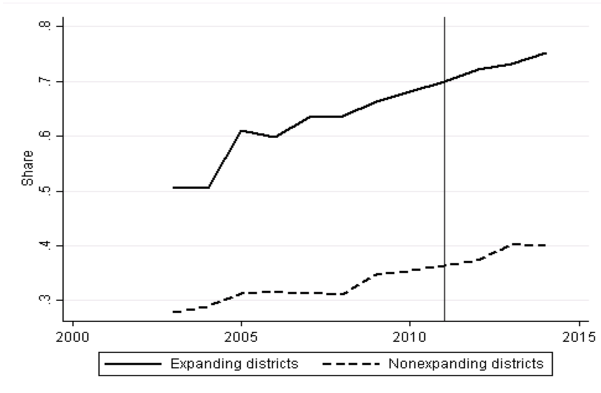
(b) Asian



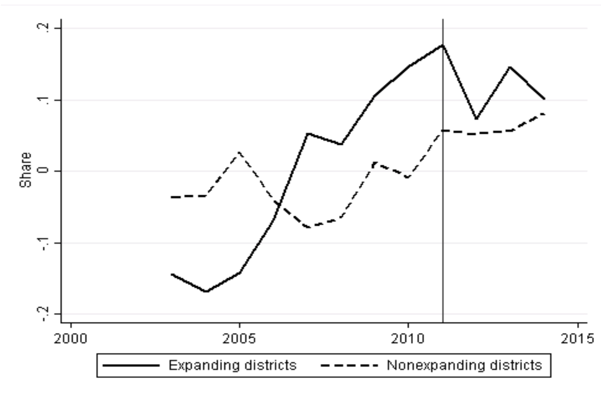
(c) Female



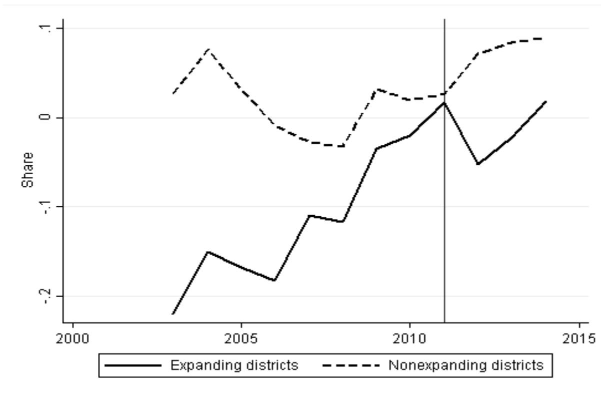
(d) Subsidized lunch



(e) Math score



(f) ELA score



Notes: This figure plots charter students' characteristics. The plain lines represent districts that experienced an increase in the share of students attending a charter school after the 2011 reform (expanding districts), and the dotted lines represent all other districts that did not experience an increase in the share of students attending a charter school.

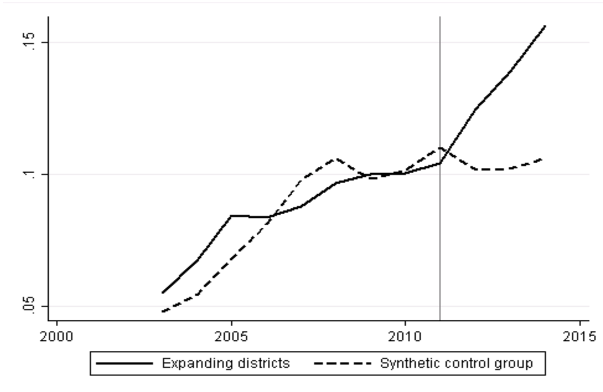
Table 4: Lottery Estimates of Charter Effects

	First stage			2SLS
	(1)	(2)	(3)	(4)
		Math		
Charter	0.455***			-0.331**
	(0.0608)			(0.117)
Charter*Urban		0.312***		0.932***
		(0.0232)		(0.126)
Charter*Post Reform			0.497***	0.0830**
			(0.0263)	(0.0320)
N	2985484	2985484	2985484	2985484
F stat	400.53	398.88	318.77	
		ELA		
Charter	0.456***			-0.160
	(0.0616)			(0.0978)
Charter*Urban		0.312***		0.398***
		(0.0230)		(0.106)
Charter*Post Reform			0.495***	0.186***
			(0.0267)	(0.0262)
N	2752583	2752583	2752583	2752583
F stat	420.89	415.74	331.89	

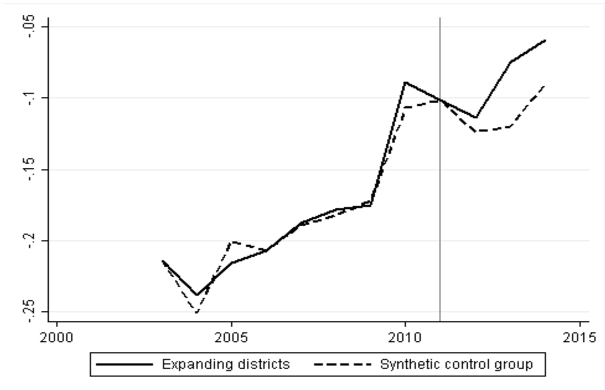
† Notes: This table reports first stage and 2SLS estimates of charter school attendance's effects on student achievement. Columns 1, 2, and 3 show estimates of the first stage coefficients, and column 4 shows estimates of the 2SLS coefficients. There are three endogenous variables: a dummy for charter school attendance, the interaction between charter attendance and a dummy for urban schools, and the interaction between charter attendance and a dummy for post-reform years. We use three sets of instruments: a lottery offer dummy, a lottery offer for an urban charter dummy, and a lottery offer for a charter school after the reform dummy. All regressions control for race, sex, special education, limited English proficiency, subsidized lunch status, and a female by minority dummy. District-by-year dummies and risk set dummies are also included. Estimates pool post-lottery outcomes for grades 4-8 and cluster by student identifier as well as district.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Figure 7: Charter Share and Students' Achievement in Expanding Districts and Synthetic Control Districts

