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Matthew Ridley
Camille Terrier

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ABSTRACT

The fiscal and educational consequences of charter expansion for non-charter students are central issues in the debate over charter schools. Do charter schools 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 variables (IV-SC) and differences-in-differences instrumental variables (IV-DiD) estimators. The results suggest greater charter attendance increases per-pupil expenditures in traditional public schools and induces them to shift expenditure from support services to instruction and salaries. At the same time, charter expansion has a small positive effect on non-charter students' achievement.

Matthew Ridley
MIT
Department of Economics
77 Massachusetts Avenue,
Cambridge 02142
mridley@mit.edu

Camille Terrier
Faculty of Business and Economics
University of Lausanne
Internef 553
CH-1015 Lausanne-Dorigny
Switzerland
cterrier@mit.edu

An online appendix is available at: https://www.nber.org/data-appendix/w25070/

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, rich literature on charter school effectiveness (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 traditional public schools by exploiting a 2011 reform that raised the cap on charter schools in Massachusetts.

Causal evidence on the fiscal impact of charter schools is very limited (Epple et al., 2015), yet this question is central to charter school policy debates. Charter school proponents claim that increased competition generated by charter expansion might induce districts to strategically reallocate spending (Hoxby, 2003).[2] Opponents, meanwhile, argue that charter schools drain resources from traditional public schools. In most states, when a student switches from a traditional public school to a charter school, the funding follows the student. This might force school districts facing fixed costs to cut per-pupil spending on variable outlays, hampering their ability to respond to competition. To counteract this, however, several states, including Massachusetts, have refund schemes that temporarily compensate school districts for lost revenues due to charter expansion. Taking all these forces together, the net fiscal impact of charter expansion is unclear (Arsen and Ni, 2012; Bifulco and Reback, 2014).[3]

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 salaries. Understanding the potential fiscal impact of charter expansion is fundamental

[1]See also Epple et al. (2015) for an excellent literature review on charter schools.

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

[3]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.

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 teacher salaries, which are more variable costs.

A second focal point in the debate on charter expansion is its spillover effects on non-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 upwardly 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).[4] To deal with this endogeneity, we exploit a Massachusetts reform that led to charter sector expansion. In 2011, the state raised the limit on district funding allocable to charter schools from 9% to 18% in districts with student performance in the lowest 10% of districts statewide.[5] In the four years following the reform, the share of students attending charter schools 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 district charter share. A potential concern with this is that expanding districts are non-randomly selected. The 2011 reform induced some, but not all, eligible districts to expand their charter sector.

To account for district selection into expansion, we start by building a synthetic control for the expanding districts (Abadie and Gardeazabal, 2003; Abadie et al., 2010, 2015).[6] This method creates a data-driven weighted average of a small number of control districts that closely matches 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 ex-

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

[5]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.

[6]An alternative would have been to use districts' pre-determined eligibility for charter expansion as an instrument for the endogenous expansion. We ruled out this strategy, however, because the take-up was too low.

penditures and charter share.[7] 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 with a differences-in-differences instrumental variables (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 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 both increases per-pupil expenditures and shifts districts' expenditures towards instruction and away from support services. The IV-SC method shows that, after the reform, total per-pupil expenditures increased by 4.8 % more in expanding districts than in nonexpanding districts. In addition, districts reallocate their resources. Per-pupil expenditures on instruction and salaries increased more in expanding districts than in nonexpanding districts by 7.5% and 5.2%, respectively. On the other hand, per-pupil expenditures on support services drop by 4.4% more in expanding districts.[8] The IV-DiD estimates confirm these results and provide additional evidence for the competition and fixed costs mechanisms.

We then investigate the impact of charter expansion on student achievement. This raises additional challenges in terms of identification. Ample evidence shows that charter students differ from non-charter students and that this selection changes when charter schools expand (Epple et al., 2015; Baude et al., 2014; Cohodes et al., 2016). 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).[9] 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

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

[8] The synthetic control method relies on a number of choices made regarding the outcome predictor variables, the donor pool, and the method used to compute the predictor variable weights. We test the robustness of our results by showing results for six different specifications.

[9] Controlling for individual charter enrollment requires us to account for student selection into charter schools. We use charter admissions lottery offers as 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).

robustness check.

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 that charter expansion makes a positive and significant impact on student 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 student achievement (Bettinger, 2005; Imberman, 2011; Zimmer and Buddin, 2009; Davis, 2013; Sass, 2006; Winters, 2012).

Our results stand in contrast to prior studies that find a negative fiscal impact of charter expansion on traditional school districts (Arsen and Ni, 2012; Bifulco and Reback, 2014; Ladd and Singleton, 2018; Cook, 2018). This may be because we provide the first causal evidence in a state that temporarily compensates districts for the revenue they lose when students move to a charter school.[10] The effect of charter expansion in such contexts is highly policy relevant, as several states, among them Illinois, New Hampshire, New York, and Pennsylvania, have adopted temporary refund schemes. However, with a temporary refund scheme, we might also expect the long- and short-run effects of charter expansion to differ. What happens to district expenditures and student achievement after the refund ends?

To look at the longer-run, post-refund consequences of charter expansion, we analyze charter school openings prior to 2011. We use an event-study analysis of all charter school openings that persistently raised the charter share. Reassuringly, this method largely replicates our previous results over the three- to four-year timeline. Our event study suggests that the effects of charter expansion on both expenditures and achievement vanish in the longer run, and in particular after the refund ends. Interestingly, the positive effects on achievement are largest five to six years after opening, which fits with previous findings that it takes several years for increased spending to impact achievement (Jackson et al., 2016; Lafortune et al., 2018). These results indicate that the refund scheme may be effectively insulating districts from the short-run financial shock due to expansion so that they can adjust in the long run.

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). 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

[10]Bifulco and Reback (2014) present case studies of traditional public schools adapting to enrollment declines in Albany and Buffalo, New York, where a temporary aid program exists. They do not provide causal estimates, however.

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.

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, 29 approved charter schools have either never opened, closed, or surrendered their charters.

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.[11] 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.[12] In 2010, a legislative amendment to the charter school statute established the current

[11] 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`.

[12] In 1997, the numerical cap was raised to 50. The funding cap only applies to Commonwealth charter schools, which represent the vast majority of the charter sector. In 2016-2017, 71 of the 80 operating charter schools were Commonwealth.

funding cap provisions for charter schools. In districts with student performance in the lowest 10% of districts statewide, 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% 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, i.e., existing operators with strong track records.[13] 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.[14] With applications limited to proven providers and the rigorous Massachusetts Board of Elementary and Secondary Education approval process, only a subgroup of low-performing districts 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 high excess 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 were, 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).[15]

Figure 2 reports the charter share evolution for middle schools only. Charter enrollment grew at the elementary and high school levels, though not as dramatically as in middle schools, where the proportion of students attending a charter school jumped from 10% to 15% in expanding districts. For that reason, we focus on middle school students when studying charter expansion effect on student achievement. However, we analyze fiscal spillovers for all levels – that is, primary, middle, and high schools – as the expenditure variables are not decomposed by level.

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

[14]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`.

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

Districts Expenditures on Charter Schools and Traditional Public Schools

The vast majority of charter school 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 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.[16] Appendix Table A.1 presents an example that describes the timing of 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.[17]

These state aid programs mean that for several years after a student switches from a traditional public school to a charter school, the district is reimbursed for at least some of the (average) amount it would have spent on that student. As a result, all else equal, the district's revenue per pupil enrolled will increase 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,

[16]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`.

[17]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.

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 traditional public schools in 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 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. In addition, spending on fixed costs (typically building maintenance or debt interest) might not generate student progress to the same extent as spending on more variable costs such as instruction, textbooks, or teachers' salaries. Second, we provide the first causal evidence of the fiscal impact of charter expansion in a state that provides temporary aid for traditional schools losing students to charters. Understanding whether the fiscal impact of charter expansion depends on the availability of such temporary support is key for policy makers across the U.S. Several states, such as Illinois, New Hampshire, New York, and Pennsylvania, have adopted temporary refund schemes. 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 Massachusetts drew national attention (The New York Times, 2016). 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 more frequent discussions on the potential fiscal effect of raising these caps 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.[18] We can therefore 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, and their expenditures on (2) fixed costs, (3) 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 in that district. Expenditures on fixed costs are the sum of expenditures on plant operation and maintenance, 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.[19] 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 encompass student support (such as counseling), teacher support (such as training or instruction supervision), and school administration.[20] We use district identifiers to match data on expenditures to

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

[19]Spending on instruction includes salaries.

[20]More specifically, expenditures on support services are the sum of several expense types. First are expenditures for administrative, guidance, health, and logistical support, including social work, student accounting, counseling, student appraisal, information, and placement services, as well as medical, dental, nursing, psychological, and speech services. Expenditures on support services also encompass expenditures for instruction supervision, curriculum development, instructional staff training, academic assessment, and media, library, and instruction-

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, were not oversubscribed, or served alternative students (like students at risk of dropping out). The resulting lottery sample includes 13 charter schools in Boston and 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 nonexpanding 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' nonrandom initial location and expansion (Glomm et al., 2005; Bifulco and Buerger, 2015).[21] 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. The charter share remained relatively constant, at about 3%, in all other districts. For identification, we exploit this differential growth in expanding versus nonexpanding districts to generate an instrument for districts' charter share. We simultaneously use a synthetic control approach to

related technology services. Support tasks also relate to school administration, including expenditures for the principal and school office.

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

deal with potential non-random selection of districts into expansion.

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. [22] 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, and Lowell and Chicopee, because they only had very marginal changes in slopes after the reform and idiosyncratic evolution patterns that do not seem related to the cap reform.

