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EXECUTIVE SUMMARY

The Knowledge Is Power Program (KIPP) is the largest network of public charter schools in the United States. KIPP began as a network of urban middle schools designed to serve underserved communities, with the goal of closing achievement gaps and preparing disadvantaged students to succeed in college. Prior research has demonstrated that KIPP has large positive impacts on student achievement (Angrist et al. 2010; Tuttle et al. 2013; Gleason et al. 2014; Tuttle et al. 2015; Knechtel et al. 2017). However, until now it has remained an open question whether these initial achievement gains (measured by improvements in standardized test scores) will ultimately lead to improvements in students' longer-term outcomes, such as college enrollment and graduation.

In this report, we present the results of a long-term tracking study that follows 1,177 students who applied to enter 1 of 13 oversubscribed KIPP middle schools through a 5th or 6th grade admissions lottery in 2008 or 2009. Those students are now old enough to have attended college for at least two years. This study uses a randomized controlled trial design to ensure that students who were offered admission to a KIPP middle school (the treatment group) are similar on average to students who did not receive an offer of admission (the control group) on both observable characteristics, such as prior test scores, and unobservable characteristics, such as levels of motivation and parental support. The study focuses on two primary research

questions:

1. What impact do KIPP middle schools have on students' enrollment in a four-year college?
2. What impact do KIPP middle schools have on persistence in four-year college programs during the first two years after high school graduation?

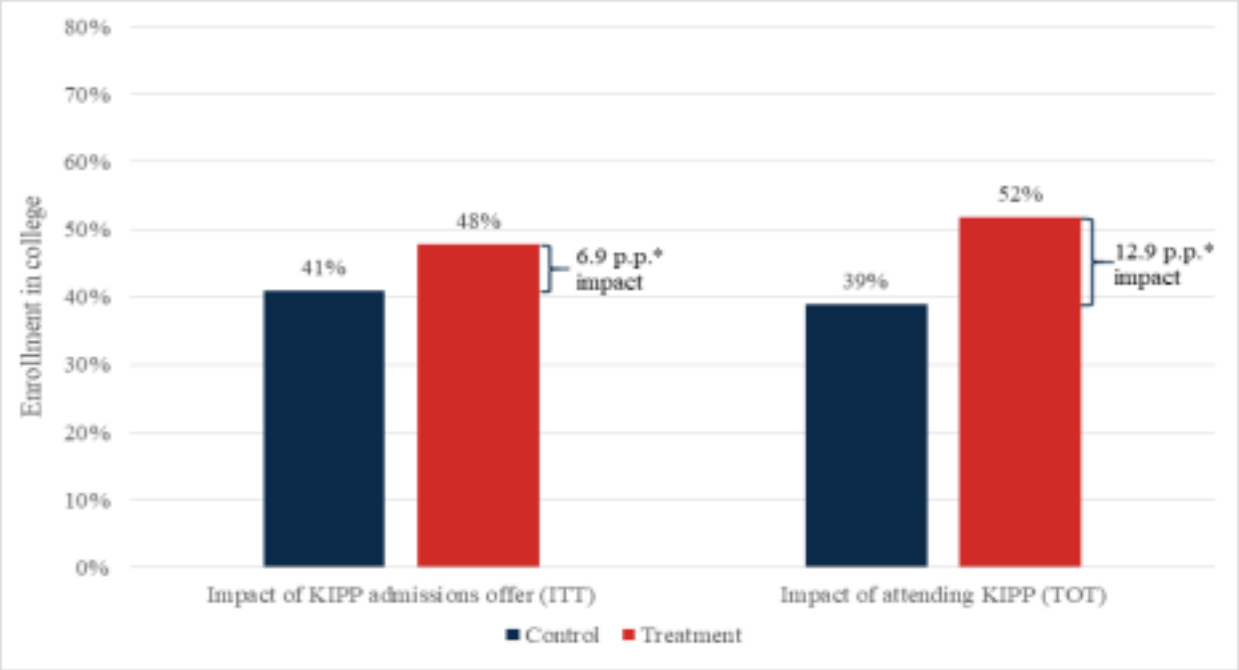
To collect information about these outcomes, we gathered data from the National Student Clearinghouse on college enrollment and estimated KIPP's impacts by comparing students in the treatment group with students in the control group.