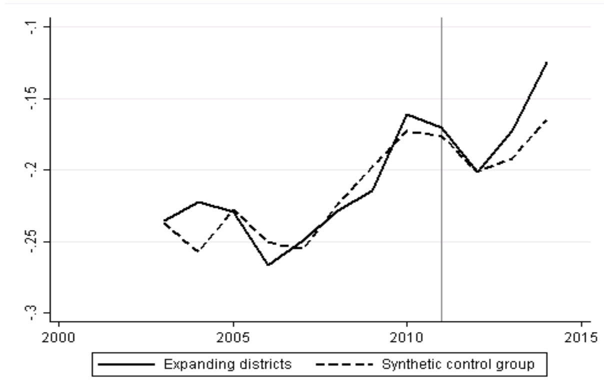
(a) Charter share



(b) Math score



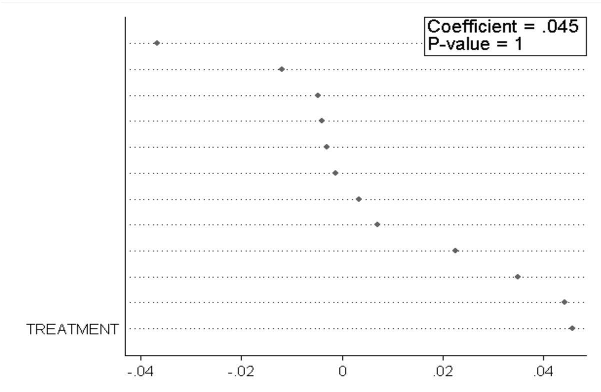
(c) ELA score



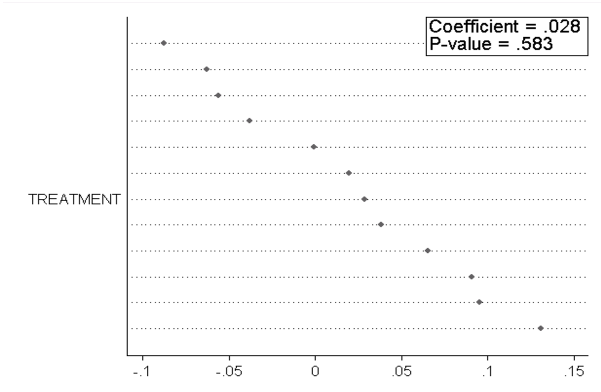
Notes: This figure plots the share of students attending a charter school (plot a) and students' average math and ELA test score (plots b and c). The plain lines represent districts that experienced an increased share of students attending a charter school after the 2011 reform (expanding districts), and the dotted lines represent the synthetic control districts. For expanding districts, we plot the average charter share (plot a), the average math test score (plot b), and the average ELA test score (plot c). For synthetic control districts, the plots represent the weighted average of the charter share or test score. We use the weights defined by the synthetic control method. The test scores used for this figure are the residuals of a regression of students' raw test scores on a set of students' demographic characteristics and a dummy for individual charter enrollment.

Figure 8: Placebo Inference for the Impact of Charter School Expansion on Student Achievement

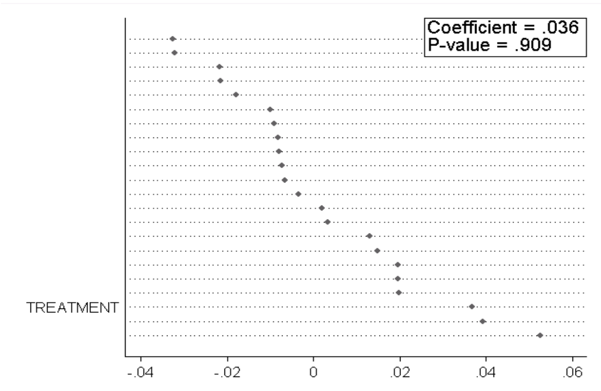
(a) Charter share (math weight)



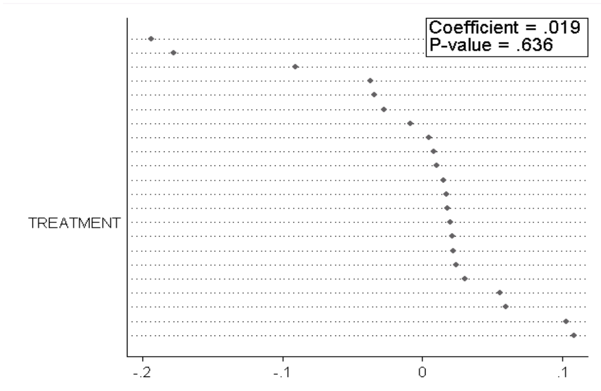
(b) Math score



(c) Charter share (ELA weight)



(d) ELA score

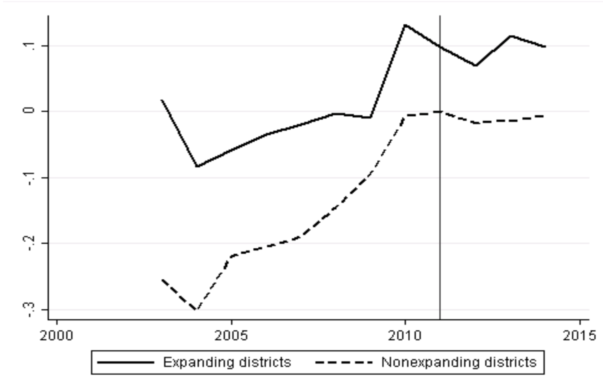


Notes: This figure plots the distribution of charter expansion's impact on student achievement, as measured by the synthetic control method. The lines "TREATMENT" report the coefficients when expanding districts are compared to their synthetic control districts. The exact value of each coefficient is reported in the top right corner of each figure. The other lines in the figures report the coefficients when a placebo district is compared to its identified group of synthetic control districts. The p-values report the probability that one of the placebo coefficients is higher than or equal to the estimated coefficient for the expanding districts.