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, before the reform, the cap was not equally binding for all districts. Districts for whom the cap was binding might be expected to take more advantage of the reform to expand their charter sector. Second, low-performing districts can only increase charter enrollment if 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.[23] 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 address the selection into expansion among eligible districts, an obvious solution would

[22]We only look at expansions that are triggered by the 2011 reform. Charter schools have also opened and expanded in other years but to a significantly lesser extent. Between 1 and 5 charter schools opened in each year from 2002 to 2010, while 13 charter schools opened in 2011.

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

be to use the eligibility criteria as instruments for the expansion. Unfortunately, most of the eligibility criteria are poor predictors of district charter expansion. The first stage is low. Instead, we use the differential change in charter share in expanding and nonexpanding districts as an instrument. Using that differences-in-differences as an instrument raises the question of the appropriate control group of nonexpanding districts: Should we use all nonexpanding districts, low-performing nonexpanding districts that were eligible to expand, or a subset of these districts that are most similar to the expanding districts? To identify the appropriate control group, we start by building a synthetic control (SC) for the expanding districts. Because SC has never been used in an instrumental variables framework, we develop a synthetic control instrumental variables approach (IV-SC), which we complement with a more standard differences-in-differences instrumental variables (IV-DiD) estimator.

4.1 Synthetic Control Instrumental Variables (IV-SC)

We use the synthetic control strategy devised by Abadie et al. (2010) 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 ρ from equation (1) directly by OLS because C_{jt} is potentially correlated with district-specific unobservables v_{jt}. Therefore, we instrument C_{jt} with a dummy for expanding districts, E_{jt}, that takes the value one for expanding districts and zero for the synthetic control districts. A dummy for expansion would clearly be endogenous when expanding districts are compared to all other districts. Comparing expanding districts to their synthetic control provides a more plausibly exogenous instrument. The first stage and reduced-form equations are:

$$C_{jt} = \gamma_2 + \beta E_{jt} + u_{jt} \tag{2}$$

$$Y_{jt} = \gamma_1 + \alpha E_{jt} + \xi_{jt} \tag{3}$$

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 $(\alpha_{j,T_0+1}, ..., \alpha_{j,T})$. 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 assume λ_t is 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 to equalize three equations for each pre-reform year. 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:

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

Importantly, 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 variables model.

If the vector of weights $(w_2^*, ..., w_{J+1}^*)$ exists, Abadie et al. (2010) show that the reduced form treatment effect α_{jt} in equation 6 can be estimated by: [24]

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

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

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

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

The same weights are used for the first stage and the reduced form. This ensures that the control group is the same for both stages. [25]

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 $\hat{\beta}$:[26]

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

In practice, this IV-SC estimator can be obtained either by estimating the first stage and the reduced form separately, and then 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. When several units are treated, a synthetic control can be computed for each treated unit separately or for the group of treated units as a whole. We chose the latter option in our application.

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.

Finally, note that although we call our method “IV-SC”, it differs in important ways from methods that use instrumental variables to eliminate bias due to selection into treatment. We instead use the synthetic control method to estimate an unbiased reduced-form treatment effect, and we then scale this effect by the first stage, which we estimate using the same group of weighted control districts. We think, however, that the IV-SC terminology illustrates well the novel attempt to use a dummy variable for treated versus synthetic control as an instrumental variable.

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

[26] 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.

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 between treated districts and synthetic districts can only be obtained if the values of the predictor variables for the treated units fall within the convex hull of the values for the potential synthetic control districts' predictor variables. When the equality does not hold perfectly, 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. Appendix B presents sensitivity tests in which more and fewer lagged years are used as predictor variables and in which 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 scores 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.[27]

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 which have 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. Appendix B 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)). Appendix B presents sensitivity tests in which we use the cross-validation method.

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 depends exclusively on the number of students enrolled in charter schools 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. In expanding districts, 91% of charter school students come from the district in which the school is located.

[27]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.

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 variables 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.

Secondly, in our IV-SC context, the independence assumption states that mean potential outcomes are the same in expanding districts and in the synthetic control. As Abadie et al. (2010) show, this holds if treatment is independent of potential outcomes conditional on a set of unobserved time-varying common factors with time-invariant district-specific factor loadings. Note that our approach therefore allows for districts to select into charter expansion based on expected future trends in outcomes, so long as these trends have the factor variable structure described just above. In other words, future trends in potential outcomes can differ between expanding and synthetic control, so long as the post-reform trend difference has the same factor variable structure as the pre-reform trends. In practice, because our synthetic control matches well the evolution of the outcome variable in expanding districts during the ten years prior to expansion, observed and unobserved determinants of per-pupil expenditures are very likely to have evolved in the same way. Such a similar evolution before the cap reform makes it unlikely that charter providers expect per-pupil expenditures, as well as their observed and unobserved determinants, to evolve differently in the future in expanding and synthetic control districts.

In our context, given the risk of non-parallel trends in district 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 hold if no other reform was adopted in 2011 or if the reforms adopted that year impacted student achievement equally in expanding and nonexpanding districts. Other reforms were indeed adopted in 2010: The Act Relative to the Achievement Gap included provisions for school turnarounds and the creation of innovation schools (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 rather on the entire population of districts in Massachusetts. Due to the absence of sampling variation, all 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 as described in Abadie et al. (2010). Following their approach, we sequentially apply the synthetic control algorithm to a random selection of nine districts drawn from the pool of potential controls.[28] 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 each combination of nine placebo districts 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 (Ferman and Pinto, 2017).[29]

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

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

[29] In practice, there is no consensus on what pre-intervention fit is good enough for placebo units. Considering all placebo units might lead to over-rejection, while restricting the placebo units too much might lead to under-rejection (Ferman and Pinto, 2017). Considering placebo districts for which the RMSPE is not more than three times larger than the RMSPE of the expanding districts seems relatively conservative.

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 year and expanding district 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.

Three 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. In addition, 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.[30]

Finally, additional assumptions are required in our fuzzy-DiD environment (de Chaisemartin and D'HaultfŒuille, 2018). Both the expanding districts and the control group are always partially treated, in the sense that they have a non-zero charter share before and after the reform. This means we need two extra assumptions for identification in addition to the standard common trend assumption. First, in both expanding and non-expanding districts, the average treatment effect among districts that had a positive pre-reform charter share should remain stable over time. This assumption seems relatively plausible because the institutional features accompanying the charter expansion are the same before and after the 2011 reform.

[30]See Hudson et al. (2017) for a discussion of the assumptions underlying the IV-DiD.

In particular, the refund scheme does not change after the reform. The second assumption is that the treatment effect is the same in the treatment and in the control group. This assumption, however, is only required if the treatment intensity changes in the control group after the reform. Appendix figures A.2 and A.3 show that the charter share evolution for each of the control groups we use is relatively stable post-reform. Any bias generated by differential treatment effects between the treatment and control groups should therefore be very limited.

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 nonexpanding districts 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 charter schools.

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: 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 both raises districts' total per-pupil expenditures and shifts expenditures from support services to instruction. We use the synthetic control method separately for each of the five expenditure outcomes in which we are interested. For each expenditure variable, we use the log of the variable as our outcome. 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%), followed by Southbridge, Athol-Royalston, and Somerville, which receive weights of 19.4, 19.2, and 14.9%, respectively. North Adams has the smallest weight at 2.3%. 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.[31] 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 district expenditures in expanding districts and the 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 5% to more than 8%.[32] 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 have increased by, on average, 4.8% in expanding districts compared to the synthetic control group; per-pupil expenditures on fixed costs have increased by 6.2%; per-pupil expenditures on instruction by 7.2%; and per-

[31]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.

[32]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 the charter share directly calculated from student-level data.

pupil expenditures on salaries by 5%. These increases are accompanied by a 4% reduction in per-pupil expenditures on support services.[33]

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 the control districts with high RMSPEs 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 group of placebo districts. The p-value, which calculates the probability of obtaining an estimate at least as large as the one obtained for the expanding districts when the treatment is reassigned at random, is equal to zero. Changes in total per-pupil spending, spending on fixed costs, instruction, salaries, and support services also appear to be large, with p-values ranging from 0.051 to 0.162.[34]

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 increases, although not 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 four years after the reform) would increase per-pupil expenditures on fixed costs by 5.8% per year. At the same time, per-pupil expenditures on instruction would also increase by 2.9% per year, meaning that increased per-pupil expenditures are not solely driven by fixed costs. With regard to the competition effect, districts seem

[33]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 increasing trends in per-pupil expenditures between 2000 and 2010 are partly due to declining enrollment trends during that period.

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

to shift resources away from support services. For a 1.25 percentage point increase in charter share per year, we find that the aforementioned positive effect on instruction expenditures would be compensated by an 3.2% drop in support services expenditures per year. Interestingly, these estimates are very similar to the ones we obtain with the IV-SC method.

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 5 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 of charter expansion are particularly interesting as they stand in contrast to prior studies that all find charter expansion has a negative fiscal impact on district spending. The fiscal impact of charter schools surely depends on whether or not a state has a refund scheme. Massachusetts has one, but the states previously studied (such as Michigan, Ohio, and North Carolina) do not, which might explain the negative fiscal spillovers observed by, for example, Arsen and Ni (2012), Ladd and Singleton (2018), and Cook (2018).[35] 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.[36] Their estimates, made under different scenarios, suggest charter expansion has a negative impact. However, Bifulco and Reback (2014) do not measure causal effects of charter expansion, which might explain why our results differ.

Our results show that districts facing the largest charter expansion tend to reduce their per-pupil expenditures on support services while increasing their expenditures on instruction. A competition effect might justify such a reallocation. When faced with the threat of losing students, traditional public schools might think more carefully about how to spend their limited resources in order to retain existing students and attract new ones. Because parents factor school ratings information into student enrollment decisions (Hastings and Weinstein, 2008), charter competition creates incentives for traditional public schools to reallocate resources in a way that boosts student achievement (Hoxby, 2003).

The fact that schools switch their resources from support services to instruction also suggests they may perceive spending on instruction as more directly related to student progress

[35]Cook (2018) finds that charter competition not only reduces state and federal revenues for traditional public schools, but also revenues raised through property taxes by depressing district-level residential property values.