Our primary impact estimates compare students who received an admissions offer through the lottery to students who did not receive an admissions offer at the time of the lottery. These "intention to treat" impact estimates use a conservative approach that includes students in the treatment group even if they declined to enroll in a KIPP school after receiving an admissions offer. On average, students who received an admissions offer to a KIPP middle school were 6.9 percentage points more likely to enroll in a four-year college than students who applied to KIPP but were not offered admission (*p*-value = 0.047).

In an exploratory analysis, we adjusted this impact estimate by accounting for which students actually attended a KIPP school (this is often referred to as a "treatment-on-the-treated" impact estimate). While this involves making additional assumptions as part of the analysis, the resulting impact estimate can more directly measure the potential effects of attending a KIPP

school. After adjusting for which students attended a KIPP school following the lottery, the impact estimate almost doubles in size: attending KIPP following a middle school lottery produced an increase of 12.9 percentage points (*p*-value = 0.042) in enrollment rates in four year college programs. In our sample, 51.8 percent of students who attended KIPP enrolled in a four-year college within two years after high school graduation, compared to 39.0 percent of control students (Figure ES.1). As we discuss in the report, a more conservative approach to adjusting for KIPP attendance patterns following the lottery would still produce a statistically significant impact estimate that is greater than 10 percentage points. An effect of this size represents a meaningful change in college enrollment rates. For example, the national gap in college enrollment rates in 2017 between white students and black or Hispanic students for any college type was approximately 14 percentage points among 20- and 21-year-olds (U.S. Census Bureau 2018). In other words, the impact of attending a KIPP school (10 to 13 percentage points) would be almost large enough to erase the nationwide racial disparity in college enrollment rates.

Figure ES.1. Impact of KIPP middle schools on four-year college enrollment



Note: Study includes 1,177 students who applied to enter KIPP through a middle schools via admissions lotteries, and compares the outcomes of students offered admission to KIPP (treatment group) to those not offered admission (control group) at the time of the lottery. Exploratory estimates of the impact of KIPP attendance use the lottery as an instrument for whether a student ever attended a KIPP middle or high school. The model pools all 13 lottery schools, controls for baseline covariates, and includes weights to account for probability of assignment to the treatment or control group.