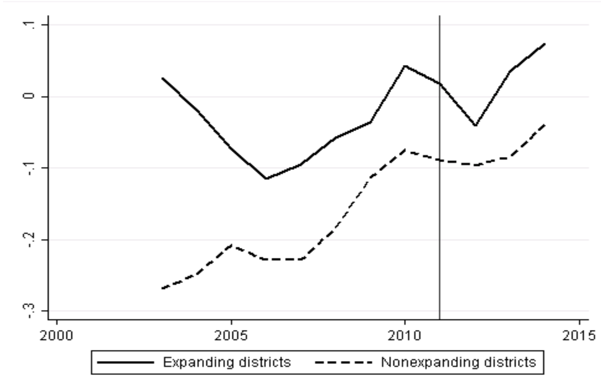
Figure 9: Pre trends in Student Achievement

Control Group A: Synthetic control districts

(a) Math score

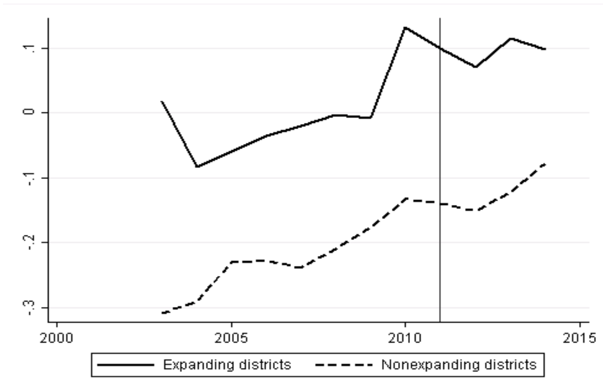


(b) ELA score

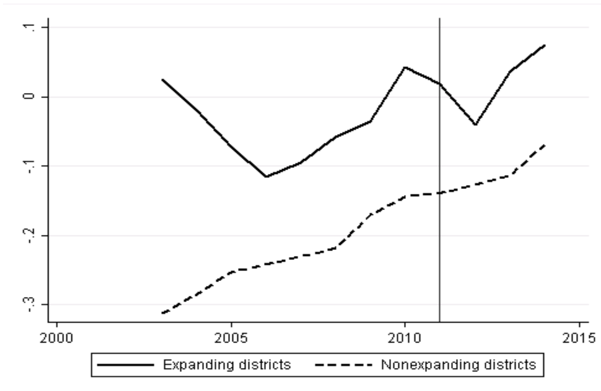


Control Group B: Bottom 10 percentile districts

(c) Math score



(d) ELA score



Notes: This figure plots student achievement in math (plots a and c) and ELA (plots b and d). The plain lines represent the district of Boston, and the dotted lines represent synthetic control districts. For the first two plots (a and b), when the synthetic control districts are used as the control group, the lines plot the average test score in these districts without using the weights defined by the synthetic control method. In the second panel (plots c and d), the control group is enlarged to all districts in the lowest 10 percentile of student achievement.

Table 5: 2SLS Estimates of Charter School Expansion's Impact on Achievement

	Math		ELA	
	First Stage	2SLS	First Stage	2SLS
	(1)	(2)	(3)	(4)
	Control group: Synthetic Control districts			
Charter share		0.6639*		0.4654*
		(0.2648)		(0.2234)
Boston * Post-reform	0.0687***		0.0659***	
	(0.0104)		(0.0092)	
Boston	1.4789		0.2479	
	(2.6863)		(1.8233)	
Post-Reform	-0.0206**		-0.0175*	
	(0.0104)		(0.0092)	
N	316001	316001	338681	338681
F-Stat	43.938		51.358	
R2		0.023		0.024
	Control group: Districts in the lowest 10th percentile			
Charter share		0.0647		-0.0492
		(0.2335)		(0.3561)
Boston * Post-reform	0.0621***		0.0621***	
	(0.0050)		(0.0046)	
Boston	-0.2847		-0.4500	
	(1.8470)		(1.7670)	
Post-Reform	-0.0140***		-0.0137***	
	(0.0050)		(0.0046)	
N	585920	585920	536720	536720
F-Stat	151.647		183.208	
R2		0.045		0.039

† Notes: This table reports first stage and 2SLS estimates of charter expansion's effect on student achievement. The endogenous variable is the charter share, which is a continuous variable that ranges from 0 to 1. The instrumental variable is the interaction between a post-reform dummy and a dummy for Boston. All regressions control for a Boston dummy, a post-reform dummy, and district time trends. Standard errors are clustered at the individual and district levels.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 6: 2SLS Estimates of Fiscal Spillovers
Robustness Check Excluding Innovation and School Redesign Grants

	Per-pupil expenditures on:				
	Total	Instruction	Fixed costs	Support services	Salaries
	(1)	(2)	(3)	(4)	(5)
	Control group: Synthetic control districts				
Charter share	1.0506	2.3060**	3.9911***	-2.6427**	0.9007
	(0.8579)	(0.9404)	(1.5103)	(1.2675)	(0.5864)
N	196	196	182	224	252
R2	0.896	0.851	0.685	0.746	0.866
F-Stat	9.5	9.9	10.9	12.4	11.9
	Control group: Districts in the lowest 10th percentile				
Charter share	0.5783	1.5784*	4.2506***	-2.8028**	0.3970
	(0.8045)	(0.8328)	(1.4729)	(1.1353)	(0.58562)
N	392	392	392	392	392
R2	0.900	0.875	0.699	0.786	0.877
F-Stat	10	10	10	10	10