[36]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 that in place in Massachusetts.

than spending on support services. However, some evidence shows that cutting spending on items such as pupil support, which is included in support services, is detrimental to students' attainment (Carrell and Hoekstra, 2014; Carrell and Carrell, 2006; Reback, 2010). We investigated this by decomposing support services spending into its main components and running a synthetic control for each; these results are in Appendix A. We do find a negative effect on pupil support spending, suggesting that cuts in support services may not be costless, though this effect is not statistically significant.

Finally, the increased per-pupil expenditures generated by charter expansion and the shift of resources we observe from support services to instruction and salaries raises questions about the impact of charter school expansion on student achievement. A large literature has linked per-pupil expenditures and student achievement (Jackson et al., 2016; Lafortune et al., 2018; Hyman, 2017; Card and Payne, 2002; Hoxby, 2001). Jackson et al. (2016) find that a 10% increase in per-pupil spending each year for all 12 years of public school leads to 0.27 more completed years of education. In Massachusetts, Guryan (2003) finds that a per-pupil expenditures increase of 1 standard deviation increases test scores by approximately 0.5 standard deviation.

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

We are interested in how charter expansion affects the achievement of those students who are left behind in the traditional public sector. Unfortunately, we cannot do this simply by estimating the causal effect of charter expansion on average traditional sector test scores. This is because charter expansion, by definition, also changes *who* is left behind in traditional public schools. Unless new charter enrollees are sampled randomly (or at least orthogonally to ability) from the traditional public school population, there will be a clear selection bias: expansion affects the baseline ability of the average public school student, and therefore average test scores.

Ample evidence confirms that this concern is not just theoretical. Selection into charter schools is non-random, correlated with achievement, and changes as 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".

In our own data, selection in charter schools also appears to change with expansion. As shown in Figure 6, before the reform, expanding districts were already growing in terms of charter share, while nonexpanding districts experienced no growth. Furthermore, 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.

The dynamic selection of students into charter schools implies that students in traditional public schools are also an increasingly selected sample as charter schools expand. Therefore, simply regressing test scores on charter share within the sample of public school students would introduce a selection bias term that is correlated with charter share, and we will not recover causal estimates of the spillover effect.

Unlike most previous studies, we address this problem by using a two-endogenous-variable approach which estimates the causal impact of charter expansion on *all* students, while accounting for the direct treatment effect of charter enrollment on charter students. Specifically, our regressions include both district-wide charter share and individual charter enrollment as explanatory variables. The effect of charter share when controlling for individual enrollment can then be interpreted as the spillover effect of charter schools.

Controlling for individual charter enrollment requires us to account for student selection into charter schools: a large body of literature suggests that charter applicants have different observed and unobserved characteristics than non-charter applicants. We therefore use a lottery instrument to recover consistent estimates of charter effectiveness.[37]

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 boards of governors with track records of high performance. Consistent with this, Cohodes et

[37] On charter school effectiveness and the 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.

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.[38] 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 increased 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 enrollment, $C_i * Urb_i$ is a dummy for enrollment 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 district-by-year fixed effects that capture the remaining variation in achievement we are interested in. More specifically, the district-by-year fixed effects estimate the district-by-year level variation in test scores, once we have accounted for charter effectiveness and students' demographics. This is the outcome variable we use in our synthetic control analysis.

When controlling for the charter effect, we allow this effect to vary along two dimensions: whether the charter school is located in an urban area and whether the charter effect is estimated

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

before or after the 2011 reform.[39] In the second step, we simply apply the synthetic control algorithm using $\hat{\mu}_{dt}$ 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 charter lottery 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. Charters 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 district-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 on a post-reform dummy P_{idt}, θ_2 is the coefficient on a dummy for expanding districts E_{id}, and ϵ_{idt} is an error term. Our treatment variable, C_{idt}, measures the share of students enrolled in a

[39] 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.

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.[40]

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 levels.

7 Results on Education Spillover of Charter Expansion

Our results show that increased charter attendance positively impacts traditional public school 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.[41]

However, placebo inference shows that these test score gains are not statistically different from gains in placebo districts. Figure 8 reports the estimate of expanding districts' treatment effect as well as the placebo effects.[42] This figure also shows the p-value, which is the proba-

[40]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.

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

[42]As for expenditure outcomes, we only keep the placebo districts with RMSPEs that are no larger than three times the expanding districts' RMSPE. This explains why the number of placebo districts varies by outcome.

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 the 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 student 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 student achievement by 0.033 standard deviations in math and 0.023 in ELA. These estimates are remarkably similar to those we obtain using 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 that charter expansion has a negative and significant, but very small, effect in Michigan. However, this paper primarily focuses on elementary schools rather than middle schools, which may explain the discrepancy. Imberman (2011) also finds significant negative effects for elementary schools but an insignificant positive effect, of similar size to our own results, for middle and high schools.[43]

It is also worth noting that the negative results in the literature tend to occur in settings 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).

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

The somewhat limited effect of charter schools on student achievement might be slightly surprising given the increased per-pupil spending observed. Two mechanisms help reconcile these two effects. First, the evidence on the effects of school spending on academic outcomes is mixed.[44] Second, it often takes time before additional spending starts to have an effect on student achievement. Jackson et al. (2016) find that, for low-income children, a 10% increase in per-pupil spending each year for all 12 years of public school is associated with 0.46 additional years of completed education. Using a similar identification, Lafortune et al. (2018) find clear changes in achievement trends following the adoption of school finance reforms. These changes cumulate over subsequent years, so that "ten years after a reform, relative achievement of students in low-income districts has risen by roughly 0.1 standard deviation". Our identification only allows us to measure outcomes up to four years after the 2011 reform. However, consistent with the two aforementioned studies, we show evidence of larger long-term effects in the mechanism section.

8 Robustness Checks

Sensitivity Tests for Synthetic Control Specifications

Identifying 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 six robustness checks that test our main results' sensitivity to changes in each of these three choices (Ferman et al., 2017; Kaul et al., 2017). Results are presented in Appendix B. Table A.7 details the specification used for each robustness check. For comparison, the first row presents the baseline specification used throughout the paper.[45] 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 sys-

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

[45]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 scores, respectively.

tematically 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. Using cross-validation to construct predictor variable weights – i.e., splitting the pre-treatment sample and choosing weights to minimize out-of-sample fit – instead of our nested optimization procedure tends to produce larger RMSPEs, possibly because splitting the sample introduces more noise with our limited number of years. 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.

The second initiative adopted in 2011 was school turnarounds. 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 schools are designated level 4 or 5 based on an analysis of four-year trends in absolute achievement, student growth, and improvement trends.[46] 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. In addition, newly identified level 4 schools are eligible to apply for federal funding through the Massachusetts School Redesign Grants program.

The instrumental variables 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, school redesign grant recipients, 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 achieve-

[46]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.

ment differences 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% of a district's total spending, we would subtract 60% of the received grants from instruction expenditures.

Table 6 indicates 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 Exploring Mechanisms

9.1 Long-run Effects and the Role of Massachusetts' Temporary Refund Scheme

The treatment effect of expansion that we measure includes both any competitive effect of charter schools on traditional public schools and the direct effect of a short-term increase in traditional public school funding due to Massachusetts' refund scheme. Separating these effects is useful both from an external validity perspective (what would we expect in states without refunds?) and a policy perspective (should more states adopt these refunds?).

To provide some evidence on these mechanisms, we analyze whether the spillover effects of expansion persist after districts' temporary aid ends. The 2011 reform we use above was too recent to measure post-refund effects of expansion. Therefore, we instead exploit, in an event-study framework, the fact that many districts in our sample saw charter openings pre-2011 which led to large, persistent, and often sudden increases in the share of students attending charter schools. We select large openings similarly to how we chose expanding districts previously, as ones where the district saw faster growth following the opening than before. Specifically, we select openings where the total increase in charter share in the seven years after opening was at least 1 percentage point higher than the increase in charter share from the beginning of our sample period (2002) to the year of opening.[47]

We then use an event study to estimate the impact of these large openings on districts' current and future charter share and outcome variables. That is, we estimate the following

[47]Note that an opening at t denotes opening in the school year beginning at t, while the outcome variables are measured for the school year ending in t, so an opening at t should only affect the charter share from $t+1$ onwards.

equation by OLS:

$$Y_{dt} = \alpha + \sum_{k=-5}^{k=7} \theta_k O_{d,t-k} + \eta_d + \phi_t + \gamma_d t + \epsilon_{it}$$

where Y_{dt} is either the charter share or a financial or achievement outcome variable, and $O_{d,t}$ denotes that the district had a large opening at time t. We control for district and time fixed effects (η_d and ϕ_t) and district-specific linear time trends ($\gamma_d t$). θ_k is the estimate of the effect of a charter opening k years after the event. Note that this specification estimates the effect of charter openings up to seven years after the opening, by which point there would be no further reimbursement due to the initial increase.

The coefficients θ_k cannot necessarily be interpreted as causal effects because, as explained in previous sections, the location and timing of charter openings are likely to be nonrandom.[48] However, we have two reasons to believe the results from this event study might be a good guide to longer-run causal effects. Firstly, by including 5 leads of charter opening ($k = -1, \ldots, -5$), we are able to investigate pre-trends in outcome variables, and perform a Granger test for the direction of causality. Our results show that although the pre-opening coefficient estimates are noisy, there is no clear pre-trend for most outcomes. Secondly, our event study estimates turn out to replicate the synthetic control estimates surprisingly well over the three- to four-year timespan. These two factors give us some confidence that the event study provides useful information on the longer-run effect of charter expansion.

Figure 10 plots the estimated effect of an opening at t on outcomes at $t + k$ for $k = -5, \ldots, 7$. The effect at $t = 0$ is normalized to zero. The district charter share rises by about 1.5 percentage points in the year after opening, about 0.5 points in the year after, and subsequently remains largely flat. This is important, as the reimbursement formula depends on the *increase* in the number of charter students in past years. Hence, the average treated district should be receiving little refund money by $t + 7$.

