How to Make Additional Time Matter:
Integrating Individualized Tutorials into an Extended Day

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Abstract

Support for extending the school day has gained substantial momentum despite limited causal evidence that it increases student achievement. Existing evidence is decidedly mixed, in part, because of the stark differences in how schools use additional time. In this paper, I focus on the effect of additional time in school when that time is used for individualized tutorials. In 2005, MATCH Charter Public High School integrated two hours of individualized tutorials throughout an extended school day. The unanticipated implementation of this initiative and the school's lottery enrollment policy allow me to use two complementary quasi-experimental methods to estimate program effects. I find that providing students with two hours of daily tutorials that are integrated into the school day and taught by full-time, recent college graduates increased achievement on 10^{th} grade English language arts exams by 0.15- 0.25 standard deviations per year. I find no average effect in mathematics beyond the large gains students were already achieving, although quantile regression estimates suggest that the tutorials raised the lowest end of the achievement distribution in mathematics.

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1. Introduction

In President Obama's first major address on education, he argued “the challenges of a new century demand more time in the classroom." The President’s statement is reflective of a growing movement among educational reformers and political advocates who view lengthening the amount of time students attend school as necessary for closing achievement gaps and keeping American students competitive with their international peers. In recent years, several states and a growing number of districts and charter schools have adopted extended learning time (ELT) initiatives to lengthen the school day. Despite the billions of dollars that have been allocated to these initiatives and the more than 1,000 schools that operate with an extended day, there exists little credible evidence of the causal effect of ELT on student academic achievement. Evidence from observational studies is decidedly mixed. Moreover, past research has focused on the potential benefit of additional time writ large, rather than on specific ways in which this time might be used. The mixed evidence on ELT and stark differences in how schools use additional time suggests that it is not the additional time itself, but how that time is used, that matters.

In this paper, I focus narrowly on estimating the effect of extended learning time on student achievement when additional time is used exclusively for individualized tutorials. In the beginning of the 2004/05 academic year, MATCH Charter Public High School in Boston, Massachusetts extended its school day from 3:00 pm to 5:00 pm four days a week in order to integrate two hours of individualized tutoring classes throughout the school day. MATCH was able to do this by establishing a Tutor Corps fellowship program that attracted and prepared high-achieving, recent college graduates to work as full-time tutors. Importantly, students who were in the sophomore cohorts at MATCH just before, and just after, the school added two hours of individualized tutorials could not have anticipated this change due to the compressed timeline in which the initiative was conceived, publically announced, and adopted.

I capitalize on this natural experiment, as well as the lottery admissions process at MATCH, by employing two quasi-experimental methods to estimate the effect of extra time for tutorials on student achievement. In my first approach, I compare the change in average student achievement at the school, before and after the plausibly exogenous implementation of the extended day, with the average change during this same time period among Boston-area charter high schools. I complement this difference-in-differences research design with an instrumental variables (IV) approach in which I take advantage of the randomized offer of enrollment to MATCH as a second source of exogenous variation. This IV estimation strategy provides a strong test of many potential threats to the validity of the first approach.

I find that providing students with two hours of daily tutorials that are integrated into the school day and taught by full-time, recent college graduates increased student achievement in English language arts by between 0.15 and 0.25 standard deviations per year. These effects are large in magnitude compared to a wide range of educational interventions targeting student achievement in English language arts, particularly at the high school level. On average, ELT tutorials had no additional effect on the already large gains MATCH students were making on the 10th grade mathematics exam prior to the implementation of the program. However, mathematics scores among the lowest-achieving students improved substantially after the introduction of ELT tutorials.

In the next section, I present an overview of the movement for increased time in U.S. schools, review the evidence of how additional time affects student achievement, and summarize the research on tutoring programs. In sections 3 and 4, I describe MATCH and its tutorial model, and I discuss my data sources. Section 5 focuses on estimating the effect of ELT tutorials using a classical difference-in-difference methodology. I supplement these analyses with an Instrumental Variables analytic approach in section 6. I explore potential explanations for the divergent results across subjects in section 7 and conclude in section 8.

2. Background and Context

2.1 Time in School

Over a quarter century ago, the landmark report, *A Nation at Risk*, warned that the academic achievement of American students was lagging behind that of students from many other industrialized nations. The report attributed this competitive decline, in part, to the comparatively fewer hours American students spent on schoolwork and to the ineffective use of time in American schools. A decade later, the National Education Commission on Time and Learning famously characterized American schools as *Prisoners of Time,* captives of an agrarian-based school year comprised of 180 six-hour days.

Recently, advocates of extended learning such The National Center on Time and Learning (NCTL) have helped place legislation before state and federal lawmakers to secure funding to extend the public school day and year. In 2005, Massachusetts lawmakers passed the Massachusetts Extended Learning Time Initiative, which after five years had grown to support 26 schools with an annual appropriation of $15.6 million. Similar proposals have been considered by state legislatures in Minnesota, Delaware, and Ohio, but currently remain unfunded (Patall, Cooper & Allen, 2010). In 2008, Senator Edward Kennedy (D-MA), introduced the Time for Innovation Matters in Education Act, or TIME Act, the first federal legislation intended specifically to bring extended school days to scale across the country.

Meta-analyses of the literature reveal that policymakers have limited evidence to inform their decisions about whether to spend scarce taxpayer dollars to increase learning time (Karweit, 1985; Patall, Cooper & Allen, 2010). Most research on the relationship between time in school and student achievement has come from descriptive case studies and covariate-controlled comparisons of schools with extended days or years with their traditional counterparts. However, as Marcotte and Hansen (2010) point out, schools that have the resources and local support for extending their school day or school year are likely to differ in systematic and unmeasured ways from those that do not. In order to account for the effect of such unobserved differences, researchers have exploited arguably exogenous variation in test-administration dates and school closings caused by inclement weather (Hansen, 2008; Marcotte & Helmet, 2008; Sims, 2008; Fitzpatrick, Grismmer & Hastedt, 2011). Collectively, these studies provide strong evidence of the gains in achievement on standardized tests that result from additional days of instruction.

Researchers have been far less successful at identifying the causal effect of extending the school day on student academic achievement or at evaluating the comparative effectiveness of specific uses of ELT. Evaluations of large-scale ELT initiatives in Massachusetts and Miami-Dade, Florida found that, after three years, students in traditional public schools with extended days performed no better, on average, than comparison group schools (Urdegar, 2009; Abt Associates Inc., 2012). However, these averages obscure the large degree of heterogeneity in improvement trends across individual schools.

Most favorable evidence of the impact of extended days on student achievement comes from recent research on urban charter schools that serve predominantly low-income students

(Hoxby, Muraka, & Kang, 2009; Angrist, et al., 2010; Abdulkadiroglu et al., 2011; Dobbie & Fryer, 2011a). Capitalizing on randomized lottery admissions processes, these studies found that many of the oversubscribed urban charter schools where students make large academic gains also have longer school days. However, as Hoxby, Muraka, and Kang (2009) point out, the design of these studies did not permit researchers to estimate the role that extended learning time played in the success of these schools (p.viii). Building on this work, Dobbie & Fryer (2011b) examined the association between school practices and charter schools' effects on student achievement and identified instructional time among a set of five practices that explain almost half of the variation in these effects.

2.2 Tutors & Tutoring Programs

A more common approach to increasing instructional time is for schools to offer after-school tutoring programs for students. Although after-school tutoring programs are available in almost half of all U.S. public elementary schools (Parsad & Lewis, 2009), there exists little rigorous evidence of their effect on student achievement (Wasik, 1998; Scott-Little, Hamann, & Jurs, 2002). Meta-analyses of the literature on programs that incorporated some form of academic enrichment found average effect sizes ranging from 0.05 to 0.20 standard deviations. (Scott-Little et al., 2002; Lauer et al., 2006).Two research reviews that focused on tutoring programs found larger average effect sizes of 0.40 standard deviations (Cohen, Kulik, & Kulik, 1982; Elbaum, Vaughn, Hughes, & Moody, 2000). However, all of these reviews relied heavily on evidence from studies that used either pretest-posttest or non-equivalent comparison group research designs which cannot support causal inferences.

In 2001, the No Child Left Behind Act expanded federal support for after-school enrichment programs, homework centers and tutoring programs for low-income students and students attending low-performing schools through funding for Supplemental Education Services (SES) and 21st Century Community Learning Centers (CLC). Along with this funding came the requirement of more rigorous evaluation. Experimental studies revealed that SES and 21st Century CLC programs were not consistently aligned with school curriculum, were characterized by low rates of participation among eligible students and rarely resulted in measurable improvements in student achievement (James-Burdumy, et al., 2005; Zimmer, et al., 2007; Ross, et al., 2008). These studies also found "wide variability in activities and services delivered across programs" (James-Burdumy, et al., 2005, p. xxii). For example, researchers found that students who attended "academically enhanced" afterschool programs that used materials developed by Harcourt School Publishers and Success for All made the equivalent of an additional month worth of gains in mathematics achievement (Black et al., 2009).