† Notes: This table reports 2SLS estimates of the charter expansion's effect on districts' per-pupil expenditures. For all expenditure variables, we use the log of the variable, and we subtract the innovation and school redesign grants received. The endogenous variable is the charter share, which is a continuous variable that ranges from 0 to 1. In this over-identified model, we use three instruments: (i) the interaction between a post-reform years dummy and a Boston dummy, (ii) the interaction between a post-reform years dummy and a dummy for other urban expanding districts, and (iii) the interaction between a post-reform years dummy and a dummy for nonurban expanding districts. All regressions control for expanding districts, post-reform years, and district time trends. For standard errors, we use the White estimator of variance. When using the synthetic control districts as a control group, the number of observations varies for each outcome depending on how many synthetic control districts were identified.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Fiscal and Education Spillovers from Charter Expansion

Camille Terrier, Matthew White Ridley

APPENDIX

Table A.1: Timing of District Aid Received by Districts for Charter School Tuition

				Reimbursement							
	Change in	Individual	Current	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7	
	charter	tuition	Year	100 pct	25 pct	25 pct	25 pct	25 pct	25 pct	End of	
Year	enrollment	rate	Tuition	reimb	reimb	reimb	reimb	reimb	reimb	reimb	Total Aid
2017	0	9,900	0	0	0	0	0	0	0	0	0
2018	10	10,000	100,000	100,000	0	0	0	0	0	0	100,000
2019	0	0	0	0	25,000	0	0	0	0	0	25,000
2020	0	0	0	0	0	25,000	0	0	0	0	25,000
2021	0	0	0	0	0	0	25,000	0	0	0	25,000
2022	0	0	0	0	0	0	0	25,000	0	0	25,000
2023	0	0	0	0	0	0	0	0	25,000	0	25,000
2024	0	0	0	0	0	0	0	0	0	0	0
Total Aid Disbursed for 2017 change in tuition										0	225,000

† This table presents an example of the timeline for the temporary aid received by districts for Commonwealth charter tuition. In Massachusetts, school districts receive a full refund of the individual tuition the year following the increase in charter school enrollment and a 25 percent refund for the next five years. In this example, 10 students switch from a traditional public school to a charter school in the year 2017. The district's individual charter tuition equals $US 10,000, so the total charter school tuition payment equals $US 100,000 in 2017. In 2018, the first year after the 10 students have transferred to a charter school, the Chapter 46 aid refunds 100 percent of the total charter school tuition payment, hence $US 100,000. The state then reimburses 25 percent of the 2017 increase amount for each of the subsequent five years, which amounts to a $US 25,000 refund for years 2019 to 2024. In 2025, the state stops reimbursing the sending district. In the end, the sending district receives $US 225,000 in state refund during the six years that follows the switch.

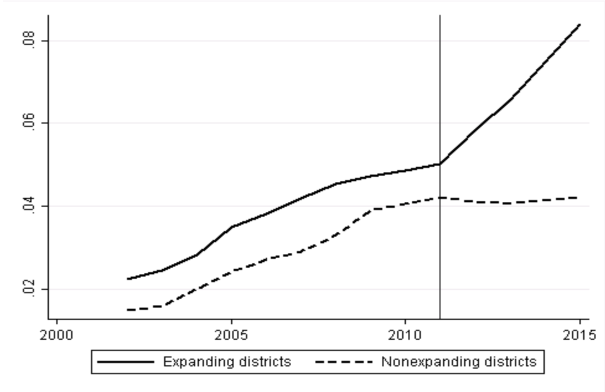
Table A.2: Massachusetts Charter Schools Eligible for the Lottery Instrument, and Grades of Lottery

Charter School Name	Town	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014
Academy Of the Pacific Rim	Boston			6	6	5-6	5	5	5	5	5	5	
Boston Collegiate	Boston					5	5	5	5	5	5	5	5
Boston Preparatory	Boston						6	6	6	6	6	6	6
Brooke - Roslindale	Boston					5	5	5					
Codman Academy	Boston												5
Excel Academy - East Boston	Boston						5	5	5	5	5	5	
MATCH Community Day	Boston						6	6	6	6	6	6	6
Roxbury Preparatory	Boston				6	6	6	6	6	6	5	5	5
Brooke - East Boston	Boston										5	5	5
Brooke - Mattapan	Boston									5	5	5	5
Excel Academy - Orient Height	Boston										5	5	
Grove Hall 2011 (UCS)	Boston									5			
KIPP Academy Boston	Boston										5	5	5
Cape Cod Lighthouse	Orleans					6	6	6	6				
Four Rivers	Greenfield	7	7	7	7	7	7	7	7				
Francis W. Parker	Devins				7	7	7	7	7				
Global Learning	New Bedford				5	5		5					
Kipp Academy Lynn	Lynn			5	5	5	5	5					
Murdoch Middle - Innovation	Tyngsboro					5	5	5	5				
Pioneer Valley Performing Arts	South Hadley				7	7	7	7	7				
Rising Tide	Plymouth							5					
Salem Academy	Salem								6				

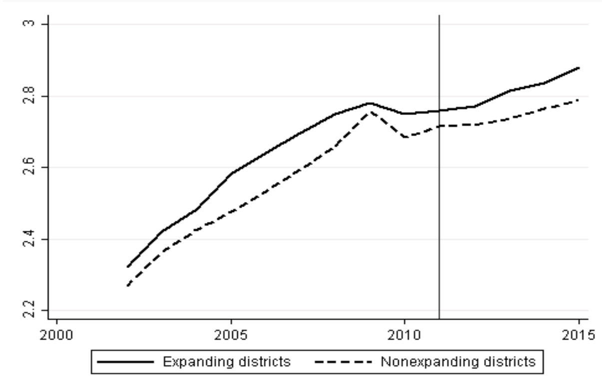
† Notes: This table lists all charter schools in Massachusetts eligible for the lottery instrument in middle school. The number in each cell indicates the grade of lottery.