Over the short term, expanding districts see a large and significant post-opening rise in total per-pupil spending and instructional spending, corresponding well to our IV-SC and IV-DiD results. Also in line with previous results, the effect on fixed costs is largely insignificant, and there appears to be a small positive effect on salaries. Slightly more surprisingly, spending on support services appears to increase, which contradicts our previous results. We tend, however, to trust the causal IV-SC and IV-DiD estimates more.

The long-term effects are directionally similar to the short-term effects, though always smaller. This suggests that the largely positive effects of charter expansion on districts tend to persist, but more modestly, or vanish past the end of the temporary refund.

The effects on achievement are modestly positive in the short run, again corresponding well to our IV-SC and IV-DiD results, and largest over the four- to six-year time horizon. This fits with previous research suggesting that it takes several years for increased spending to impact

[48] We prefer not to do a synthetic control for these charter openings, as there is no clear way to both adjust for the fact that different openings happen at different times and simultaneously aggregate outcomes across districts to reduce noise.

achievement (Jackson et al., 2016; Lafortune et al., 2018).

This distinction between the short-term and long-term effects also partially reconciles our results with the negative fiscal spillover effect identified by previous studies, which use data from states where districts are not refunded for their charter tuition expenditures (Arsen and Ni, 2012; Ladd and Singleton, 2018; Cook, 2018). This event study provides suggestive evidence that our main results are driven largely by the temporary injection of extra funding into the school system, rather than any competitive effect. The small long-run effect is also consistent with the idea that the refund effectively "cushions the blow" to districts from a sudden loss of students due to charter expansion.

9.2 Impact of Charter Expansion on the Pupil-Teacher Ratio

We find that the districts facing the largest charter expansion tend to increase their spending on instruction. This might be due to a decreased pupil-teacher ratio in traditional public schools. Indeed, when facing charter school competition, over the short and medium terms, traditional public schools might be losing students without dismissing teachers. We confirm this by comparing the evolution of the teacher-student ratio in expanding and nonexpanding districts. To do so, we apply the same IV-SC methodology as we have used for the main analysis.

Figure 12 shows our results. The districtwide ratio of students to teachers falls in expanding districts relative to the synthetic control post-reform. Hence class size is also likely to fall. This reduction in class size could be one mechanism for explaining the positive achievement effects we observe, including over the longer run. Numerous studies have demonstrated the positive effect of smaller class sizes on student achievement (Krueger, 1999; Angrist and Lavy, 1999; Hoxby, 2000; Urquiola, 2006; Fredriksson et al., 2013) [49] and on longer-term outcomes, such as the probability of taking the ACT and SAT exams or being enrolled in college (Krueger and Whitmore, 2001; Chetty et al., 2011). However, according to our placebo inference the effect on the pupil-teacher ratio is not statistically significant.[50]

10 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.

[49] Hoxby (2000) finds no impact on U.S. data.

[50] Teacher transition from traditional public schools to charter schools is another potential mechanism through which charter school expansion might affect non-charter school students achievement. However, some evidence suggests that this mechanism is unlikely. Cohodes et al. (2016) examine the composition of Boston charter schools before and after the 2011 expansion. They show that in Boston 66 percent of the expansion charter teachers have less than one year of experience teaching in Massachusetts public schools and 25 percent of the expansion charter teachers came from the proven provider parent campus. This confirms that teacher transitions from traditional public schools to charter schools are very rare, in part because salaries are higher in traditional public schools than in charters.

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 increases per-pupil expenditures in traditional public schools. In addition, these schools react to competition by shifting their resources from support services to instruction. The IV-SC method shows that, after the reform, total per-pupil expenditures, per-pupil expenditures on instruction, and salaries increased by 5.2%, 7.2%, and 5%, respectively, in expanding districts compared to nonexpanding districts. This is accompanied by a 4% reduction in per-pupil expenditures on support services. Further, our results indicate that charter expansion positively impacts 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 limited impact on student achievement (Bettinger, 2005; Imberman, 2011).

It is worth noting one additional caveat 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. Similarly, our estimates hold for districts where the share of students that attend a charter school raises from 5 to 12%. If the fiscal and education spillover effects are nonlinear, we would not recommend using our estimates to predict effects of charter expansions in vastly different ranges.

Finally, our analysis focuses on the spillover effects of charter schools on traditional public schools. From a policy perspective, it is important to consider the effect of charter expansion on the entire school system. One might worry that if a temporary aid scheme for traditional school districts is necessary, charter expansion will be very costly to the state. However, this cost is partially compensated for by the reduced funding received by charter schools. Wolf et al. (2017) report that in Boston, charter schools' per-pupil revenue is about 17% lower than in traditional public schools.

Equally policy-relevant is the overall effect of charter expansion on student achievement. We find charter expansion has a small positive effect on the achievement of students who stay in traditional public schools. In addition, several papers show that urban charter schools in Massachusetts significantly boost their students' test scores, while nonurban charter schools seem to reduce student achievement (Abdulkadiroğlu et al., 2011; Angrist et al., 2013; Abdulkadiroğlu et al., 2016). Taken together, these analyses show that a student's transition from a traditional public school to a urban charter school can potentially have a large positive effect not only on her achievement but also on the achievement of students who stay in traditional public schools. The overall effect is more uncertain when students transition to nonurban charter schools.

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Figure 1: Charter Sector Expansion after the 2011 Reform

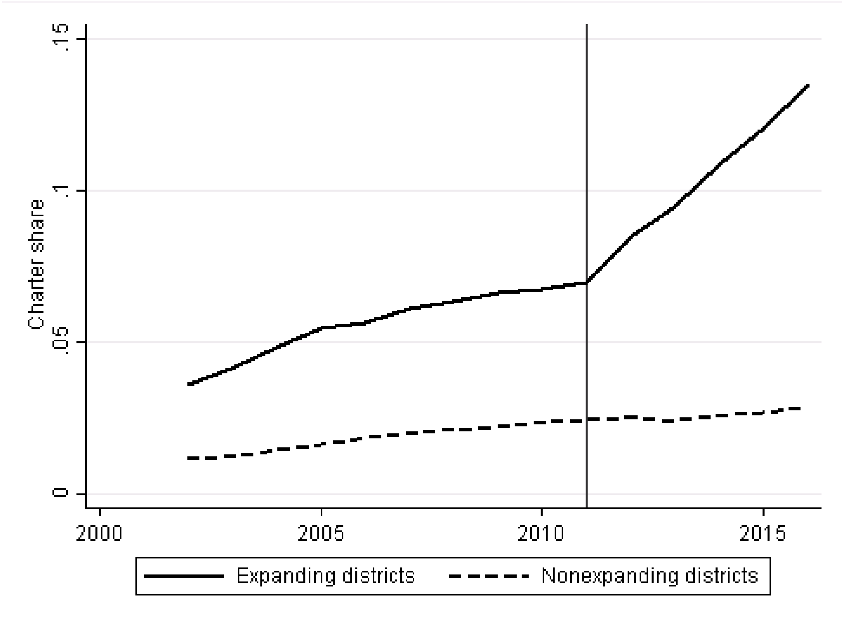
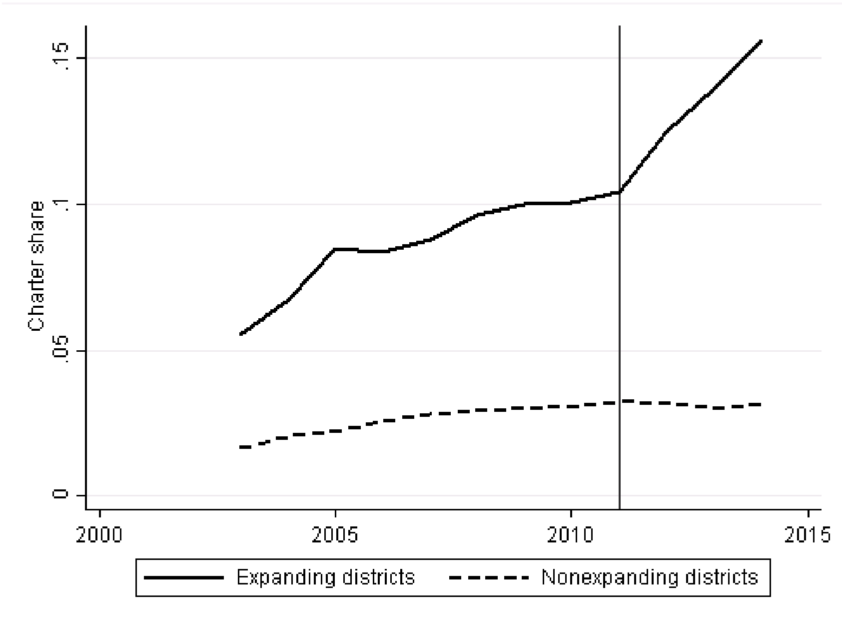


Figure 2: Charter Sector Expansion after the 2011 Reform - Middle Schools



Notes: These figures plot the share of students attending a charter school over time. Figure 1 plots the share for elementary, secondary, and high school students, while Figure 2 is limited to middle school students. The plain lines represent districts that saw an increase in the share of students attending a charter school after the 2011 reform (expanding districts), and the dotted lines represent the 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.