Policymakers and scholars debate about whether this variability in effectiveness stems from differences in the training and qualifications of academic tutors. This question first gained national attention when President Clinton proposed to place one million volunteer reading tutors in schools through the America Reads Challenge Act (Manzo & Sack, 1997; Wasik, 1998; Edmondson, 1998). In practice, volunteers were largely recruited through the support of college work-study programs and AmeriCorps. A government funded pretest-posttest evaluation found that AmeriCorps tutors promoted student gains of 0.25-0.33 standard deviations above national benchmarks on the Woodcock-Johnson Reading Test (Abt Associates Inc., 2001). The most compelling evidence of the effectiveness of volunteer tutors comes from a meta-analysis of 21 experimental studies that found significant positive effects on students' reading achievement, but no effects in mathematics (Ritter, Barnett, Denny, & Albin, 2009).

3. Individualized Tutoring at MATCH

A review of the literature demonstrates the importance of how additional time in school is used and how tutoring programs are designed. Thus, I focus narrowly on one school's use of extended learning time for individualized tutoring. MATCH Charter Public High School is a "no-excuses" charter school with high academic expectations, a strict behavioral code, and a college-preparatory curriculum. The school admitted its first freshman class in the fall of 2000 and has since grown to serve 220 students in grades 9-12. Students at MATCH come from predominantly low-income, minority households and are admitted through an open-application lottery admissions process. MATCH has been recognized for its strong record of student achievement by the U.S. Department of Education and the Center on Education Reform.

Starting in the 2004/05 academic year, MATCH extended the school day by two hours, Monday through Thursday, in order to integrate two periods of mandatory tutorial sessions throughout the school day. This amounts to over 250 hours of individual or small-group (2-4 students) tutorials over the course of the academic year. Tutorial sessions at MATCH focus on skill development rather than homework completion. Typically, tutorial sessions begin with a silent five-minute warm-up task. Students then work with tutors to complete exercises and activities that reinforce and extend the core academic curriculum. Freshmen and sophomores receive an hour each of tutorials in mathematics and English. Juniors receive tutoring focused on supplementing their learning in AP U.S. History, a required course, and preparing them for the SAT. Tutorials for seniors consist of supporting them in an AP class of their choice (often Calculus or Biology).

Individualized tutorial classes at MATCH were made possible by the creation of the MATCH Tutor Corps fellowship designed to attract recent college graduates to work at the school as full-time tutors. In its inaugural year, MATCH recruited over 300 applicants for 40 Tutor Corps positions. Corps members attend a three-week training institute during the summer and are then paired with six to seven students with whom they work throughout the academic year. Instructional materials for the tutorials are developed by Corps members in collaboration with the MATCH teaching staff. In exchange for their commitment of 11 months of service, Corps members receive a $600 monthly stipend raised from a combination of AmeriCorps funding, grants and philanthropic donations. Tutors also live rent free on the third floor of the school, which was converted into a dormitory.

4. Data Sources

4.1 State Administrative Records

Starting in the 2001/02 academic year, the Massachusetts Department of Elementary and Secondary Education has maintained two statewide databases that record student enrollment and demographic data (Student Information Management System [SIMS]) and test results (Massachusetts Comprehensive Assessment System [MCAS]). Detailed SIMS attendance records allow me to track students' enrollment status across schools both within and across academic years. These enrollment data are particularly important given the high mobility rates among high school students in urban districts. The MCAS test records contain scores on state achievement tests as well as specific item responses. I combine data from these databases to construct a student-level dataset for all sophomores who were enrolled in a traditional public high school or charter high school in Massachusetts, from spring 2002 to 2009.

The 10^{th} grade English language arts (ELA) and mathematics scaled scores maintained in the MCAS data provide the key outcomes of interest for my analyses. Tests in both subjects are comprised of multiple-choice (assigned 1 point if correct, 0 otherwise) and open-response items (scored on a 0-4 point scale). In addition, the mathematics exam also contains short-answer items (scored on a 0-1 point scale). Content domains covered by the mathematics exam include number sense and operations; patterns, relations and algebra; geometry; measurement; and data analysis, statistics and probability. The ELA exam consists of language, literature, and composition content domains. Since the 2002/03 academic year, Massachusetts has required all high school students to earn a score of "proficient" or higher on both exams in order to earn a high school diploma (going forward, I refer to cohorts by the spring year in which they took the MCAS).

The lack of reliable test records before 2002 limits the range of prior test scores available for my analysis. In 2002, 8th grade was the highest grade in which students took a mathematics exam before 10th grade. Thus, sophomores in 2004 are the first cohort for which prior exam scores in mathematics are available. Similarly, seventh grade was the highest grade in which students took an ELA exam before 10th grade. This restricts prior exam scores in ELA to the sophomore cohort of 2005 and those that followed. Given that my central research question concerns changes from 2004 to 2005, I am only able to use prior test scores in mathematics in my primary analyses.[1]

4.2 Lottery Records

[1] Although the absence of prior ELA scores could potentially decrease the precision of my estimates, neither of my analytic approaches relies on prior scores to account for student sorting as in a value-added modeling approach. In my first approach, I account for potential unobserved differences in baseline characteristics across cohorts at MATCH by subtracting the change in performance among students at other Boston-area charter schools from my estimates. The lottery admission process at the center of my second analytic approach allows me to compare lottery winners and losers who are equal in expectation on all characteristics, including prior academic performance, when estimating the effect of ELT tutorials.

I augment the state administrative data described above with 9th grade lottery-admissions records at MATCH from 2003 to 2009.[2] These records contain the names, dates of birth, and lottery numbers for all students who applied as well as information on which applicants received an automatic offer of admission because they had a sibling who was already enrolled at MATCH. MATCH made an initial offer of admission to applicants with randomly-assigned lottery numbers below a given cutoff early in the summer and then extended offers of admission in sequential order to fill open roster spots. Records in the first several years do not contain complete information on the timing of the admissions offer causing me to define and code my *OFFER* variable as any student who was ever offered admission prior to the beginning of the 9th grade school year.[3]

Lottery records show that 3,053 students applied to be members of the MATCH cohorts that would become sophomores in 2003 through 2009.[4] Due to the limited number of applicants for the 2003 cohort, every student who entered the admissions lottery was eventually offered a roster spot by the beginning of the academic year. This prevents me from using the 2003 cohort in my primary analyses in which I exploit the random offer of enrollment as an instrumental variable because there are no students for whom *OFFER* is equal to zero in that year. Of the applicant cohorts from 2004-2009, 46.07% ever received an offer of enrollment before the beginning of the school year and 16.47% enrolled at MATCH, while another 13.52% attended other Boston-area charter high schools.

[2] Note that I refer to the year of the lottery-admissions record as the academic year in which applicants were sophomores.
[3] This is the same primary approach taken by Abdulkadiroglu and his colleagues (2011) in coding lottery admissions data for use as an instrumental variable.
[4] Lottery data were matched to the SIMS database using last names, first names, dates of birth and application year. In some cases, this procedure did not produce a unique match. I conducted a second round of matching by hand based on fewer criteria and cross-checked against additional variables such as town of residence, middle name, race and free and reduced price lunch status when available. This resulted in matches for 85.00% of all lottery records.

5. Charter-School Difference-in-Differences

My first empirical approach makes use of data on all students from the sophomore cohorts in 2004 and 2005 who ever attended MATCH or another Boston-area charter high school before taking the 10th grade MCAS. I include all Boston-area charter high schools in operation from 2004 to 2005.[5] Like MATCH, these six schools are characterized by their high academic expectations, strict codes of conduct, and college-preparatory curricula.[6] Restricting my sample to students with valid test scores results in an analytic sample of 589 students. In further analyses, I expand this sample to include the 2,635 students who ever attended one of these charter schools as part of the sophomore cohorts of 2002 through 2009. This expanded sample allows me to examine whether any effects of implementing ELT tutorials at MATCH were sustained over time as well as to account for potential differences in student achievement trends across schools.

5.1 Analytic Approach

The arguably exogenous implementation of the MATCH Tutor Corps program provides the opportunity to estimate the effect of ELT tutorials on student achievement by employing a difference-in-differences design. I obtain a "first difference" that estimates the difference in average academic achievement for the cohorts immediately before and just after the implementation of the ELT tutorials at MATCH, serving as control and treatment groups respectively. From this, I subtract a "second difference" in average achievement that summarizes

[5] An alternative approach would be to derive weights for each school in the comparison group to create a synthetic control group that most closely approximates the pre-intervention trends in achievement at MATCH. See Abadie et al. (2010) for a detailed discussion of synthetic control methods.

[6] These schools include Academy of the Pacific Rim Charter School, Boston Collegiate Charter School, City on a Hill Public Charter School, Codman Academy Charter Public School, Healthy Careers Academy Charter School, and Prospect Hill Academy Charter School.

any secular trend in achievement at MATCH estimated using all other Boston-area charter schools over the same period (Murnane & Willett, 2011).

Importantly, the implementation of the ELT tutorials satisfies two key assumptions required for the internal validity of my conclusions. First, due to the timing of the school's initial public presentation of the extended day schedule, students who were in the sophomore cohorts at MATCH just before, and just after, the school day was extended by two hours could not have anticipated the change when applying to the admissions lottery or when deciding to accept an offer of enrollment. The sophomore cohort of 2004 and 2005 both applied to the MATCH lottery before school leaders began to discuss the idea of extending the school day or of creating the MATCH Tutor Corps. Secondly, careful inspection of school documents and interviews with staff members at MATCH lend strong support to the assumption that extending the academic day to accommodate additional tutoring was the only substantial concurrent change at MATCH from 2004 to 2005. The 9th and 10th grade academic schedules of the sophomore classes of 2004 and 2005 were identical, apart from the two hours of additional tutoring students received Monday through Thursday. There were also no changes in the core leadership and administrative staff of the school over the same period.