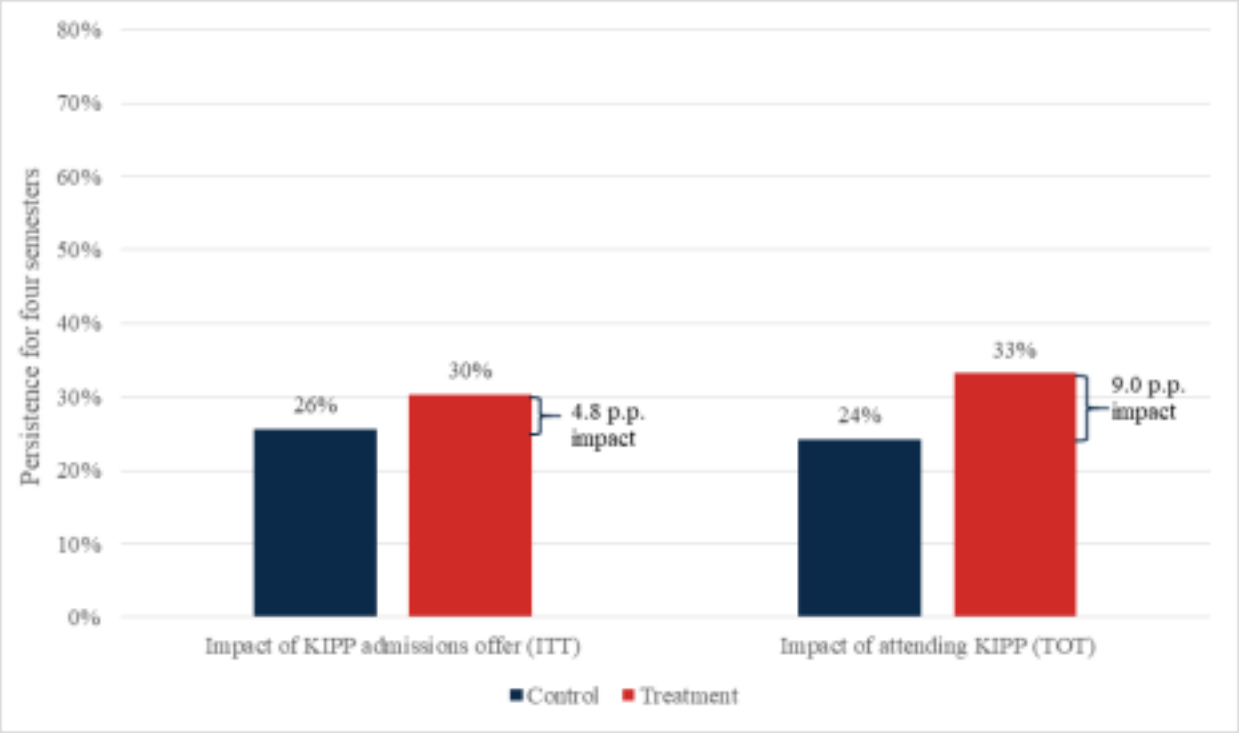
p.p. = percentage points; ITT = intent-to-treat; TOT = treatment-on-the-treated

*Impact estimate is significantly different from zero at the .05 level, two-tailed test.

**Impact estimate is significantly different from zero at the .01 level, two-tailed test.

At this point in time, for the students in this study it is only possible to observe college persistence patterns for a period of two years following high school graduation. The study's results for measures of early college persistence (Figure ES.2) are more ambiguous than the findings on initial college enrollment. In this sample, our primary impact estimates reveal that students who received an admissions offer to KIPP were 4.8 percentage points more likely to enroll immediately in a four-year college and remain enrolled for two years, but this difference was not statistically significant (*p*-value = 0.135).

Figure ES.2. Impact on persistence through four semesters of a four-year college



Note: Study includes 1,177 students who applied to enter KIPP through a middle schools via admissions lotteries, and compares the outcomes of students offered admission to KIPP (treatment group) to those not offered admission (control group) at the time of the lottery. Exploratory estimates of the impact of KIPP attendance use the lottery as an instrument for whether a student ever attended a KIPP middle or high school. The model pools all 13 lottery schools, controls for baseline covariates, and includes weights to account for probability of assignment to the treatment or control group.

p.p. = percentage points; ITT = intent-to-treat; TOT = treatment-on-the-treated

*Impact estimate is significantly different from zero at the .05 level, two-tailed test.

**Impact estimate is significantly different from zero at the .01 level, two-tailed test.

After adjusting for which students attended KIPP after the lottery in our exploratory analysis, we find that 33.2 percent of students who attended KIPP enrolled immediately in a four-year college program after high school and persisted in college for four consecutive semesters, compared to 24.2 percent of students who did not attend KIPP. While rates of entering and persisting in college were higher in the treatment group, the difference between the two groups was not large enough to be statistically significant (*p*-value = 0.129).

For the students in this study, it is too early to know whether KIPP will ultimately produce improvements in college graduation rates. Within two years of high school graduation, we have observed that the initial effects of KIPP middle schools on college enrollment rates are promising. More time will be needed to gain a clearer picture of KIPP's effects on college persistence and ultimately college graduation.