Figure A.1: Pre trends in Charter Share and Districts' Per-Pupil Expenditures

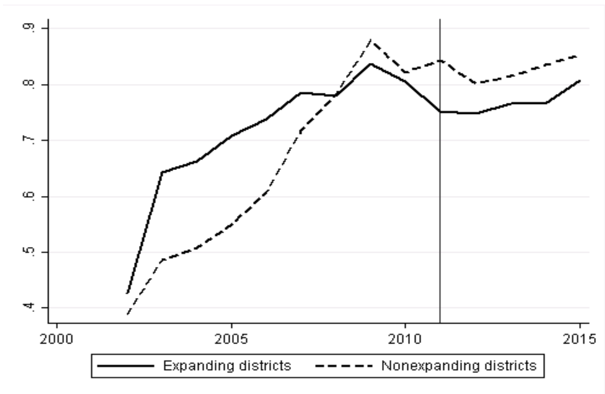
(a) Charter share



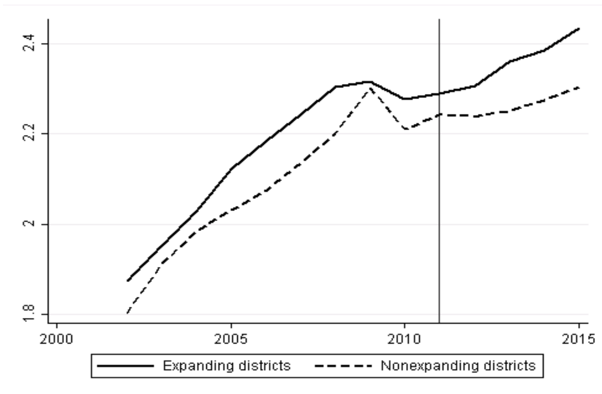
(b) Total spending



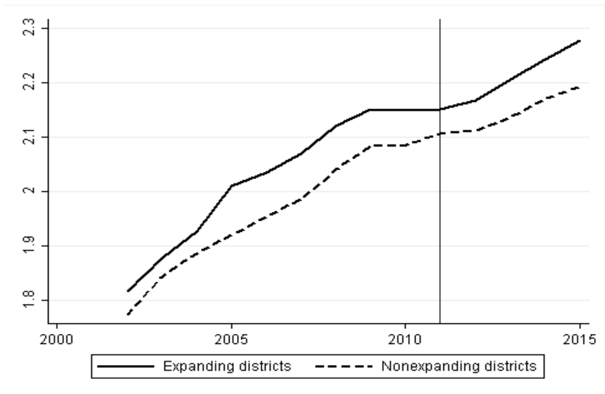
(c) Spending on fixed costs



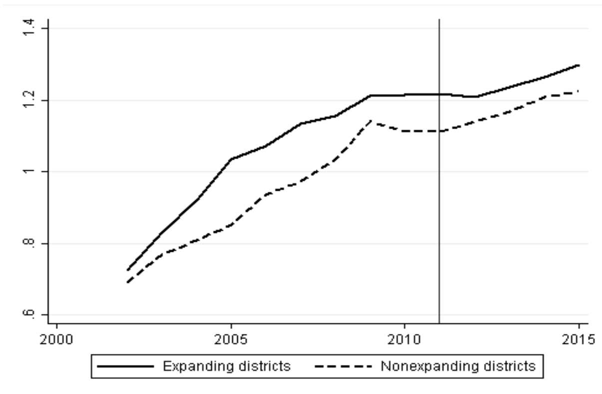
(d) Spending on instruction



(e) Spending on salaries



(f) Spending on support services



Notes: This figure plots the share of students attending a charter school (plot a), districts' per-pupil expenditures (plot b), their per-pupil expenditures on fixed costs (plot c), instruction (plot d), salaries (plot e), and support services (plot f). For all expenditure variables, we use the log of the variable. The plain lines represent districts that experienced an increase in charter school attendance after the 2011 reform (expanding districts), and the dotted lines represent districts in the lowest 10th percentile of student achievement that did not experience an increase in charter school attendance.

Table A.3: First Stage Estimates of Fiscal Spillovers

	Control group:					
	Synthetic control districts					Bottom 10
	Per-pupil expenditures on:					districts
	Total	Instruction	Fixed costs	Support services	Salaries	
	(1)	(2)	(3)	(4)	(5)	(6)
Post Reform * Boston	0.0497***	0.0502***	0.0491***	0.0526***	0.0497***	0.0469***
	(0.0095)	(0.0095)	(0.0092)	(0.0093)	(0.0091)	(0.0091)
Post Reform * Other urban	0.0159***	0.0164***	0.0153***	0.0188***	0.0159***	0.0131***
	(0.0050)	(0.0049)	(0.0043)	(0.0046)	(0.0041)	(0.0041)
Post Reform * Nonurban	0.0144**	0.0149***	0.0138***	0.0174***	0.0144***	0.0117**
	(0.0056)	(0.0055)	(0.0049)	(0.0052)	(0.0048)	(0.0048)
N	196	196	280	224	252	392
F-Stat	9.5	9.9	10.9	12.4	11.9	10