I implement the charter-school difference-in-differences (charter diff-in-diffs) strategy described above by modeling student achievement in ELA or mathematics on the 10th grade MCAS state exam for student *i*, at school s, in year *t* as follows:

(I) $$Y_{ist} = \beta_1 YEARS\_M_{it} + \beta_2 POST_t + \beta_3 (YEARS\_M_{it} * POST_t) + \alpha X_{it} + \pi_s + \varepsilon_{ist}$$

Here, *YEARS_M* captures the number of years a student attended MATCH prior to taking the 10th grade MCAS. The variable *POST* represents an indicator for all observations in 2005 or after, the years post-implementation of ELT tutorials. The parameter β_3 provides an estimate of the population average treatment effect (ATE) per year of the ELT tutorial program. In all model specifications, I include a vector of student demographic characteristics, X_{it}, that consists of controls for gender, race, age, non-native English speakers, low-income status, and special education status. Finally, I include fixed effects for the schools students attended while taking their 10^{th} grade MCAS exam, π_s, to control for all time-invariant differences across schools. This restricts my identifying variation to changes in average achievement within schools over time. I account for potential serial correlation among residuals within schools over time by clustering my standard errors at the school level.

This modeling approach addresses the important challenge of student mobility and attrition among high-school students in two key ways.[7] Following Abdulkadiroglu et al. (2011), I code students as having attended a full year at MATCH if they were ever enrolled in a given year. This allows me to include every student who ever attended MATCH in my estimate of the effect of ELT tutorials on student achievement, rather than only those who persisted through the end of 10^{th} grade. Second, I account for the endogenous choice of whether and where students chose to transfer schools by including fixed effects for the schools students attended in the spring of 10^{th} grade when they took their MCAS exams. I also examine the robustness of my estimates from model (I) by controlling for student attendance patterns across the seven charter high schools included in my sample. I do this by refitting model (I) with additional fixed effects for having ever attended a given charter school and a vector of controls for the number years attended at each charter school. I further account for any differential enrollment or attrition

[7] Detailed attendance data in the SIMS database allow me to calculate two-year mobility rates among students at MATCH, at other Boston-area charter schools, among Boston Public School students and across the state as a whole. These mobility rates capture the percentage of 9^{th} graders who originally enrolled in a given school and who persisted at that school through the end of 10^{th} grade pooled across the 2003 through 2009 school years. The mobility rate at MATCH over this period was 29.28% which was higher than the rate among all other Boston-area Charter high schools, 20.61%, but lower than the rate among students who attend Boston Public Schools, 35.28%. As expected, these mobility rates all exceeded the state average of 17.74%.

patterns by including 8th grade MCAS mathematics scores in my set of covariates.[8] The absence of reliable test records before 2002 prevents me from also including a prior measure of achievement in ELA as described above.

Two additional potential threats to the charter diff-in-diffs model remain. It is possible that any estimated effects are due to differential trends in student achievement at MATCH and other Boston-area charter high schools over time or to chance sampling variation across years. I am able to examine both potential threats by employing my full panel dataset. First, I examine whether any estimated effect of ELT tutorials is sustained over time by refitting model (I) using data from 2002 to 2009. I replace the main effect of $POST$ with fixed effects for years to account more flexibly for any shocks to achievement that are common across all students in a year.

Second, I modify model (I) in order to estimate linear achievement trends separately for MATCH and all other Boston-area charter high schools both before, and after, the implementation of ELT tutorials.

$$\text{(II)} \qquad Y_{ist} = \beta_1 YEARS\_M_{it} + \beta_2 POST_t + \beta_3 (YEARS\_M_{it} * POST_t) + \beta_4 TREND_{t-2005} + \beta_5 (POST_t * TREND_{t-2005}) + \beta_6 (YEARS\_M_{it} * TREND_{t-2005}) + \beta_7 (POST_t * TREND_{t-2005} * YEARS\_M_{it}) + \alpha X_{it} + \pi_s + \epsilon_{ist}$$

The inclusion of a linear trend for year centered on 2005, $TREND_{t-2005}$, and its two- and three-way interactions with $YEARS\_M_{it}$ and $POST_t$ allow the achievement trends to vary flexibly. In this specification, β_3 again represents the population average treatment effect per semester of ELT tutorials. As before, I examine the robustness of these sensitivity tests to the inclusion of

[8] Valid 8th grade MCAS mathematics scores are available for 74% of the 2004 and 2005 charter diff-in-diffs sample. Models that include standardized 8th grade MCAS mathematics scores as a control are estimated using Multiple Imputation following Rubin (1987) in order to maintain a consistent sample across model specifications. I construct twenty distinct data sets where the missing data has been imputed using the full set of student demographic variables, 10th grade achievement scores, and school indicators as predictors. I calculate the average effect across the twenty imputed data sets and the average standard error corrected for the degrees of freedom used in the multiple imputation process.

fixed effects for ever attending a given charter school and the number of years attended. The addition of the 2002 and 2003 cohorts prevent me from also conducting robustness tests that include 8th grade MCAS mathematics scores as baseline controls.

5.2 Findings

Descriptive statistics reveal that comparison-group students are, on the whole, similar if not more advantaged than students who attended MATCH. In Table 1, I present sample means of selected demographic characteristics and middle school MCAS results for the 2004 and 2005 sophomore cohorts of students who were ever enrolled at MATCH or one of the Boston-area charter high schools. Boston-area charter students are more likely than MATCH students to be white (20% vs. 2%) and less likely to be from low-income families (57% vs. 82%). Comparison-group students also entered high school with higher exam scores in mathematics and, to a lesser degree, in ELA.

Students as Boston-area charter high schools only provide an appropriate estimate of the secular trends in achievement at MATCH if student characteristics appear to be changing over time in similar ways across both groups. In Table 2, I present descriptive statistics for changes in observable student characteristics at MATCH and comparison group students over time as well as the difference between these changes. Despite differences in average student characteristics presented in Table 1, the absence of any statistically significant difference in the rates of change on observable student characteristics in Table 2 suggests that students at Boston-area charter high schools capture the underlying secular trend in academic achievement at MATCH.

I present estimates of the causal effect of ELT tutorials on student achievement in English language arts and mathematics from my fitted regression models (I) and (II) in Table 3. Using my primary specification of model (I), I estimate that one semester of ELT tutorials increased student achievement on the MCAS test by 0.29 test-score standard deviations (hereafter, σ) in ELA (p<.001) and 0.11σ in mathematics (p=0.036). Adding controls for having ever attended a given charter school and the number of years attended slightly increases the magnitude of these estimates (column 2). When I include prior test scores in mathematics in column 3, estimates in ELA remain large and significant while the smaller effects in mathematics are attenuated and are no longer statistically significant.

Estimates of the effect of ELT tutorials on student achievement in ELA are robust to the inclusion of my full panel of data and to a model that allows for differential achievement trends across the treatment and comparison groups. Using model (I) with an expanded panel, I estimate that ELT tutorials increased achievement in English language arts by approximately 0.14σ (p<.01) without and with additional controls for student mobility across charter schools (columns 4 & 5). This suggests that the effect of ELT tutorials estimated within the two year sample is somewhat inflated by chance sampling variation across years. However, models which include additional terms to account for potential differential trends produce somewhat larger effects in ELA than suggested by the simple expanded panel (columns 6 & 7). Most notably, including both differential trends and additional charter mobility controls results in an estimated effect of 0.25σ (p=0.021). At the same time, corresponding estimates across all alternative specifications confirm that ELT tutorials did not appear to affect students' performance on the 10th grade mathematics exam. Results for mathematics across all specifications using the expanded panel are near zero in magnitude and insignificant.

Graphical analysis helps to illustrate these findings. In Figures 1 and 2, I present trends in the average 10th grade achievement for MATCH students from 2002 to 2009 as well as for the Boston-area charter comparison group and other alternative comparison groups discussed below.

Figure 1, which plots trends in ELA achievement illustrates the similar performance of students at MATCH and other Boston-area charter schools prior to 2005. The effect of the ELT tutorials is evident in the large vertical rise in scores for MATCH students between 2004 and 2005. We also see that these gains, relative to students in other Boston-area charters, are maintained through 2009 with the exception of a temporary decline in scores in 2007. In contrast, we see little evidence in Figure 2 of a differential gain made by MATCH students on the mathematics exam in 2005. Students at MATCH began to outperform their peers at other Boston-area charter schools in mathematics starting in 2003 and have maintained a large relative advantage as tests scores increased steadily across all groups over time.