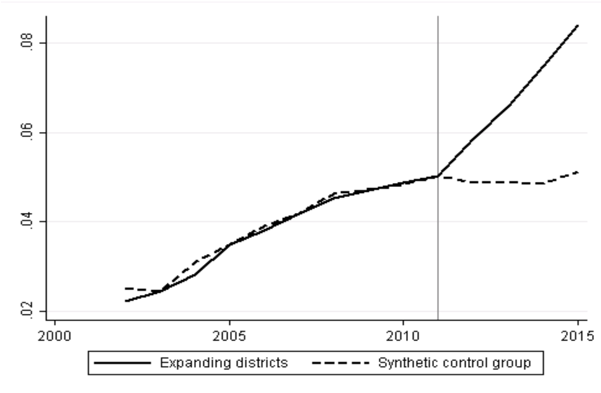
Table 2: Synthetic Control Districts' Weights

	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					

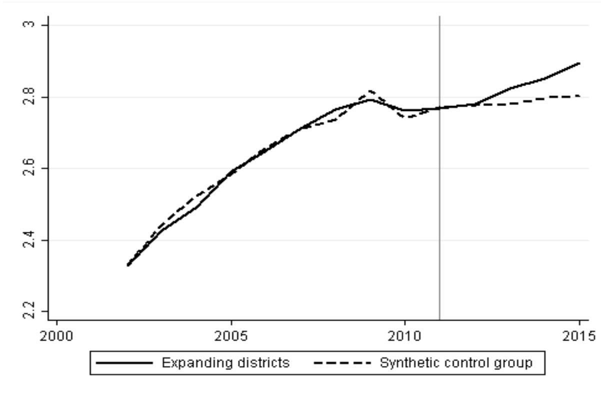
† Notes: This table reports the district weights assigned by the synthetic control method. Columns 1 to 7 report weights computed when the outcome variable is, respectively, districts' per-pupil expenditures (column 1); districts' per-pupil expenditures on fixed costs (column 2), instruction (column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

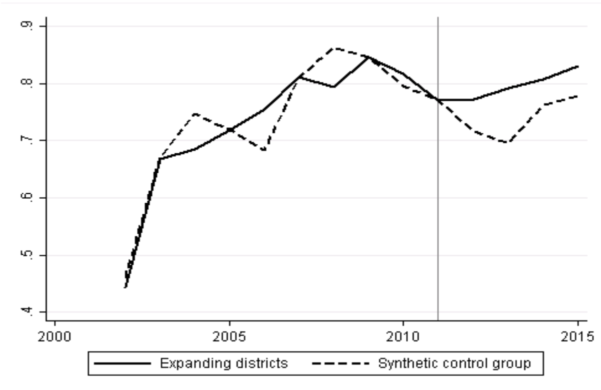
(a) Charter share



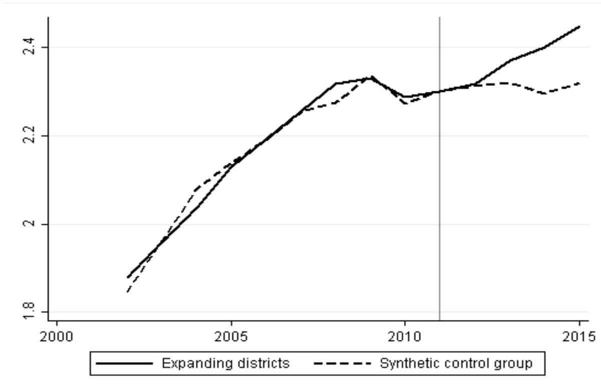
(b) Total spending



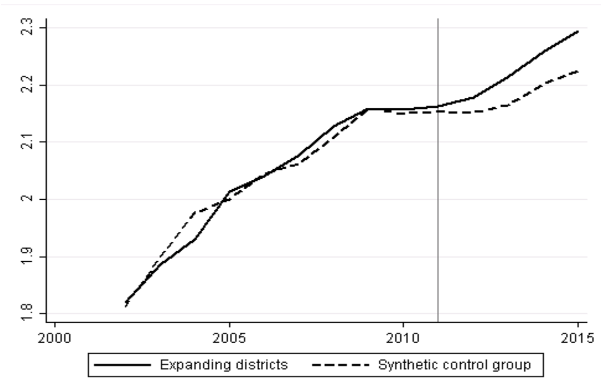
(c) Spending on fixed costs



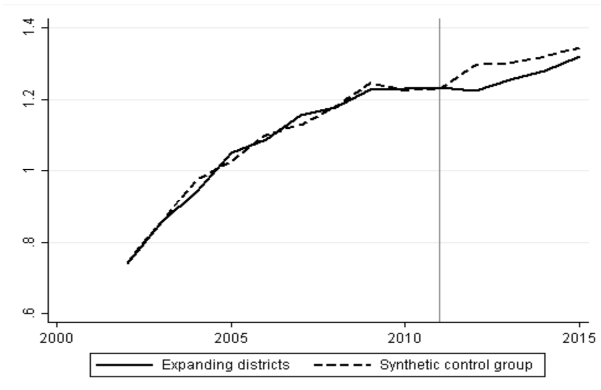
(d) Spending on instruction



(e) Spending on salaries



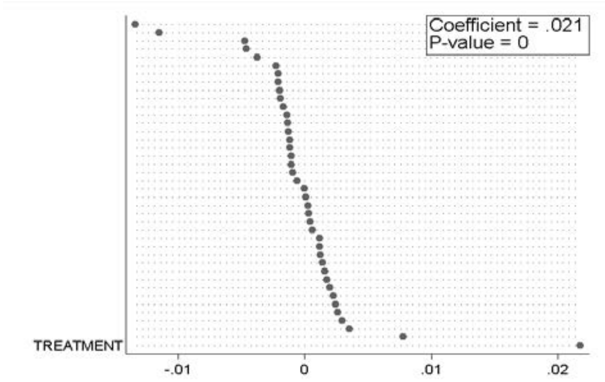
(f) Spending on support services



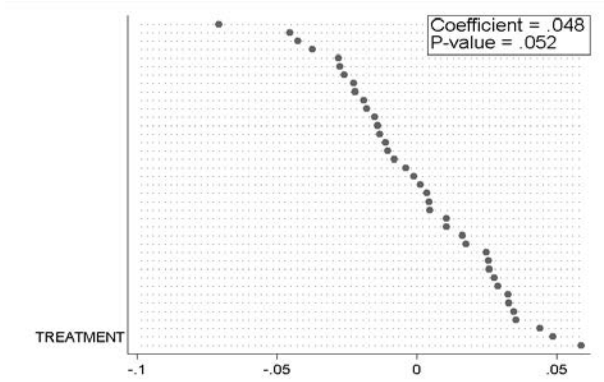
Notes: This figure plots the share of students attending a charter school (plot a); districts' per-pupil expenditures (plot b); and their per-pupil expenditures on fixed costs (plot c), instruction (plot d), salaries (plot e), and support services (plot f). For all expenditure variables, we use the log of the variable. The plain lines represent districts that saw an increase in the share of students attending a charter school after the 2011 reform (expanding districts), and the dotted lines represent the synthetic control districts. For expanding districts, we plot the average charter share and expenditures. For synthetic control districts, we plot the weighted average of the charter share and expenditures. We use the weights defined by the synthetic control method.