† Notes: This table reports first stage estimates of charter expansion effects on districts' per-pupil expenditures. For all expenditure variables, we use the log of the variable. The dependent variable is the charter share, which is a continuous variable that ranges from 0 to 1. In this over-identified model, we use three instruments: (i) the interaction between a post-reform years dummy and a Boston dummy, (ii) the interaction between a post-reform years dummy and a dummy for other urban expanding districts, and (iii) the interaction between a post-reform years dummy and a dummy for nonurban expanding districts. All regressions control for expanding districts, post-reform years, and district time trends. For standard errors, we use the White estimator of variance. When using the synthetic control districts as a control group, the first stage coefficients and the number of observations vary depending on how many synthetic control districts were identified for each outcome.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table A.4: 2SLS Estimates of Fiscal Spillovers - Just-identified model

	Per-pupil expenditures on:				
	Total	Instruction	Fixed costs	Support services	Salaries
	(1)	(2)	(3)	(4)	(5)
	Control group: Synthetic control districts				
Charter share	1.9218	4.4483*	2.2171	-3.3946	0.7523
	(2.0438)	(2.2917)	(3.0962)	(2.7929)	(1.4828)
N	196	196	280	224	252
R2	0.873	0.820	0.687	0.694	0.830
First stage F-Stat	15.2	16.6	19.7	24.3	22.8
First stage coefficient	0.0191***	0.0196***	0.0186***	0.0221***	0.0191***
	(0.0049)	(0.0048)	(0.0042)	(0.0045)	(0.0040)
	Control group: Districts in the lowest 10th percentile				
Charter share	0.4432	2.0078	2.8589	-3.8848	-0.5244
	(1.6718)	(1.7837)	(2.9886)	(2.6931)	(1.4644)
N	392	392	392	392	392
R2	0.872	0.849	0.700	0.720	0.829
First stage F-Stat	16.6	16.6	16.6	16.6	16.6
First stage coefficient	0.0164***	0.0164***	0.0164***	0.0164***	0.0164***
	(0.0040)	(0.0040)	(0.0040)	(0.0040)	(0.0040)

† Notes: This table reports 2SLS estimates of charter expansion effects on district spending. The endogenous variable is the charter share, which is a continuous variable that ranges from 0 to 1. In this just-identified model, the instrument is the interaction between a post-reform years dummy and a dummy for expanding districts. All regressions control for expanding districts, post-reform years, and district time trends. For standard errors, we use the White estimator of variance. When using the synthetic control districts as a control group, the number of observations varies for each outcome depending on how many synthetic control districts were identified. The first stage coefficient is the coefficient of the interaction between a post-reform years dummy and a dummy for expanding districts. *** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table A.5: First Stage Estimates of Charter School Expansion's Impact on Student Achievement

	Control group: Synthetic control districts				Control group: Bottom 10 pctile districts			
	Single instrument		Multiple instruments		Single instrument		Multiple instruments	
	Math (1)	ELA (2)	Math (3)	ELA (4)	Math (5)	ELA (6)	Math (7)	ELA (8)
Post Reform * Expanding district	0.0385**	0.0344*			0.0322*	0.0326*		
	(0.0194)	(0.0186)			(0.0174)	(0.0170)		
Post Reform * Boston			0.0676***	0.0639***			0.0612***	0.0621***
			(0.0114)	(0.0098)			(0.0078)	(0.0070)
Post Reform * Other urban			0.0116	0.0075			0.0052	0.0057
			(0.0113)	(0.0100)			(0.0077)	(0.0072)
Post Reform * Nonurban			0.0080	0.0013			0.0016	-0.0006
			(0.0117)	(0.0100)			(0.0083)	(0.0072)
N	529206	528156	529206	528156	778756	712531	778756	712531
F-Stat	3.942	3.429	82.847	250.406	3.431	3.662	86.239	261.572

† Notes: This table reports first stage estimates of charter school expansion's impact on student. The dependent variable is the charter share, which is a continuous variable that ranges from 0 to 1. In the just-identified model, the instrument is the interaction between a post-reform dummy and an expanding district dummy. In the over-identified model, we use three instruments: (i) the interaction between a post-reform years dummy and a Boston dummy, (ii) the interaction between a post-reform years dummy and a dummy for other urban expanding districts, and (iii) the interaction between a post-reform years dummy and a dummy for nonurban expanding districts. All regressions control for expanding districts, post-reform years, and district time trends. Standard errors are clustered at the individual and district levels.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

A Sensitivity Tests for the Synthetic Control Specifications

Sensitivity tests for predictor variables

For all results presented in the paper, we use five years of lagged outcome variables as predictor variables and five years of charter share. As explained in the methodological section, including lagged outcome variables and lagged charter share is crucial to ensuring that these variables' pre-reform trends are as similar as possible in expanding districts and the synthetic control. However, including too many lagged outcome variables might render other outcome predictors irrelevant (Kaul et al., 2017). In our case, the IV-SC method requires a good fit for both the outcome variable (for the reduced form estimates) and the charter share (for the first stage estimates). Therefore, it is important to check if the number of outcome and charter share lagged values impacts the fit quality for the reduced form and the first stage. In particular, we might worry that adding too many lagged values of the outcome variable makes it more difficult to get a good fit for the charter share.

The sensitivity tests reveal that reducing the number of lagged values for the outcome variable and charter share from five to either three or one systematically yields to a worse fit on outcomes, while fit on charter share is sometimes better and sometimes worse. For instance, when looking at districts' total per-pupil expenditures, the pre-reform RMSPE of the reduced form goes up from 0.0179 with five lagged values up to 0.0237 with three lagged values and 0.0248 with only one lagged value. Most importantly, reducing the number of lagged values often significantly increases the number of synthetic districts identified, a result that indicates overfitting. For instance, for districts' total per-pupil expenditures, the number of synthetic districts jumps from five to 19 when the number of lagged outcomes shrinks. Including the entire pre-reform path of the outcome variable and charter share as predictors systematically worsens fit quality for districts' expenditures. Such inclusion also generates a significantly worse fit for the charter share and a larger number of synthetic control districts when the outcome is districts' test scores.