5.3 Alternative Comparison Groups

Figures 1 and 2 also depict trends in achievement among two alternative comparison groups, all students in Boston Public Schools and MATCH lottery applicants who were not offered admission. The average achievement trends among these alternative comparison groups provide no evidence of a sudden rise in ELA test scores among either group in 2005 which might explain away the estimated effect of ELT tutorials. I test the robustness of my results formally by refitting models (I) and (II) using these alternative comparison groups. Findings from these analyses, presented in Table 4, confirm that my results in ELA are not sensitive to the definition of the comparison group. Across all model specifications, estimates for ELA are significant and similar in magnitude to the corresponding estimates reported above. Most strikingly, point estimates of the average sustained effect of ELT tutorials on ELA achievement across the full panel of data available for each group are all within two one-hundredths of a standard deviation of 0.15σ. Results in mathematics illustrate the importance of testing for differential trends and using multiple comparison groups. Initial effect estimates in mathematics are positive and significant when BPS students are used as the comparison group (Panel A columns 1-3). However, these results are eliminated by allowing for differential trends in achievement before and after MATCH adopted ELT tutorials and fail to appear when using lottery losers as the comparison group.

6. Instrumental-Variables Difference-in-Differences

The open-lottery admissions process at MATCH provides an important opportunity to test two key assumptions of the charter-diff-in-diffs estimation approach. First, I have assumed that any differential selection into MATCH across cohorts that might be confounded with the treatment effect is controlled away by 1) conditioning on student demographic characteristics and prior achievement in mathematics, and 2) removing the secular trend estimated from students who attended other Boston-area charter schools. Although I find that my results are not sensitive to two alternative definitions of the comparison group, it is still possible that this assumption is violated. Second, I have assumed that differential attrition patterns across schools over time are not driving my results. I attempt to address this concern by including fixed effects for ever attending a given charter school, and controls for the number of years attended at each charter; however, it is possible that these variables do not fully account for the endogenous mobility patterns among high school students.

I address these two assumptions by using the random offer of enrollment to MATCH as an instrument for the endogenous choice to attend and stay at MATCH. This instrumental-variables adjustment to the basic difference-in-differences approach (IV diff-in-diffs) accounts for the first assumption by estimating the effect of attending MATCH within lottery applicant cohorts. Thus, any change in the composition of the applicant pool over time is removed by identifying these effects using lottery winners and losers within the same applicant cohorts. My IV diff-in-diffs estimates are also not affected by endogenous student mobility patterns directly because this estimation technique is driven by comparisons of lottery winners and losers regardless of which school they ultimately attended or remained enrolled at through 10^{th} grade (Abdulkadiroglu et al., 2011). The 2SLS reduced-form estimates of the effect of the offer of enrollment on student achievement provide a simple and intuitive way to verify this.

Despite these important advantages, the IV diff-in-diffs approach also comes with several drawbacks. First, the 2SLS estimation procedure restricts the variability in my question predictor at the second stage, which reduces the precision of my estimates. Therefore, I expect that my IV diff-in-diffs estimates will have larger standard errors than estimates derived from my charter diff-in-diffs approach. Second, the 2004 sophomore cohort was the first cohort where all roster spots were filled before an offer of admission could be made to every lottery applicant. Thus, I cannot extend my primary IV diff-in-diffs approach to include multiple cohorts prior to the implementation of ELT tutorials. Finally, IV methods provide Local Average Treatment Effects (LATE) which generalize to a more narrowly defined set of students (Imbens & Angrist, 1994). These LATE estimates will characterize the effect of ELT tutorials only for students whose decision to attend MATCH was a consequence of the admissions lottery.

I draw on a sample of 540 students who applied to attend MATCH in the sophomore cohorts of 2004 and 2005 and for whom I have valid 10^{th} grade test scores. I exclude all lottery applicants who received an automatic offer of enrollment due to the priority given to students with siblings enrolled at MATCH. I also extend these analyses with additional lottery cohorts through 2009 which expands my analytic sample to include 1998 students.

6.1 Analytic Approach

To implement my IV diff-in-diffs estimation approach, I specify a modified version of model (I) where I have replaced school fixed effects with indicators for lottery-application cohorts following Angrist et al. (2010).

$$\text{(III. a)} \quad Y_{ist} = \beta_1 YEARS\_M_{it} + \beta_2 POST_t + \beta_3 (YEARS\_M_{it} * POST_t) + \alpha X_{it} + \sum_j \delta_j d_{ij} + \varepsilon_{ist}$$

Here the set of indicator variables d_{ij} controls for each lottery-application cohort indexed by j. All other variables remain as defined previously. Parameter β_3 again represents the quantity of interest. To arrive at a causal estimate of β_3, I use the random offer of enrollment in each year, $OFFER04_{it}$ and $OFFER05_{it}$, as instrumental variables and specify two first-stage equations as follows:

$$\text{(III. b)} \quad YEARS\_M_{it} = \gamma_1 OFFER04_{it} + \gamma_2 OFFER05_{it} + \gamma_3 POST_t + \tau X_{it} + \sum_j \rho_j d_{ij} + \xi_{ist}$$

(III. c)

$$(YEARS\_M_{it} * POST_t) = \delta_1 OFFER04_{it} + \delta_2 OFFER05_{it} + \delta_3 POST_t + \phi X_{it} + \sum_j \omega_j d_{ij} + r_{ist}$$

In addition to the two instrumental variables necessary to satisfy the identification requirements of 2SLS in this setting (Angrist & Pischke, 2009), I also include my full set of covariates that are carried through to the second stage equation above. These include my vector of student demographic control variables, fixed effects for lottery-application cohorts, and my indictor for all years post implementation of ELT tutorials, $POST_t$.

I test the sensitivity of my primary IV diff-in-diffs results in two ways. I expand the model described above to include my full panel of lottery data through 2009 to examine the degree to which chance sampling variation might be driving my results. As before, I replace the main effect of $POST_t$ with a more flexible set of fixed effects for years in these specifications. I augment each of my first-stage equations to include additional instrumental variables for the random offer of admission interacted with application-cohort indicators from 2006 to 2009.[9] I also augment my primary models as well as this expanded-panel specification with a control for 8th grade mathematics test scores. I estimate standard errors clustered at the school level across all models.

6.2 Findings

The implementation of an instrumental variables approach imposes two additional requirements on the diff-in-diffs estimation procedure. First, the offer of enrollment must be orthogonal to the error term and second, it must be correlated with the number of years that a student attends MATCH. In Table 5, I present results from linear probability models where I regress individual student demographic characteristics and baseline measures of achievement on the offer of enrollment aggregated across years as well as application cohort indicators. Conditional on cohort, winning the lottery is uncorrelated with student characteristics or baseline student achievement, which suggests the lottery admissions process was indeed random.

Reduced-form estimates of the effect of this random offer to attend MATCH just before, compared to just after, the implementation of ELT tutorials confirm the findings of the Charter Diff-in-Diff analyses. In Table 6, I present results from reduced-form models using the two-year and the full-panel lottery samples. I find that among the 2004 sophomore cohort, those who received a random offer of admission appear to have performed worse on the ELA test than those who did not receive an offer although the negatively signed coefficient on $OFFER04$ is imprecisely estimated. In contrast, students who won the lottery among the 2005 cohort performed 0.20σ better in ELA than those who lost (p=0.097), independent of whether they

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[9] These over-identified models serve to increase the precision of my estimates as compare to a just-identified model using $OFFER_{it}$ and its interaction with $POST_t$ as instruments (Angrist, Pathak, & Walters, 2011).

choose to accept an offer of enrollment. A general linear hypothesis (GLH) test of the difference between these two coefficients shows that the *offer* to addending MATCH with ELA tutorials increased student achievement in ELA by 0.31σ (p<.01). Expanding my panel to include lottery applicant cohorts through 2009 only slightly reduces this estimate to 0.26σ (p=0.027).

Reduced-form estimates for mathematics achievement suggest that students who were offered admission at MATCH outperformed their peers who lost the lottery in both 2004 and 2005. I estimate the effect of an offer of enrollment to be 0.27σ and 0.15σ in 2004 and 2005, respectfully, although these effects are imprecisely estimated. A GLH test confirms that there was no statistically significant change across cohorts. These results suggest that students at MATCH were already experiencing large gains in mathematics achievement before the implementation of ELT tutorials.

As required by the IV approach, I also find that the random offer of enrollment is a strong predictor of the number of years a student attended MATCH. In Table 7, I present fitted result from models (III.b) and (III.c), the simultaneously estimated first-stages equations of my IV diff-in-diffs approach. These estimates demonstrate that the offer of enrollment for each cohort is a strong and statistically significant predictor of the number of years attended at MATCH. Students in the 2004 sophomore cohort who were offered the opportunity to enroll at MATCH attended about 0.6 years on average while a lower take-up rate among the 2005 cohort results in an average enrollment of approximately 0.4 years. The predictive power of $OFFER05$ remains almost unchanged when used to predict $(YEARS\_M_{it} * POST_t)$, while receiving an offer of enrollment for the 2004 sophomore cohort, $OFFER04$, is unrelated to years attended at MATCH among the sophomore cohort of 2005. This makes sense as previous lotteries should be independent from the enrollment of future cohorts. Larger coefficients on the $OFFER$ variables for the 2008 and 2009 cohorts from models using the expanded panel indicate that later cohorts were more likely than earlier cohorts to take up the offer of enrollment at MATCH. F-tests of the joint significance of the instruments included in each first-stage model result in F-statistics well above the ad-hoc value of 10 often used as a threshold for identifying weak instruments (Stock & Watson, 2007).