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

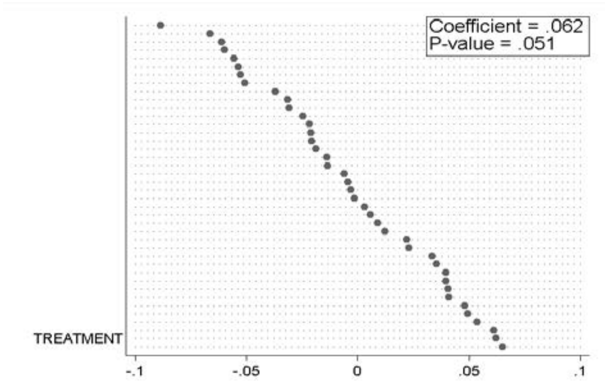
(a) Charter share



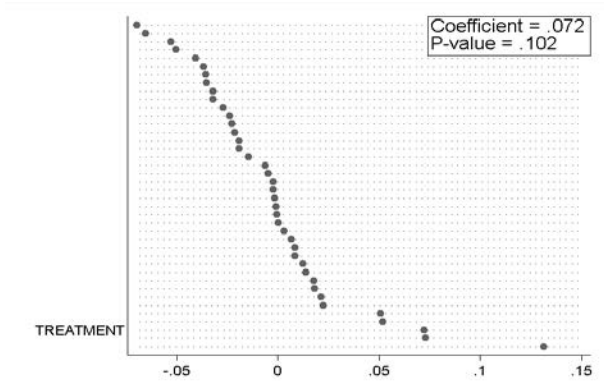
(b) Total spending



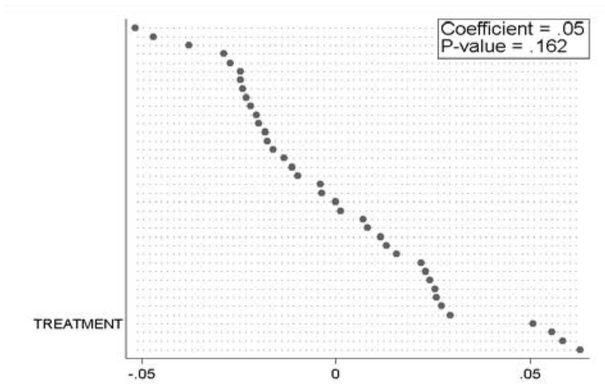
(c) Spending on fixed costs



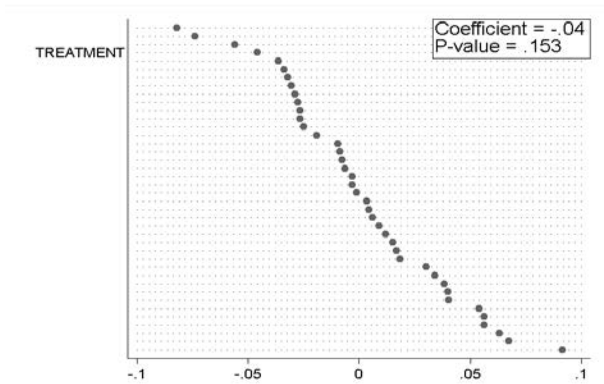
(d) Spending on instruction



(e) Spending on salaries



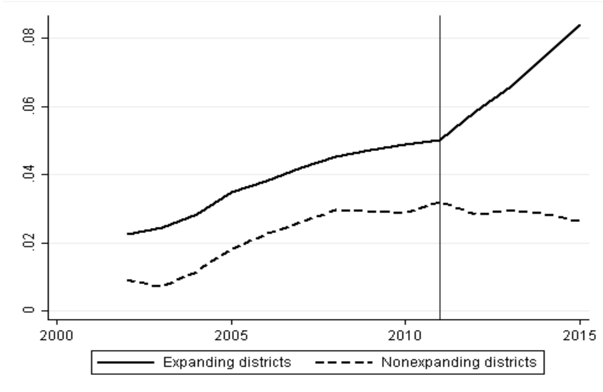
(f) Spending on support services



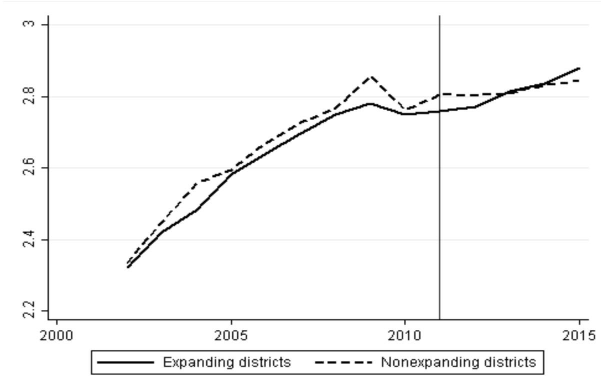
Notes: This figure plots the distribution of the charter expansion's effect on districts' per-pupil expenditures, as measured by the synthetic control method. The lines "TREATMENT" report the coefficients when expanding districts are compared to their synthetic control districts. The exact value of each 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 group of non-expanding districts is compared to its identified group of synthetic control districts. The p-value is calculated as the probability of obtaining a placebo estimate that is greater than the actual estimated treatment effect (less than it when the effect is negative), multiplied by two to approximate a two-tailed test.

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

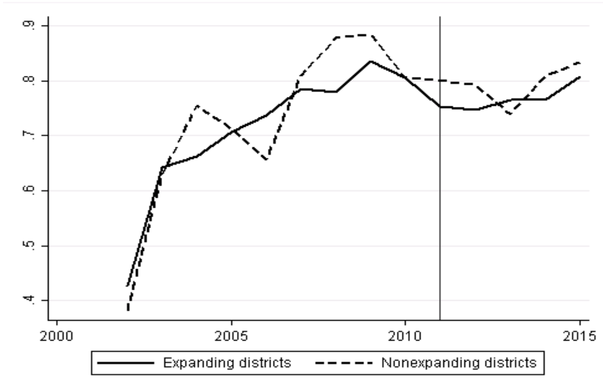
(a) Charter share



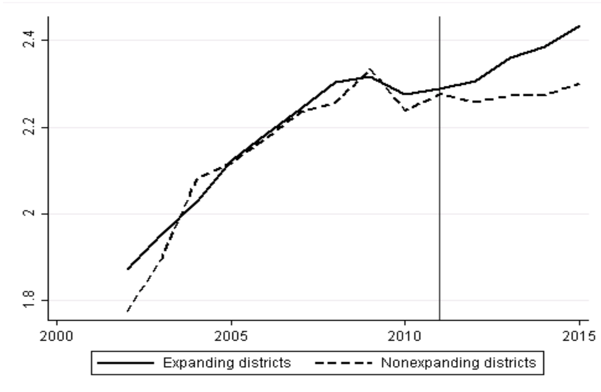
(b) Total spending



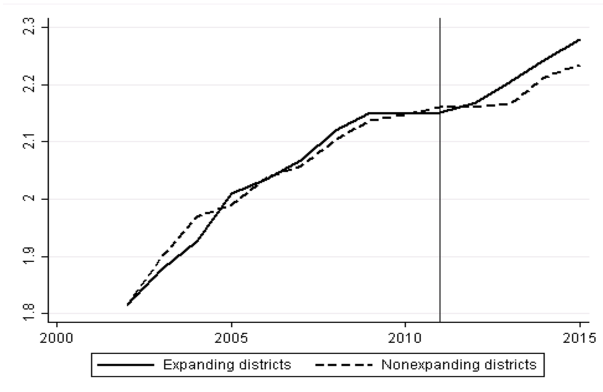
(c) Spending on fixed costs



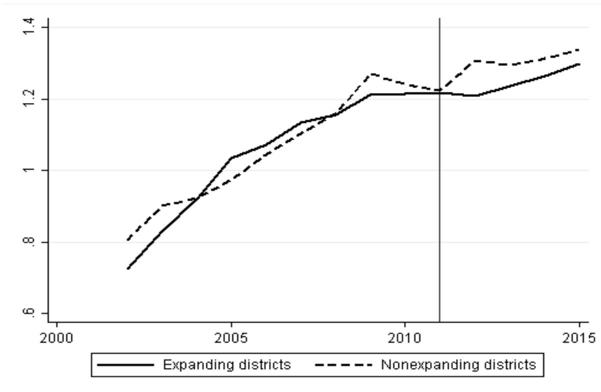
(d) Spending on instruction



(e) Spending on salaries



(f) Spending on support services



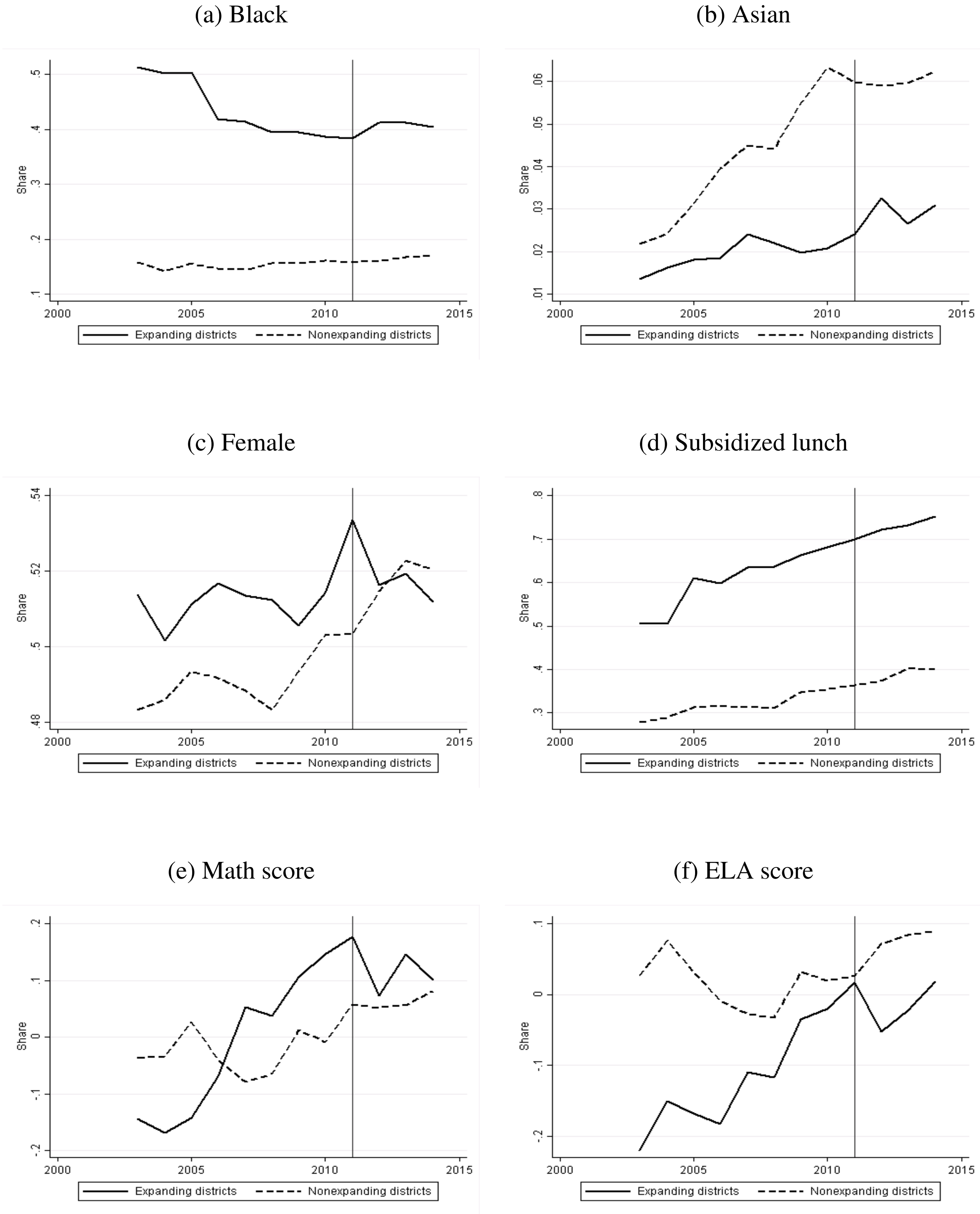
Notes: This figure plots the share of students attending a charter school (plot a), districts' per-pupil expenditures (plot b), their per-pupil expenditures on fixed costs (plot c), instruction (plot d), salaries (plot e), and support services (plot f). For all expenditure variables, we use the log of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



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. Each endogenous variable is instrumented by the three instruments. However, for readability, we only report the coefficient of the relevant instrument in the first three columns, that is (1) the coefficient on the lottery offer dummy for the charter school attendance variable, (2) the coefficient on the urban charter lottery offer for the interaction between charter attendance and urban schools, and (3) the coefficient on the post-reform lottery offer for the interaction between charter attendance and post-reform years. 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.