Sensitivity tests for predictor variable weights

We test three options for the method used to compute the predictor variable weights. For the results presented in the paper, we employ an iterative optimization procedure that searches among all predictor weights matrices and sets of districts weights for the best-fitting convex combination of the control units. Best-fitting refers to the fit between the pre-reform outcomes of the treated districts and synthetic control. The second method we test is very similar, but it only runs the optimization procedure once, instead of three times in the iterative procedure. This second method should achieve either the same RMSPE for the outcome variable or a lower one. Finally, the default option to compute the variable weights is a data-driven regression-based method that has the advantage of speed, but often yields less satisfactory results in terms of minimizing the MSPE. As expected, the results in Tables A.7 and A.8 show that using either the regression-based method or the simple optimization method produce lower-quality fit for outcome variables than the iterative optimization method. Quality fit for charter share, however, is not always worse.

Sensitivity tests for donor pools

For most fiscal results presented in the paper, we use as a donor pool the districts in the lowest 10th percentile of test scores. For achievement results and results on districts' per-pupil expenditures on fixed costs, we use the districts in the lowest 25th percentile of test scores. For the latter outcomes that are more difficult to match, the larger donor pool markedly increases fit quality. That said, increasing the donor pool size also raises the risk of overfitting: a larger donor pool increases the chance that donor districts are matched because of idiosyncratic noise rather than an underlying trend shared with the expanding districts. Overfitting occurs when expanding districts are matched to a large number of donor districts, many of which have very small weights. Needless to say, we must strike a balance between having a sufficiently large donor pool, in order to have enough donor districts similar to the expanding districts, and not having too large a donor pool, to avoid overfitting. The results in tables A.7 and A.8 rule out concerns about overfitting for results on achievement and per-pupil expenditures on fixed costs. For these outcomes, the number of synthetic districts identified remains relatively constant when we increase the donor pool from the bottom 10th percentile districts to the bottom 25th percentile districts. For all other outcomes, however, overfitting is systematic with the larger donor pool. When looking at districts average test scores for instance, more than 50 districts are often identified as synthetic controls.

Finally, and perhaps most importantly, for most sensitivity tests we run, the 2011 reform's reduced form effect value is notably consistent across specifications. This is particularly true for districts' expenditures, with the exception of per-pupil expenditures on fixed costs. For that outcome, as mentioned in the section on fiscal spillover, the reduced form estimate seems very sensitive to the specification adopted.

Table A.6: Sensitivity Tests for Synthetic Control Specifications

	Specification name	Predictor variables	Number of lags	Variable weight method	Donor pool
		Outcomes: Districts' Per-Pupil Expenditures			
1	Results reported in Figures 3 and 4	Expenditures + charter share	5	3 optimizations	Bottom 10
2	1 lag for outcome and charter share	Expenditures + charter share	1	3 optimizations	Bottom 10
3	3 lags for outcome and charter share	Expenditures + charter share	3	3 optimizations	Bottom 10
4	All lags for outcome and charter share	Expenditures + charter share	9	3 optimizations	Bottom 10
5	Additional predictor variables	Expenditures + charter share + cov	5	3 optimizations	Bottom 10
6	Variable weights: Regression based	Expenditures + charter share	5	Regression based	Bottom 10
7	Variable weights: Single optimization	Expenditures + charter share	5	1 optimization	Bottom 10
8	Donor pool: Bottom 25th percentile	Expenditures + charter share	5	3 optimizations	Bottom 25
		Outcomes: Districts' Average Test Scores			
9	Results reported in Figures 7 and 8	Test score + charter share	5	3 optimizations	Bottom 25
10	1 lag for outcome and charter share	Test score + charter share	1	3 optimizations	Bottom 25
11	3 lags for outcome and charter share	Test score + charter share	3	3 optimizations	Bottom 25
12	All lags for outcome and charter share	Test score + charter share	9	3 optimizations	Bottom 25
13	Additional predictor variables	Test score + charter share + cov	5	3 optimizations	Bottom 25
14	Variable weights: Regression based	Test score + charter share	5	Regression based	Bottom 25
15	Variable weights: Single optimization	Test score + charter share	5	1 optimization	Bottom 25
16	Donor pool: Bottom 10th percentile	Test score + charter share	5	3 optimizations	Bottom 10

† Notes: This table presents the features of each synthetic control sensitivity test. For purposes of comparison, the first row of each panel presents the baseline specification used through the paper. The specifications 2 and 10 include only one lagged value for the outcome variable and charter share. They use the average value of the outcome variable over the pre-reform years 2003 to 2011. The specifications 3 and 11 include 3 years of lagged values. They use years 2003, 2007, and 2011 for the lags. The specifications 4 and 12 include all lagged values, that is the entire pre-reform path of the outcome variable and charter share as predictors (for the years 2002 to 2011). For outcomes on districts' expenditures, the additional predictor variables (specification number 5) are the percentage of black, Hispanic, female, special education, limited English proficient, subsidized lunch, and minority female students. For outcomes on districts' test scores, the additional predictor variables (specification number 13) are districts' per-pupil total expenditures and per-pupil expenditures on fixed costs, instruction, support services, and salaries. Details on specifications number 6, 7, 8, 14, 15, and 16 are provided in the text.