I present second-stage estimates of the per-year effect of ELT tutorials in Table 8. To gain intuition about how the magnitudes of these estimates are determined, it is helpful to remember that 2SLS estimates with a single endogenous regressor are the ratio of the reduced-form estimate to the first-stage estimate. Here, the coefficients on my set of instrumental variables from the first-stage are close to or less than one for all instruments. This is a result of the fact that students who were offered a roster spot at MATCH did not always accept this offer or persist at MATCH for two years. Dividing my reduced-form estimates by these fractions inflates my 2SLS estimates accordingly, although the multiple first-stage equations involved in my diff-in-diffs framework require slightly more complex calculations. This IV-diff-in-diffs approach results in an estimated effect of approximately 0.70σ for ELA using my two-year sample with prior mathematics tests scores (p=0.065). Similar to my charter-diff-in-diffs analyses, I find that this estimate is somewhat attenuated when I include more cohorts. The corresponding estimate using data from 2004-2009 is 0.43σ (p=0.093). These findings provide strong support for the substantive conclusion that ELT tutorials had a large positive effect on student achievement in ELA. They suggest that, if anything, the assumptions of the charter-diff-in-diffs method result in a downward bias on the magnitude of the estimated effects. Effects on mathematics achievement remain indistinguishable from zero across all second-stage models.

6.3 Alternative Instrumental Variables

I test the sensitivity of these results by replacing my instrumental variables for whether a student ever received an offer of enrollment with a flexible function of the randomly assigned lottery numbers themselves (Angrist, 1990; Angrist & Krueger, 1992). Lottery numbers are strong predictors of the number of years a student attended MATCH because of the sequential order in which offers of admission were made. They also satisfy the exclusion restriction because they are randomly assigned. I specify these lottery numbers as a set of indicator variables for each quintile of the lottery number distribution in each year.[10]

Additional analyses using students' randomly assigned lottery number interacted with lottery-application cohort indicators as an alternative set of instrumental variables confirm the findings above. Comparing the point estimates in Table 8 to these alternative estimates in Table 9 reveals that both sets of instruments produce similar results. Using lottery numbers as the source of exogenous variation also allows me to expand my panel to include the 2003 sophomore cohort. Although everyone in this cohort was eventually offered admission to MATCH, the rolling timing of offers throughout the summer created a strong relationship between the randomly assigned lottery numbers and the number of years students attended MATCH.[11] This relationship is a function of the decreasing probably that students accepted an offer to attend MATCH late in the summer when they would have already arranged to attend another charter school, private school, or traditional Boston Public School. Expanding my panel to include 2003 results in an estimate of the effect of ELT tutorials on ELA achievement of 0.21σ (p=0.048). This result provides further evidence that although estimates using only one year on either side

[10] Specifying lottery numbers as decile indicators or as higher order polynomials, an indicator for *OFFER*, and their interactions does not change the character of these results.

[11] Among the 2003 cohort, the correlation between the number of years a student attended MATCH and their lottery number is -0.43.

of the implementation of ELT tutorials somewhat exaggerate its effect, ELT tutorial resulted in a sudden and permanent change in performance on the MCAS English language arts exam.

6.4 Differential Attrition and Mobility

I examine the potential that dynamic patterns of differential attrition and mobility across lottery winners and losers might account for my findings. The difference-in-differences analytic approach at the core of this study mitigates much of the potential threat posed by attrition and mobility. For example, a pattern where lottery winners were more likely to appear in my analytic dataset or were more likely to change schools would not bias my results. In order to potentially bias my results, I would need to observe a dynamic pattern where differences in attrition or mobility across lottery winners and losers changed after the implementation of ELT tutorials.

Results from regressions analyzing these patterns demonstrate that there is little evidence to suggest my findings are driven by attrition or mobility. As shown in Table 10, the overall difference in attrition rates among lottery winners and losers in both my two-year sample (2004-2005) and my expanded panel (2004-2009) are less than three percentage points and insignificant, conditional on lottery-applicant cohort. Differential follow-up rates post- versus pre-ELT tutorials remain insignificant across both samples suggesting that attrition is unlikely to impart substantial selection bias in my analyses. Overall differences in mobility rates among lottery winners and losers who are matched to the Massachusetts state dataset and have valid 10^{th} grade test scores are also small and insignificant — about two percentage points or less across both samples. Differential mobility patterns post- versus pre-ELT tutorials remain insignificant providing evidence that student mobility does not appear to pose a substantial threat to validity.

7. Examining Differences in Results across Subjects

Given the large effect of the ELT tutorials on student achievement in English language arts, why did extra time for individualized tutorials not lead to measurable improvements in mathematics? Students spent equal amounts of time working with their tutors on ELA and mathematics, one period each per subject. One interpretation of the empirical evidence is that tutoring as an instructional method is simply more effective at raising achievement in ELA than in mathematics (Ritter et al., 2009). I present evidence of two alternative explanations below.

7.1 Differential Potential Gains

One alternative explanation is that the large effect of attending MATCH on students' mathematics achievement prior to the implementation of ELT tutorials reduced the potential impact of these tutorials. Figure 2 helps to illustrate this point. Students at MATCH scored substantially higher on their MCAS mathematics tests in 2003 compared to students at other Boston-area charters.[12] This increase of 0.81σ across the 2002 and 2003 sophomore cohorts raised MATCH into the 83st percentile of average mathematics achievement across high schools statewide. Comparatively, MATCH students were only at the 52th percentile of average ELA achievement in the same year. Using the instrumental variables estimation strategy described above, I estimate that the effect of attending a year at MATCH in 2004 was 0.52σ per year for mathematics ($p=0.035$). In contrast, these same students did no better on the ELA exam than their peers who would have attended MATCH had they been offered a roster spot. Given the extremely large effect size of attending MATCH on students' achievement in mathematics

[12] A review of the changes in academic programing at MATCH reveals that the large gains in mathematics achievement evident in 2003 coincided with the first substantial expansion of instructional time for tutoring at the school. In 2003, two years prior to the implementation of ELT tutorials, MATCH formalized its fledgling weekend tutorial program so that students were required to attend a total of 25 weekend sessions. The school hired college students through work-study programs to tutor sophomores for four hours in mathematics and ELA on either Friday afternoons or Saturday or Sunday mornings. It is possible that these untrained work-study tutors were able to raise student achievement in mathematics, but not in ELA. However, the effect of these weekend tutoring sessions on MCAS scores cannot be isolated from other important changes that occurred concurrently at the school. Thus, I do not make them the subject of a more detailed analysis.

before 2005, it may be that there was less opportunity for improvement in mathematics achievement through the addition of ELT tutorials as compared to ELA.

I explore this hypothesis by using quantile regression to examine how individualized tutorials affected the conditional distribution of test scores among MATCH students. I refit my full-panel specification of the charter diff-in-diffs model reported in column 5 of Table 3 and minimize the sum of absolute weighted deviations for a given quantile, which I specify as every decile of the test-score distribution.[13] In Table 11, I report the corresponding nine estimates for each subject test which reveal substantial and systematic differences in how achievement changed across test score distributions. I estimate that mathematics achievement at the lowest end of the score distribution (10th decile) was 0.41σ (p<0.001) higher for MATCH students who received ELT tutorials. The steady decline in the estimated differences in quantiles suggests that ELT tutorials produced systematically larger gains for lower-performing students who had greater potential for gains than higher performing students.[14] The estimated differences across quantiles become negative by the 50th percentile of the score distribution but begin to oscillate between -0.03σ and -0.09σ rather than continuing in a negative decline.

ELT tutorials also appear to have benefited lower-performing students in ELA most overall, although the estimated differences in quantiles remain positive and exhibit far less heterogeneity across the score distribution. Comparing the slow and continual decline in achievement differences across the score distribution in ELA to the rapid decline and plateau of quantile differences in mathematics suggests that additional gains for higher performing students

[13] Standard errors are calculated by bootstrapping on the estimation sample because STATA does not allow for clustered standard errors when fitting quantile regression.
[14] Interpreting these results as evidence that ELT tutorials raised the performance of lower-achieving students requires that the intervention was rank-preserving, a plausible but unverifiable assumption. See chapter 7 of Angrist & Pischke (2009) for a full discussion.

in mathematics were either beyond the reach of the tutorial program or were not captured by the MCAS 10th grade mathematics exam.

7.2 Test Score Ceiling Effects

A second potential explanation for the null findings in mathematics is that the full effect of ELT tutorials was not captured by the MCAS 10th grade exam. Distributions of the scores of all sophomores who ever attended MATCH after the implementation of ELT tutorials help to illustrate this point. Appendix Figure A1 Panel A shows that the mathematics scores of MATCH students were concentrated at the highest range of the scale, with 31 students scoring within 10 points of the maximum possible score. The comparable distribution for ELA scores shown in Panel B demonstrates how ELA scores were concentrated in the upper-middle range of the score distribution with only two students scoring within 10 points of the maximum possible score. The degree of negative skewness for each distribution provides a helpful measure of the ceiling effect severity. The skewness for mathematics is -1.27 which is approaching the range of severe skewness that Koedel and Betts (2010) demonstrate can induce strong ceiling effects. The skewness for ELA is a more moderate -0.77. The skewed distribution of test scores in mathematics, and to a lesser degree in ELA, is characteristic of "minimum-competency tests" such as the Massachusetts high school exit exam.