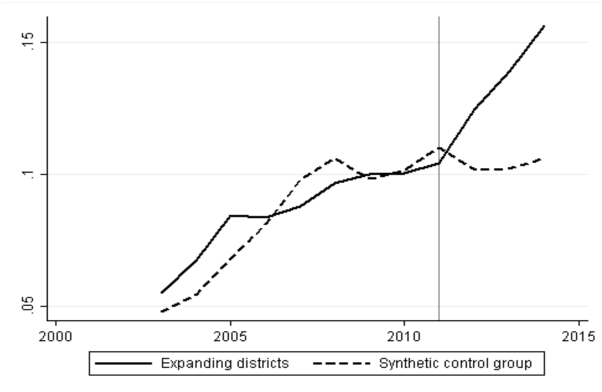
*** Significant at the 1 percent level.

** Significant at the 5 percent level.

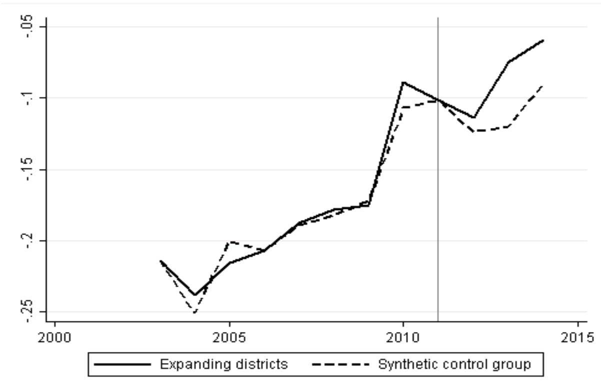
* Significant at the 10 percent level.

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

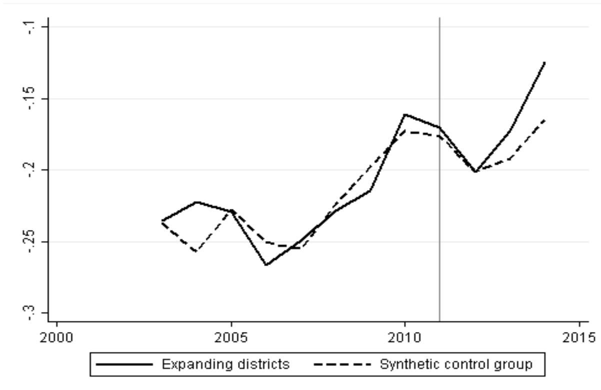
(a) Charter share



(b) Math score



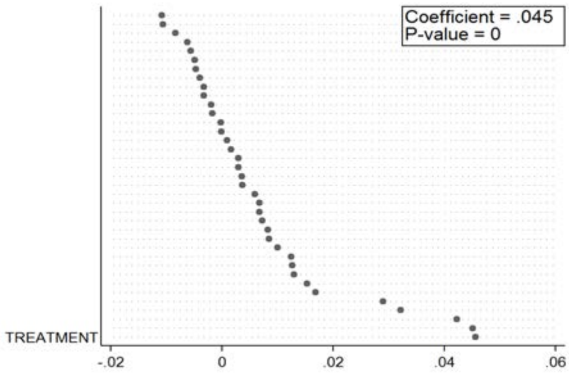
(c) ELA score



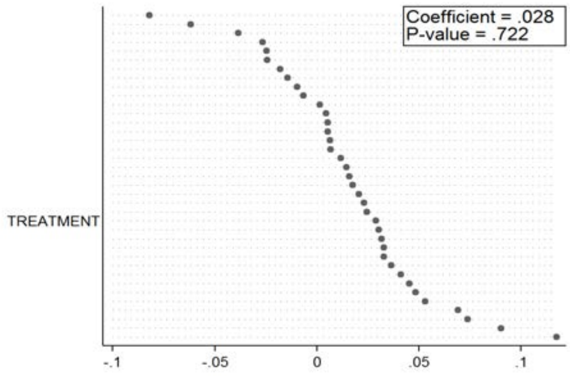
Notes: This figure plots the share of students attending a charter school (plot a) and students' average math and ELA test scores (plots b and c). The plain lines represent districts that saw an increased share of students attending a charter school after the 2011 reform (expanding districts), and the dotted lines represent the synthetic control districts. 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

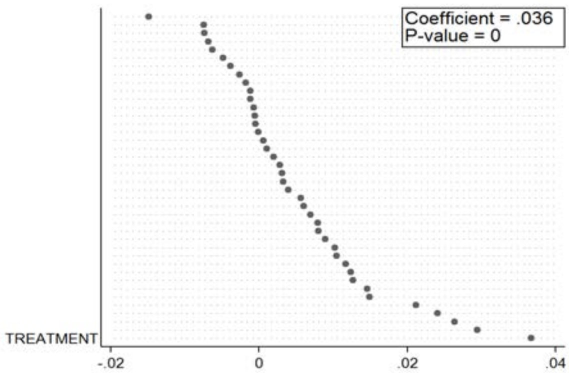
(a) Charter share (math weight)



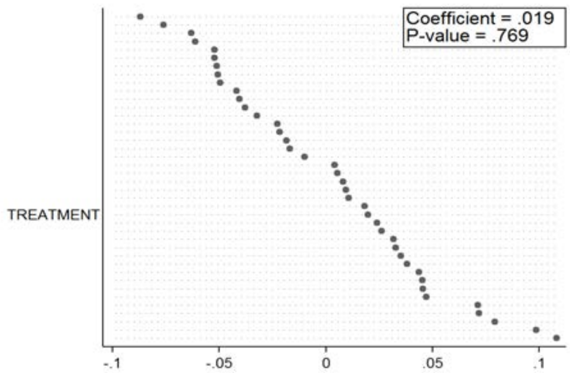
(b) Math score



(c) Charter share (ELA weight)



(d) ELA score

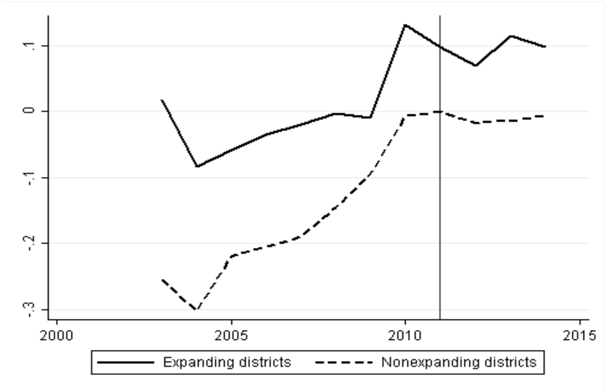


Notes: This figure plots the distribution of charter expansion's impact on student achievement, as measured by the synthetic control method. The lines "TREATMENT" report the coefficients when expanding districts are compared to their synthetic control districts. The exact value of each coefficient is reported in the top right corner of each figure. The other lines in the figures report the coefficients when a placebo group of non-expanding districts is compared to its identified group of synthetic control districts. The p-value is calculated as the probability of obtaining a placebo estimate greater than the actual estimated treatment effect (less than it when the effect is negative), multiplied by two to approximate a two-tailed test.

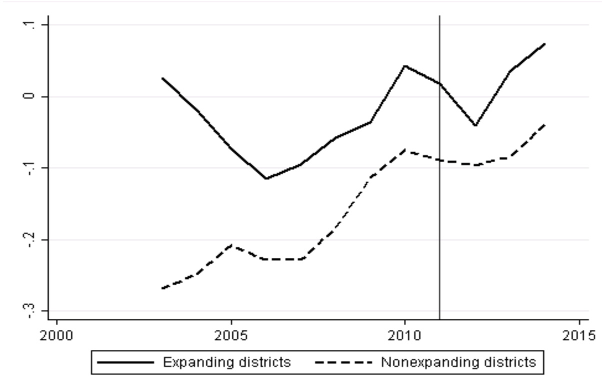
Figure 9: Pre-trends in Student Achievement

Control Group A: Synthetic control districts

(a) Math score

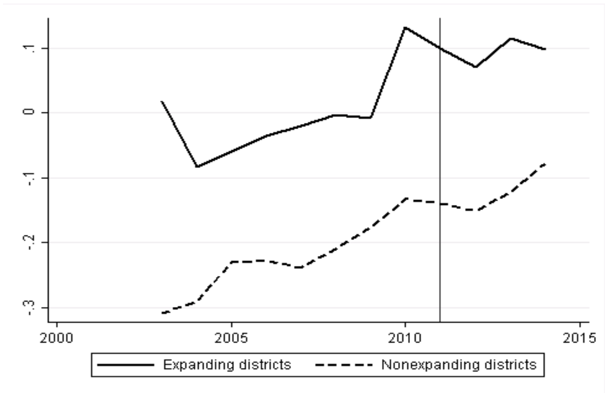


(b) ELA score

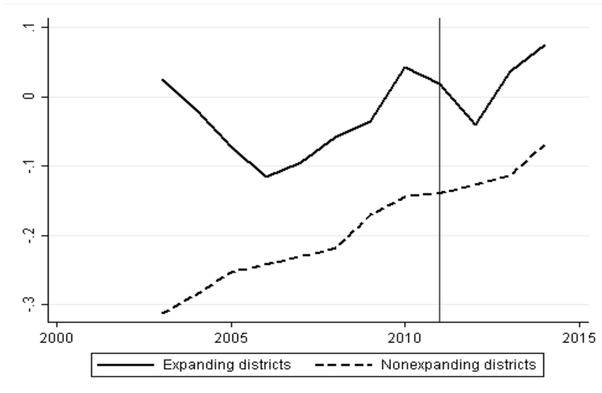


Control Group B: Bottom 10th percentile districts

(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 below the 10th 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.