I. INTRODUCTION

The Knowledge Is Power Program (KIPP) is the nation's largest network of public charter schools, serving more than 100,000 students across a network of more than 240 schools located throughout the United States. KIPP schools largely enroll low-income students of color: approximately 88 percent of KIPP students qualify for free or reduced-price school meals and 95

percent are African American or Hispanic. KIPP's model focuses on preparing these students for success in college, with an approach that includes emphasizing high expectations, expanding the school day and year, developing students' character strengths, and empowering effective teachers and school leaders.

Prior research has shown that KIPP schools have large positive impacts on student achievement, as measured by standardized test scores (Angrist et al. 2010; Tuttle et al. 2013; Gleason et al. 2014; Tuttle et al. 2015; Knechtel et al. 2017). For example, Tuttle et al. (2013) used a random assignment design to estimate the impacts of 13 oversubscribed KIPP middle schools on student achievement. The design used the schools' admission lotteries to identify a treatment group of students who received a lottery-based admissions offer and compared their outcomes to an equivalent control group of students who did not receive a lottery-based admissions offer. For students who attended KIPP, the study found that these KIPP middle schools produced a large and statistically significant gain in math achievement of 0.36 standard deviations after two years and a gain in English Language Arts (ELA) achievement of 0.15 standard deviations, which was positive but not statistically significant. That study concluded when these students were still in middle school, but now enough time has elapsed to observe whether these students entered college and are on track to complete college degrees.

The Tuttle et al. (2013) study also paired the lottery study with a nonexperimental analysis and verified that a matched comparison group design produced estimates of KIPP's achievement impacts that were similar to the lottery-based estimates for schools where both approaches were possible. Further, the study used the matched comparison group approach to examine the impacts of a broader sample of 41 KIPP middle schools and found that KIPP's achievement impacts were positive and statistically significant in both reading and math throughout four years of middle school. These middle schools also had positive and statistically significant impacts on test scores in the subjects of science and social studies.

Other studies have also provided evidence that KIPP schools have positive impacts for elementary and high school students. A lottery-based study of KIPP elementary schools found that an admissions offer to KIPP resulted in positive and statistically significant impacts on three of four measures of students' reading and mathematics skills after three years (Tuttle et al. 2015). Additional evidence from that study suggested that KIPP prekindergarten programs positively affected student achievement above and beyond the effect of KIPP elementary schools (Knechtel et al. 2017). There is also evidence of positive effects in KIPP high schools. In a matched comparison group analysis, Tuttle et al. (2015) found that KIPP high schools had a positive and statistically significant impact on new KIPP entrants (students who did not transition from a KIPP middle school) in the subjects of math, ELA, and science (but not social studies). The study also found that KIPP high schools have positive effects on several aspects of college

preparation, including exposure to guidance counseling about college, applying to college, and taking Advanced Placement courses and exams.

While the evidence of KIPP's effectiveness in improving student test scores is widely known, less is known about the network's impacts on longer-term outcomes, such as entry into and success in college. These are important questions given that success in college is a central pillar of KIPP's mission. There are also questions about whether the success of charter school

networks like KIPP in improving student test scores can translate into success in improving longer-term college outcomes (Greene 2016). Doubts about this have been bolstered by the mixed results from other recent lottery-based studies of charter schools' long-term effects. One study found that a set of charter high schools in Boston had large positive impacts on test scores and on enrollment in four-year college programs (Angrist et al. 2016). Another study found similar positive impacts on college enrollment of a Chicago charter high school (Davis and Heller 2019). However, a national study of charter middle schools found that there was no relationship between the schools' effects on middle school test scores and the schools' effects on college enrollment (Place and Gleason 2019). In other words, the schools in that study that were successful in improving middle school test scores did not improve students' postsecondary outcomes as well.