Table A.7: Results of Sensitivity Tests for the Synthetic Control Specifications

	Number of SC districts (1)	RMSPE of the first stage (2)	RMSPE of the reduced form (3)	Treatment effect (reduced form) (4)
	Outcome: Districts' Total Per-Pupil Expenditures			
Results reported in Figures 3 and 4	5	0.0026	0.0179	0.0487
1 lag for outcome and charter share	19	0.0025	0.0248	0.0179
3 lags for outcome and charter share	19	0.0012	0.0237	0.0477
All lags for outcome and charter share	7	0.0028	0.0159	0.0500
Additional predictor variables	7	0.0064	0.0163	0.0514
Variable weights: Regression based	5	0.0027	0.0188	0.0501
Variable weights: Single optimization	5	0.0022	0.0182	0.0550
Donor pool: Bottom 25th percentile	55	0.0014	0.0162	0.0160
	Outcome: Districts' Per-Pupil Expenditures on Fixed Costs			
Results reported in Figures 3 and 4	4	0.0174	0.0469	-0.0149
1 lag for outcome and charter share	19	0.0022	0.0808	-0.0833
3 lags for outcome and charter share	4	0.0103	0.0477	-0.0190
All lags for outcome and charter share	4	0.0071	0.0428	-0.0388
Additional predictor variables	5	0.0071	0.0450	-0.0310
Variable weights: Regression based	6	0.0022	0.0889	0.0044
Variable weights: Single optimization	5	0.0053	0.0549	-0.0465
Donor pool: Bottom 25th percentile	11	0.0015	0.0379	0.0623
	Outcome: Districts' Per-Pupil Expenditures on Instruction			
Results reported in Figures 3 and 4	5	0.0025	0.0229	0.0724
1 lag for outcome and charter share	19	0.0019	0.0287	0.0520
3 lags for outcome and charter share	19	0.0017	0.0306	0.0702
All lags for outcome and charter share	6	0.0025	0.0151	0.0592
Additional predictor variables	6	0.0058	0.0199	0.0678
Variable weights: Regression based	7	0.0018	0.0257	0.0823
Variable weights: Single optimization	5	0.0025	0.0229	0.0724
Donor pool: Bottom 25th percentile	54	0.0013	0.0239	0.0125
	Outcome: Districts' Per-Pupil Expenditures on Support Services			
Results reported in Figures 3 and 4	7	0.0024	0.0176	-0.0457
1 lag for outcome and charter share	19	0.0028	0.0424	-0.0304
3 lags for outcome and charter share	18	0.0054	0.0241	-0.0439
All lags for outcome and charter share	8	0.0035	0.0162	-0.0424
Additional predictor variables	7	0.0046	0.0190	-0.0348
Variable weights: Regression based	6	0.0026	0.0223	-0.0381
Variable weights: Single optimization	7	0.0024	0.0176	-0.0457
Donor pool: Bottom 25th percentile	54	0.0010	0.0128	-0.0346

† Notes: See next table.

Table A.8: Sensitivity Tests for the Synthetic Control Specifications (continued)

	Number of SC districts (1)	RMSPE of the first stage (2)	RMSPE of the reduced form (3)	Treatment effect (reduced form) (4)
	Outcome: Districts' Per-Pupil Expenditures on Salaries			
Results reported in Figures 3 and 4	9	0.0013	0.0177	0.0508
1 lag for outcome and charter share	19	0.0020	0.0235	0.0303
3 lags for outcome and charter share	19	0.0009	0.0220	0.0479
All lags for outcome and charter share	8	0.0023	0.0110	0.0305
Additional predictor variables	7	0.0072	0.0130	0.0372
Variable weights: Regression based	7	0.0007	0.0226	0.0530
Variable weights: Single optimization	9	0.0013	0.0177	0.0508
Donor pool: Bottom 25th percentile	53	0.0014	0.0122	0.0264
	Outcome: Districts' Average Test Scores in Math			
Results reported in Figures 7 and 8	7	0.0139	0.0090	0.0288
1 lag for outcome and charter share	55	0.0446	0.1314	0.0496
3 lags for outcome and charter share	270	0.0089	0.0211	-0.0088
All lags for outcome and charter share	54	0.0543	0.0001	-0.0086
Additional predictor variables	8	0.0253	0.0165	0.0440
Variable weights: Regression based	7	0.0135	0.0144	0.0354
Variable weights: Single optimization	7	0.0143	0.0090	0.0296
Donor pool: Bottom 10th percentile	7	0.0143	0.0090	0.0296
	Outcome: Districts' Average Test Scores in ELA			
Results reported in Figures 7 and 8	6	0.0090	0.0150	0.0198
1 lag for outcome and charter share	49	0.0245	0.0686	0.0075
3 lags for outcome and charter share	228	0.0196	0.0143	0.0126
All lags for outcome and charter share	49	0.0617	0.0003	0.0029
Additional predictor variables	11	0.0118	0.0097	0.0289
Variable weights: Regression based	7	0.0066	0.0269	-0.0032
Variable weights: Single optimization	6	0.0122	0.0118	-0.0112
Donor pool: Bottom 10th percentile	6	0.0122	0.0118	-0.0112

† Notes: This table reports results of sensitivity tests done for the synthetic control method. For purposes of comparison, the first row of each panel presents the baseline specification used through the paper. The upper panel shows results when the outcome variable is districts' total per-pupil expenditures. Moving down the table, we document results when the outcome variable is districts' per-pupil expenditures on fixed costs, instruction, and support services. The first column shows the number of synthetic control districts identified by the synthetic control algorithm. The root mean squared prediction error (RMSPE) measures the fit quality between expanding districts' pre-reform path and nonexpanding districts' outcome path (column 2) and charter share path (column 3). In column 4, the reduced form treatment effect estimate is the average post-reform gap between the expanding districts' outcome and the weighted average outcome of the synthetic control districts.