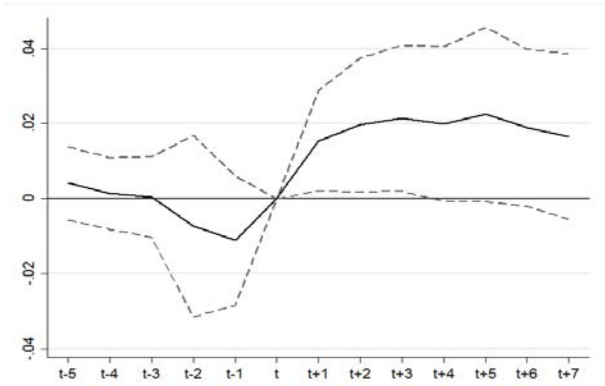
Table 6: 2SLS Estimates of Fiscal Spillovers
Robustness Check Excluding Innovation and School Redesign Grants

	Per-pupil expenditures on:				
	Total (1)	Instruction (2)	Fixed costs (3)	Support services (4)	Salaries (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

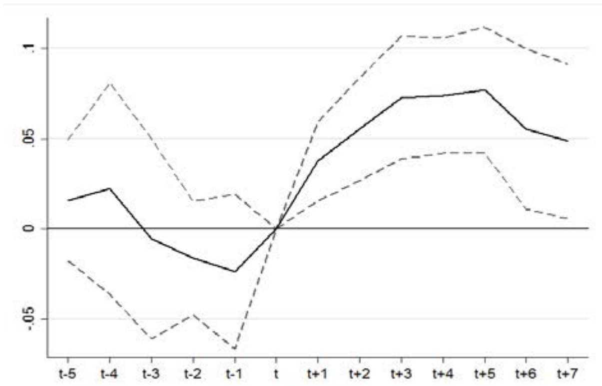
† Notes: This table reports 2SLS estimates of the charter expansion's effect on districts' per-pupil expenditures. For all expenditure variables, we use the log of the variable, and we subtract the innovation and school redesign grants received. The endogenous variable is the charter share, which is 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.

Figure 10: Event-Study Estimates of the Effect of Charter Opening

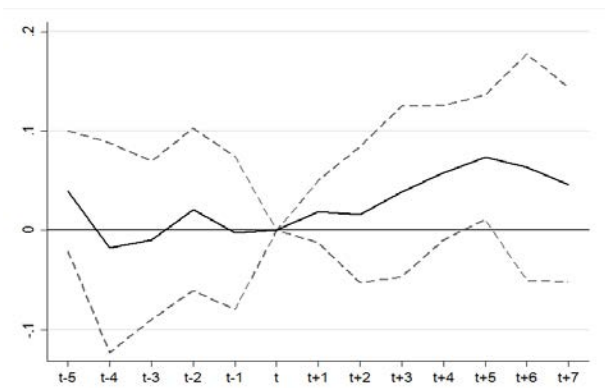
(a) Charter share



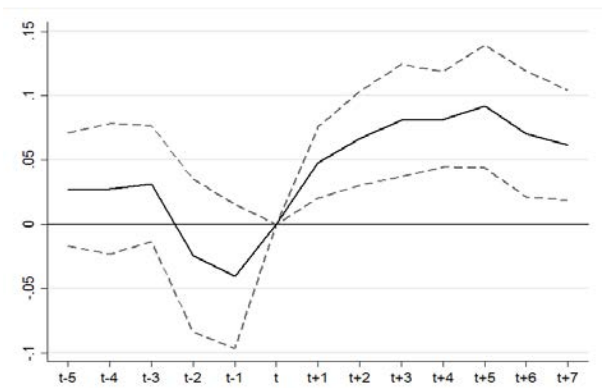
(b) Total spending



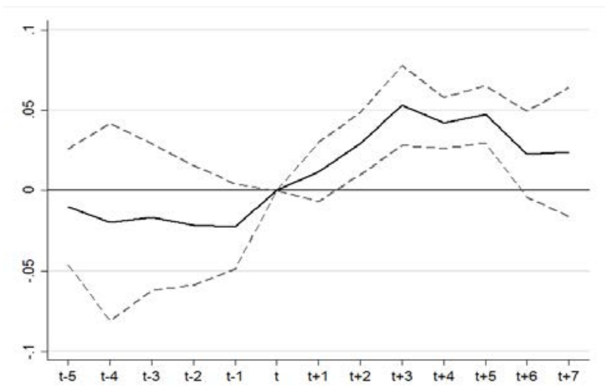
(c) Spending on fixed costs



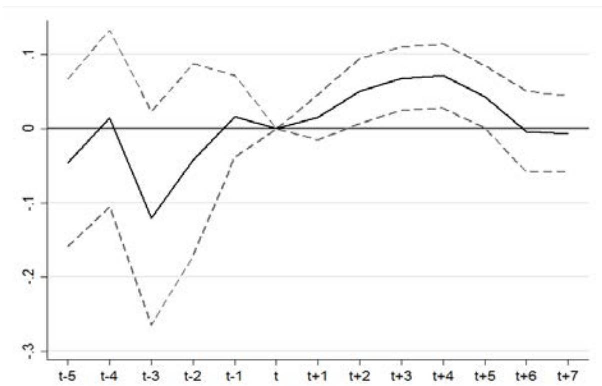
(d) Spending on instruction



(e) Spending on salaries



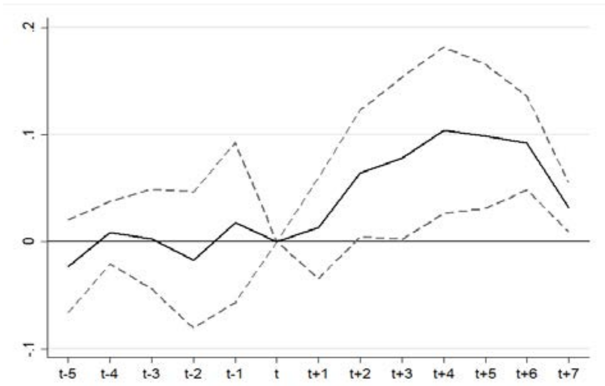
(f) Spending on support services



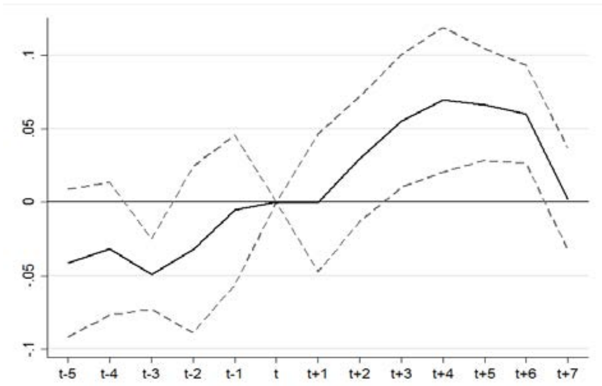
Notes: This figure plots the estimated coefficients on lags and leads of charter openings in our event study regression. The dependent variable is either the charter share (plot a) or districts' per-pupil expenditures (plot b to f). All regressions control for district and time fixed effects, as well as district-specific linear time trends. $t + k$ in the graph above corresponds to the coefficient on the k^{th} lag of a dummy for a charter opening. The solid line denotes estimates and the dashed lines 95% confidence intervals calculated from standard errors clustered at the district level. All regressions include district and time fixed effects and district-specific linear trends. District expenditure variables are in logs.

Figure 11: Event-Study Estimates of the Effect of Charter Openings

(a) Math



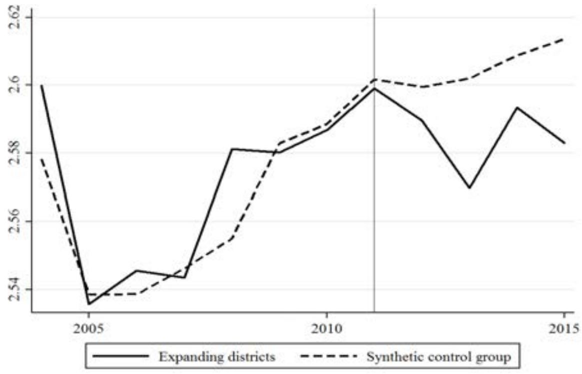
(b) ELA



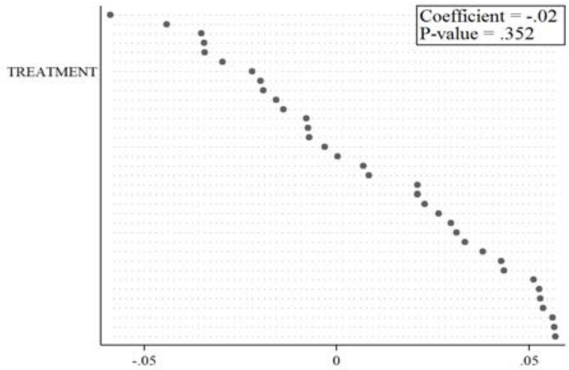
Notes: This figure plots the estimated coefficients on lags and leads of charter openings in our event study regression. The dependent variable is districts' average achievement in math (plot a) and ELA (plot b). All regressions control for district and time fixed effects, as well as district-specific linear time trends. $t + k$ in the graph above corresponds to the coefficient on the k^{th} lag of a dummy for a charter opening. The solid line denotes estimates and the dashed lines 95% confidence intervals (calculated from standard errors clustered at the district level). All regressions included district and time fixed effects and district-specific linear trends. Achievement is measured as MCAS scores standardized to have mean zero and variance 1 in each year.

Figure 12: Pupil-Teacher Ratio in Expanding Districts and Synthetic Control Districts

(a) Pupil-Teacher Ratio



(b) Placebo Inference



Notes: Figure (a) plots the log of the districtwide pupil-teacher ratio in the expanding districts and the synthetic control we construct for this variable. Figure (b) shows our placebo inference for figure (a). "TREATMENT" reports the average treatment effect measured in figure (a). The exact value of the coefficient is reported in the top right corner. The other lines in figure (b) report the coefficients when a randomly chosen placebo group of non-expanding districts is compared to its identified group of synthetic control districts. The p-value is calculated as the probability of obtaining a placebo estimate greater than the actual estimated treatment effect (less than it when the effect is negative), multiplied by two to approximate a two-tailed test.