Another reason to examine whether KIPP's success in improving middle school test scores translates to improvements in long-term outcomes is that prior lottery-based studies did not find a clear pattern of effects on students' academic attitudes that could be related to long-term academic success. As measured by student surveys, KIPP middle schools had no statistically significant effect on such outcomes as student self-control, school engagement, educational aspirations, or academic motivation (Tuttle et al. 2013, 2015). These survey results are somewhat ambiguous because it is possible that self-reported outcomes are influenced by respondents' frame of reference (the standard by which they judge themselves), and this reference point could be different for students attending a KIPP school compared to students attending a traditional public school. By measuring longer-term educational outcomes, we can also assess whether early measures of academic attitudes and perceptions are predictive of longer-term academic success.

By tracking students' progression from KIPP middle school lotteries into college, this study provides reliable evidence on the efficacy of KIPP middle schools in improving postsecondary outcomes. Results from this analysis, and potential future studies tracking this sample of students further into their college years, will enrich our understanding of the full effects of KIPP schools on student outcomes beyond their test scores and inform efforts to replicate key elements of the KIPP model in other charter schools and traditional public schools.

More specifically, this report presents the results of a long-term tracking study following students who applied to enter 1 of 13 KIPP middle schools through an admissions lottery in 2008 or 2009 and who are now old enough to have entered college. Applying a rigorous random assignment design, this study provides the first rigorous estimates of the long-term effects of KIPP middle schools on their students, including college enrollment and early patterns of persistence in college degree programs.

II. RESEARCH METHODS

In this chapter, we describe the data and methods we used to estimate the effects of KIPP middle schools on students' postsecondary outcomes. We begin by summarizing our research questions and estimation approach before defining our study sample, data sources, primary and secondary outcomes, and analytical model.

A Research questions and overview of estimation methods

This study focuses on two primary research questions:

1. What is the impact of KIPP middle schools on students' enrollment in a four-year college?

2. What is the impact of KIPP middle schools on persistence in four-year college programs for at least two years?

The study continues to follow the cohorts of students who participated in the randomized controlled trial of 13 KIPP middle schools described above (Tuttle et al. 2013). These 13 KIPP schools are listed in Table II.1. The original study examined the impacts of KIPP middle schools on outcomes observed in middle school (primarily standardized test scores) one and two years after the lottery. This follow-up study estimates the impacts of these middle schools on students' entrance into college and persistence in postsecondary programs approximately 10 to 11 years after the lottery.

Table II.1. KIPP middle schools included in analytic sample

CA Los Angeles Academy of Opportunity 2003 X CA Los Angeles Los Angeles College Prep 2003 X CA San Lorenzo Summit Academy 2003 X X DC Washington DC KEY Academy 2001 X DC Washington DC WILL Academy 2006 X GA Atlanta WAYS Academy 2003 X GA East Point South Fulton Academy 2003 X X MA Lynn Academy Lynn 2004 X X NY New York City Academy New York 1995 X TX Austin Austin College Preparatory 2002 X TX Dallas TRUTH Academy 2003 X TX Houston Academy Middle 1995 X X TX San Antonio Aspire Academy 2003 X

The lottery study design uses random assignment to form treatment and control groups, making it essentially a randomized experiment—the gold standard for estimating impacts. In a properly conducted random assignment study, the treatment group will be similar to the control group at the time of the lottery on both observable characteristics, such as prior test scores, and unobservable characteristics, such as levels of motivation and parental support.

B. Study sample and data collection

The study sample consists of 1,177 students who applied to enter grade 5 or grade 6 at an oversubscribed KIPP middle school for either the 2008–2009 school year or the 2009–2010 school year. The study includes data from a total of 19 admission lotteries at the 13 schools, with each lottery representing a distinct combination of school, cohort, and entry grade. (Appendix A includes a list of each school in the study). As of summer 2019, all students in the sample would have been old enough to have completed at least their second year of college if they followed a standard grade progression through middle and high school (Table II.2).