I attempt to overcome this data limitation by examining the effect of ELT tutoring on open-response test items separately from multiple-choice items. The 10th grade MCAS mathematics exam includes six open-response items scored on a zero to four point scale. I compute the total points awarded across these six items for each student and use this sum as an alternative outcome. Unfortunately, even these open response items do not appear to differentiate well among high-performing students. As Appendix Figure A2 depicts, the most common total scores among sophomore students who ever attended MATCH after 2004 are concentrated at the highest end of the score distribution. Not surprisingly, replicating my analyses with this outcome does not change the character of my findings in mathematics.[15]

8. Conclusion

In this study, I use two complementary quasi-experimental methods to estimate the causal effect on academic achievement of extending the school day to incorporate two hours of individualized tutorial sessions. I find that, on average, ELT tutorials at MATCH Charter Public High School raised student achievement on the 10th grade English language arts examination between 0.15 and 0.25 standard deviations per year. This is equivalent to approximately an additional years' worth of instruction (Hill et al., 2008). These findings provide evidence that the addition of tutorial classes that are integrated throughout the school day, complement core curriculum materials, and are taught by full-time, recent college graduates have the potential to be an effective school reform for improving student achievement.

These gains are particularly relevant given the lack of evidence on effective educational interventions aimed at increasing student achievement in reading, particularly at the high school level. Reviews of the research on well-known instructional interventions for adolescent readers including Advancement Via Individual Determination (AVID), the Carnegie Learning Curricula and Cognitive Tutor® Software, Accelerated Reader™, Fast ForWord®, and Reading Apprenticeship® find limited or no evidence of their effects on student achievement.[16] Furthermore, studies of "no-excuses" charter schools like MATCH consistently find that students in these schools are making gains in mathematics that are two to three times as large as gains in

[15] Results are available from the author upon request.
[16] See What Works Clearinghouse (WWC) Intervention Reports on these programs for complete summaries.

ELA (Angrist et al., 2010; Abdulkadiroglu et al., 2011; Dobbie & Fryer, 2011a). Individualized tutorials provide an instructional intervention through which these schools might use additional time to further increase student performance in English language arts.

In contrast to ELA, I find that ELT tutorials did not increase average achievement on the 10th grade mathematics exam beyond the large gains MATCH students were already making prior to the implementation of the program. Quantile regression estimates suggests that this estimate of the average effect masks how ELT tutorials raised the lowest end of the test score distribution in mathematics. Fryer's (2012) analysis of the effort to inject successful charter-school practices into failing public schools provides further evidence that ELT tutorials raise mathematics achievement. He finds large effects in the grades in which high-dosage tutorials in mathematics were offered but only modest effects in all other grades. Given these results, it seems likely that high levels of success prior to ELT tutorials and test-ceiling effects at least partially account for the null effects in mathematics achievement.

As with most case studies of individual schools, there are also important limitations to these findings. My data only allow me to arrive at effect estimates with limited precision because of this case study approach and the single grade for which standardized achievement tests are available in Massachusetts high schools. Demographically, students at MATCH are representative of the population of students found in many Boston public high schools. However, to the extent that the students who attend MATCH differ from their peers because of their active choice to apply to a charter school, these findings might have limited generalizability beyond oversubscribed charter schools. Despite these limitations, my findings are still relevant to the population of schools that have adopted ELT, given that charter schools represent almost two-third of all ELT schools.[17]

Any large-scale attempt to extend the school day for individualized tutorials with full-time tutors who are well trained and well supported would require dedicated financial resources. President Clinton's effort to organize unpaid volunteers and work-study college students to serve as tutors through the America Reads Challenge largely succumbed to these financial limitations. One potential opportunity to fund the expansion of ELT tutorials would be to reallocate current educational expenditures on Supplemental Educational Services and 21st Century Community Learning Centers. Experimental evaluations of SESs and 21st CCLCs document large variation in program quality and limited effects on academic achievement. Despite this discouraging evidence, the federal government mandates that 20% of all Title I, Part A funds be used to pay for SESs and school choice transportation costs. In addition, the federal government continues to fund 21st Century Community Learning Centers through Title IV, Part B. Repurposing even a fraction of the $2.95 and $1.26 billion allocated for each program in Present Obama's FY2012 budget could finance ELT initiatives in school districts across the country. For example, a back-of-the-envelope calculation suggests that in 2010, Boston Public Schools could have used its $10.13 million in Title I, Part A funding alone to sponsor Tutor Corps-like programs for over 3,000 students assuming a cost of $20,000 per tutor and a tutor-to-student ratio of 1 to 6.

Clearly, funding ELT is not the only policy challenge in shifting from an afterschool model of supplemental tutoring to an extended school day with individualized tutorials taught by full-time, recent college graduates. This would require school districts and local teacher unions to reach new collectively-bargained agreements that allow for longer school days. Boston Public

[17] I calculate the percent of ELT schools that are charter schools using the database of expanded time schools maintained by the National Center on Time & Learning. As of 8/28/12 this database contained 1035 schools, 621 of which were charter schools.

Schools provides an example of a large urban district with a strong teachers union that has successfully negotiated for extended school days in select turnarounds schools.

Many questions also remain about the importance of each particular characteristic of MATCH ELT tutorials to the success of the program. Would volunteers be as effective as full-time, recent college graduates from highly-competitive schools? Would students achieve comparable gains if tutorials were offered at the end of the school day? Although I am unable to isolate the relative contribution of each aspect of the MATCH ELT tutorial program, the body of evidence on extended learning time and afterschool tutoring suggest that having trained tutors who coordinated with teachers and worked with the same students throughout the academic year in a class-like setting was central to the program's success.

More time in school is only as good as the quality of the programming that schools can provide during that extra time. MATCH has demonstrated one model, in which extended time for learning can be leveraged through creative funding and the motivation and passion of young adults looking to make a difference. One can also envision the expansion of ELT tutorials through an AmeriCorps-like National Tutor Corps program that funds recent college graduates to work full-time as tutors in high-need schools. The strong demand among recent college graduates to participate in programs like MATCH Tutor Corps, City Year, and Teach for America suggests that there are many motivated and capable young adults willing to serve as full-time tutors in schools that have extended the academic day to incorporate tutorial classes.

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Tables & Figures

Table 1: Descriptive Statistics for the 2004 and 2005 Sophomore Cohorts of Students who Ever Attended MATCH or Boston-area Charter Schools

	Demographics										MCAS Scores (SD)			
	Students	Female	White	Black	Hispanic	Asian	Low-income	Special Ed.	Non-Native Eng.	Age	Students	8th Grade Math	Students	7th Grade ELA
MATCH	100	0.65	0.02	0.67	0.26	0.05	0.82	0.13	0.20	16.01	68	-0.63	33	-0.41
Boston-area Charters	489	0.58	0.20	0.65	0.11	0.03	0.57	0.11	0.12	15.97	391	-0.31	183	-0.24
Difference		0.07	-0.18***	0.02	0.15***	0.02	0.25***	0.02	0.08*	0.04		-.32**		-.17
		(0.05)	(0.04)	(0.05)	(0.04)	(0.02)	(0.05)	(0.03)	(0.04)	(0.07)		(0.10)		(0.15)
Boston-area Charters														
School A	55	0.45	0.00	0.89	0.11	0.00	0.78	0.16	0.04	16.15	36	-0.56	21	-0.69
School B	93	0.77	0.08	0.72	0.14	0.04	0.68	0.04	0.23	15.96	83	-0.53	41	-0.13
School C	66	0.50	0.23	0.64	0.08	0.06	0.47	0.12	0.00	16.15	62	-0.06	32	-0.24
School D	95	0.60	0.31	0.47	0.19	0.03	0.40	0.05	0.29	15.81	80	-0.04	44	-0.10
School E	136	0.60	0.04	0.87	0.07	0.01	0.65	0.12	0.06	15.93	90	-0.57	26	-0.56
School F	47	0.45	0.94	0.04	0.00	0.02	0.43	0.23	0.00	15.96	42	0.06	20	0.10

Notes: *$p<0.05$, **$p<0.01$, ***$p<0.001$. Robust standard errors reported in parentheses. Total students who ever attended individual charter schools sums to 492 because three students attended multiple charter schools in the sample.

Table 2: Difference-in-Differences Estimates of Baseline Student Characteristics for the 2004 and 2005 Sophomore Cohorts of Students Who Ever Attended MATCH or Boston-area Charter Schools

	Demographics										MCAS Scores (SD)			
	Students	Female	White	Black	Hispanic	Asian	Low-income	Special Ed.	Non-Native Eng.	Age	Students	8th Grade Math	Students	7th Grade ELA
MATCH														
2004	55	63.64	1.82	70.91	23.64	3.64	78.18	12.73	20.00	16.02	34	-0.773		
2005	45	66.67	2.22	62.22	28.89	6.67	86.67	13.33	20.00	16.01	34	-0.495	33	-0.405
Difference		3.03	0.40	-8.69	5.25	3.03	8.48	0.61	0.00	-0.01		0.278		
Boston-area Charters														
2004	255	60.78	18.43	68.63	11.37	1.18	56.08	10.20	8.63	15.95	193	-0.393		
2005	234	55.98	22.65	61.97	9.83	4.70	58.55	11.54	15.81	15.99	198	-0.229	183	-0.280
Difference		-4.80	4.22	-6.66	-1.54	3.52	2.47	1.34	7.18	0.04		0.164		
Difference-in-Differences		7.83	-3.81	-2.03	6.80	-0.49	6.02	-0.74	-7.18	-0.06		0.11		
		(10.59)	(4.65)	(10.44)	(9.32)	(4.77)	(8.79)	(7.36)	(8.60)	(0.13)		(0.17)		

Notes: *p<0.05, **p<0.01, ***p<0.001. Robust standard errors reported in parentheses. Difference-in-Differences and their associated standard errors are estimated from models that include indicators for ever attending MATCH, for the 2005 year, and their interaction without additional controls.

Table 3: Difference-in-Differences Estimates of the Per-year Effect of ELT Tutorials at MATCH on Student Achievement

	Model (I)					Model (II)	
	2004-2005			2002-2009		2002-2009	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
English Language Arts	0.293***	0.302***	.251**	0.143**	0.134**	0.165*	0.250*
	(0.073)	(0.071)	(0.068)	(0.048)	(0.046)	(0.071)	(0.106)
Mathematics	0.106*	0.118*	0.032	0.029	0.024	-0.060	0.002
	(0.049)	(0.049)	(0.054)	(0.059)	(0.056)	(0.072)	(0.072)
Observations	589	589	589	2,635	2,635	2,635	2,635
Fixed effects for ever attending a charter		Yes	Yes		Yes		Yes
Controls for total semesters attended at each charter		Yes	Yes		Yes		Yes
Prior achievement in 8th grade mathematics			Yes				

Notes: *p<0.05, **p<0.01, ***p<0.001. Standard errors clustered at the school-level reported in parentheses. All models include fixed effects for schools students were attending when they took the 10th grade MCAS exam and for student demographic characteristics. Controls for student demographic characteristics include sex, race, and age as well as indicators for low-income students, special education students, and students who are non-native English speakers. Estimates that include 8th grade MCAS mathematics scores are estimated using multiple impuation with twenty replication data sets. Model (I) is a standard diff-in-diffs specification with school and year fixed effects. Model (II) allows for specific linear time trends for MATCH other Boston-area charter schools both pre and post implementaiotn of ELT tutorials.

Table 4: Difference-in-Differences Estimates of the Per-year Effect of ELT Tutorials at MATCH on Student Achievement Using Alternative Comparison Groups

	Model (I)			Model (II)
	2004-2005		2002-2009	2002-2009
	(1)	(2)	(3)	(4)
Panel A: Boston Public Schools Comparison Group				
English Language Arts	0.290***	.218***	0.135***	0.137**
	(0.031)	(0.038)	(0.023)	(0.043)
Mathematics	0.212***	.111**	0.192***	-0.064
	(0.043)	(0.036)	(0.025)	(0.056)
Observations	7,783	7,783	29,997	29,997
Panel B: Lottery Looser Comparison Group				
English Language Arts	0.344***	.314***	0.147*	
	(0.064)	(0.063)	0.068	
Mathematics	0.089	0.036	-0.069	
	(0.063)	(0.050)	(0.057)	
Observations	339	339	1,407	
Prior achievement in 8th grade mathematics		Yes		

Notes: *p<0.05, **p<0.01, ***p<0.001. Standard errors clustered at the school-level reported in parentheses. See Table 3 notes for futher details.

Table 5: Tests of Covariate Balance across Lottery Winners and Loosers

	2004-2005	2004-2009
	(1)	(2)
Female	0.049	-0.009
	(0.046)	(0.037)
White	0.011	0.009
	(0.026)	(0.012)
Black	-0.003	0.02
	(0.041)	(0.024)
Hispanic	-0.002	-0.023
	(0.032)	(0.019)
Asian	0.002	-0.005
	(0.026)	(0.010)
Low-income	0.057	0.004
	(0.047)	(0.023)
Special Education	-0.001	-0.028
	(0.032)	(0.018)
Non-native English Speaker	0.051	-0.004
	(0.035)	(0.016)
Age (years)	-0.012	-0.011
	(0.046)	(0.041)
Observations	540	1,998
MCAS Math 8th Grade	-0.02	0.008
	(0.084)	(0.045)
Observations	395	1,640
MCAS ELA 7th Grade	0.032	0.049
	(0.139)	(0.055)
Observations	232	1,451

Notes: Standard erros clustered at the school level in parentheses. Each cell reports the results of a separate regression of a given student characteristic on an indicator for receiving a lottery offer. All models include fixed effects for lottery-applicant cohorts.

Table 6: Reduced-form Estimates of the Effect of the Random Offer of Enrollment at MATCH on Student Achievement

	English Language Arts		Mathematics	
	2004-2005	2004-2009	2004-2005	2004-2009
	(1)	(2)	(3)	(4)
OFFER 04	-0.109	-0.116	0.269	0.251
	(0.111)	(0.110)	(0.198)	(0.197)
OFFER 05	0.197+	0.230*	0.145	0.197
	(0.117)	(0.114)	(0.145)	(0.140)
OFFER 06		0.052		0.010
		(0.081)		(0.115)
OFFER 07		0.011		0.100
		(0.090)		(0.132)
OFFER 08		0.244		0.332
		(0.200)		(0.249)
OFFER 09		0.178		0.233
		(0.170)		(0.231)
Difference between the average of	.307**	.264*	-0.124	-0.068
OFFER 05-*OFFER* 09 and *OFFER* 04	(0.114)	(0.118)	(0.125)	(0.134)
Observations	540	1,998	540	1,998

Notes: +p<0.10, *p<0.05, **p<0.01. Standard errors clustered at the school-level reported in parentheses. All fitted models include indicators for lottery-application cohorts and student demographic controls for sex, race, and age as well as indicators for low-income students, special education students, and students who are non-native English speakers.

Table 7: First-stage Estimates of the Effect of the Random Offer of Enrollment on the Number of Years Attended at MATCH

	NUM_YEAR		*NUM_YEAR_POST*	
	2004-2005	2004-2009	2004-2005	2004-2009
OFFER 04	0.591***	0.609***	0.030	0.048
	(0.090)	(0.089)	(0.058)	(0.081)
OFFER 05	0.374***	0.386***	0.384***	0.390***
	(0.071)	(0.067)	(0.046)	(0.061)
OFFER 06		0.248***		0.247***
		(0.074)		(0.068)
OFFER 07		0.396***		0.390***
		(0.067)		(0.061)
OFFER 08		1.052***		1.050***
		(0.069)		(0.063)
OFFER 09		0.793***		0.793***
		(0.075)		(0.069)
F-statistic from joint F-test	35.82***	84.25***	35.17***	78.61***
Observations	540	1,998	540	1,998

Notes: *p<0.05, **p<0.01, ***p<0.001. Standard errors reported in parentheses. All fitted models include indicators for lottery-application cohorts and student demographic controls for sex, race, and age as well as indicators for low-income students, special education students, and students who are non-native English speakers.

Table 8: 2SLS Estimates of the Per-year Effect of ELT Tutorials at MATCH on Student Achievement

	2004-2005		2004-2009	
	(1)	(2)	(3)	(4)
English Language Arts	0.731*	0.701+	0.476+	0.433+
	(0.361)	(0.380)	(0.263)	(0.260)
Mathematics	-0.070	-0.120	-0.105	-0.176
	(0.296)	(0.187)	(0.269)	(0.210)
Observations	540	540	1,998	1,998
Prior achievement in 8th grade mathematics		Yes		Yes

Notes: +p<0.10, *p<0.05, **p<0.01. Standard errors clustered at the school-level in parentheses. Each cells contains results from separate regressions. All fitted models include indicators for lottery-application cohorts and student demographic controls for sex, race, and age as well as indicators for low-income students, special education students, and students who are non-native English speakers. Estimates that include 8th grade MCAS mathematics scores are estimated using multiple impuation with twenty replication data sets.

Table 9: 2SLS Estimates of the Per-year Effect of ELT Tutorials at MATCH on Student Achievement Using Lottery Numbers as Alternative Instrumental Variables

	2004-2005		2004-2009		2003-2009
	(1)	(2)	(3)	(4)	(5)
English Language Arts	0.550*	0.356+	0.399*	0.315*	0.210*
	(0.251)	(0.209)	(0.180)	(0.151)	(0.105)
Mathematics	0.286	-0.030	0.143	0.004	0.004
	(0.232)	(0.171)	(0.183)	(0.130)	(0.141)
Observations	540	540	1,998	1,998	2,101
Prior achievement in 8th grade mathematics		Yes		Yes	

Notes: +p<0.10, *p<0.05, **p<0.01. Standard errors clustered at the school-level reported in parentheses. Each cells contains results from separate regressions. Lottery number instruments are a set of indicator variables for each quintile of the lottery number distribution in each year. See Table 8 notes for further details.

Table 10: Differential Attrition and Mobility across Lottery Winners and Losers

	2004-2005	2004-2009
	(1)	(2)
Differential Attrition (winner - loser)		
Overall	-0.026	0.010
	(0.033)	(0.018)
Post - Pre ELT Tutorials	-0.104	-0.052
	(0.068)	(0.059)
Observations	772	2,887
Differential Mobility (winner - loser)		
Overall	-0.013	0.022
	(0.035)	(0.018)
Post - Pre ELT Tutorials	-0.006	0.027
	(0.070)	(0.057)
Observations	540	1,998

Notes: *p<0.05, **p<0.01, ***p<0.001. Robust standard errors reported in parentheses. Differential attrition sample includes all students who ever applied to attend MATCH. Differential mobility sample is consists of all students included in the IV Diff-in-Diffs analytic samples. All regressions include fixed effects for academic years.

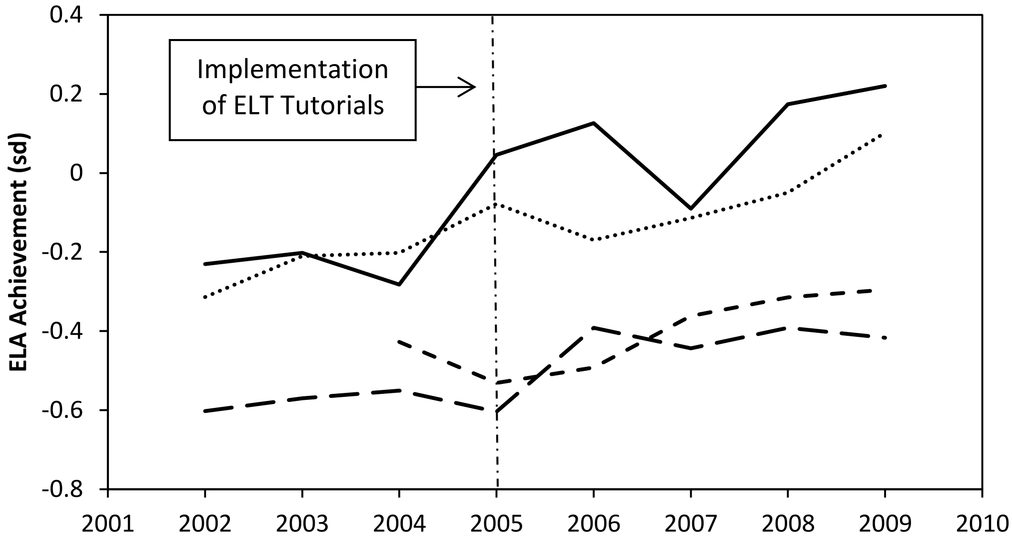
Table 11: Quantile Regression Estimates of the Per-year Effect of ELT Tutorials at MATCH on Student Achievement at each Decile of the Score Distribution.

	Quantile								
	10th	20th	30th	40th	50th	60th	70th	80th	90th
English Language Arts	0.213*	0.222**	0.171*	0.124	0.150*	0.107*	0.098	0.099+	0.018
	(0.100)	(0.083)	(0.070)	(0.081)	(0.063)	(0.052)	(0.071)	(0.055)	(0.076)
Mathematics	0.408***	0.166+	0.081	0.024	-0.034	-0.077+	-0.091*	-0.048	-0.085*
	(0.090)	(0.087)	(0.090)	(0.077)	(0.071)	(0.042)	(0.039)	(0.058)	(0.035)
Observations	2,635	2,635	2,635	2,635	2,635	2,635	2,635	2,635	2,635

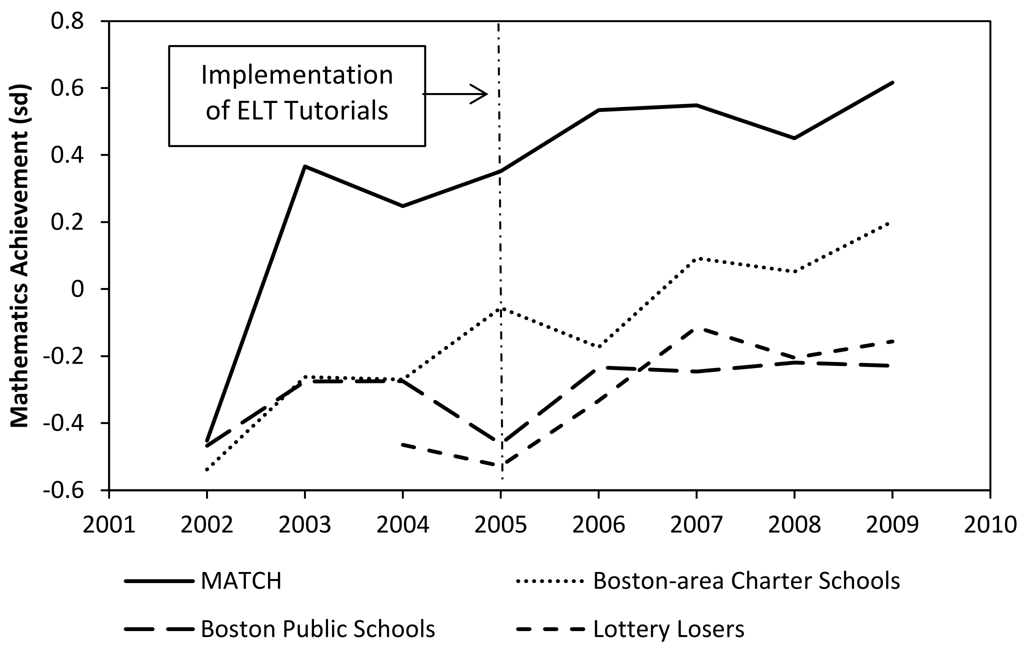
Notes: +p<0.10, *p<0.05, **p<0.01. Bootstrapped standard errors reported in parentheses. Each cells contains results from a separate regression. All models include fixed effects for schools students were attending when they took the 10th grade MCAS exam and for student demographic characteristics. Controls for student demographic characteristics include sex, race, and age as well as indicators for low-income students, special education students, and students who are non-native English speakers.

Figure 1: Trends in Average Achievement on the 10th Grade MCAS Exam from 2002 to 2009

Panel A: English Language Arts



Panel B: Mathematics



Notes: Achievement trends for MATCH and Boston-area Charter Schools are conservative estimates adjusted for student mobility. These data points represent the average achievement of a prototypical student who attended MATCH or one of the other Boston-area charters for all of 9th and 10th grade before taking the MCAS examinations. I obtain these estimates by first fitting a series of bivariate regressions of standardized MCAS scores on *YEAR_M* or an equivalent measure of years attended at one of the comparison group charters in the full statewide sample of students for each subject and year. I then estimate the average achievement in each subject and year of a prototypical student who attended two years by calculating the linear combination of the intercept plus twice the parameter estimate on the corresponding years attended variable. Achievement trends estimated solely from students still enrolled at MATCH or other charter schools in 10th grade would fail to account for student attrition. In practice, these fitted achievement trends reflect the same trends as simple averages of only those students who persisted through 10th grade, but are slightly lower in absolute magnitude.

Appendix

Figure A1: 10th Grade MCAS Scores among Students Who Ever Attended MATCH from 2005 to 2009

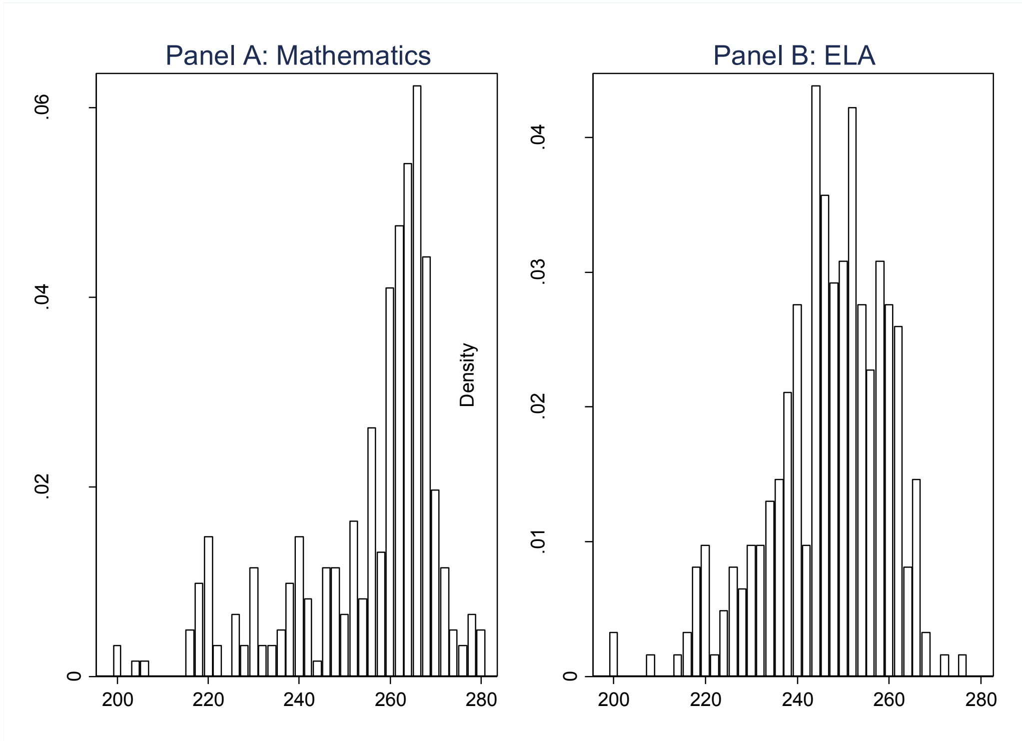


Figure A2: Total Points Scored on the Six Open-Response 10th Grade MCAS English Language Arts items among Students Who Ever Attended MATCH from 2005 to 2009