Table II.2. Overview of possible college semester exposure by cohort

	Number of lotteries	semesters by summer 2019	Treatment	(N) Control (N)	Percentage of sample
2008	5	3	6	76	100
15	2008	6	1	8	33
33	6	2009	5	6	4
276	230	43	2009	6	9
6	150	279	36	**Total**	19
535	642				

Note: Since we do not directly observe high school graduation in our study sample, we assume a standard grade progression to estimate a student's potential number of college semesters.

For a KIPP school to be eligible for the study, it had to (1) be oversubscribed—have more applicants than open seats—for 5th or 6th grade by its scheduled lottery date, (2) conduct a lottery to randomly select students for admissions offers and produce a randomly ordered waitlist of students not selected for admission via the lottery, (3) make subsequent offers of admission to fill additional open seats following the randomly ordered waitlist, and (4) not exhaust the randomly ordered waitlist of original lottery participants through the start of the school year. A member of the original study team personally attended each lottery to observe the lottery procedures, obtain an independently verified copy of the lottery results and waitlist, and document any stratification used.

The original study team excluded students from the sample if they were automatically admitted to the school without participating in the lottery—typically those who had a sibling already enrolled in the school. As a result, no students in the study's treatment or control group had any siblings enrolled at KIPP at the time of the lottery. Parental consent was obtained for eligible applicants to participate in the study prior to the schools' admissions lotteries, which ensured that there was no systematic relationship between the likelihood of consent for a given student and whether he or she was offered admission to the school (and thus was in the treatment group) or not offered admission (and thus was in the control group). The average consent rate among lottery participants was 75 percent and was statistically equivalent for treatment and control students (74 percent and 76 percent, respectively).

To help confirm that the lotteries resulted in treatment and control groups with similar characteristics, we tested for differences between the two groups on key baseline student characteristics. Of the 23 baseline indicators available for the sample, the treatment and control

group differ by less than 0.12 standard deviations in all cases. There were no statistically significant differences on the study's four baseline and prebaseline achievement measures, and there was a statistically significant difference on only 2 of 19 measures of the students' demographic characteristics (Table II.3). These small differences are consistent with the random amount of variation we would expect in a sample of this size. In our regression model to estimate impacts, we control for any remaining differences in baseline characteristics.

These baseline characteristics also provide a picture of how the students in the study compare to broader populations of students. Most of the sample consists of students of color from low income households. Among the treatment group, approximately 55 percent of the students are Hispanic, 38 percent are black, 83 percent are eligible for free or reduced-price lunch, 63 percent of families have incomes of less than $35,000 a year, 46 percent speak another language than English at home, and 50 percent of the mothers of students never enrolled in any type of postsecondary program. In terms of academic achievement, treatment group students had baseline test scores that were close to the average for their school district (scoring at the 51st percentile in reading and the 52nd percentile in math).

Table II.3. Baseline equivalence for the analytic sample

Baseline reading score (z-score) 0.026 –0.029 0.055 (0.080) 619

Baseline math score (z-score) 0.048 –0.019 0.067 (0.087) 553

Prebaseline reading score (z-score) 0.004 –0.106 0.110 (0.083) 556

Prebaseline math score (z-score) –0.027 –0.057 0.030 (0.089) 1,170

Student is female 0.521 0.486 0.035 (0.038) 1,074

Age relative to cohort (in years) 0.047 0.008 0.039 (0.035) 1,094

Student is Hispanic 0.551 0.557 –0.006 (0.026) 1,094

Student is white 0.028 0.032 –0.004 (0.011)

Student is black 0.376 0.333 0.043 (0.024) 1,094

Student is other ethnicity 0.045 0.078 –0.033* (0.016)

valid data 610 1,094

reduced-price lunch

0.106 0.130 –0.024 (0.026) 1,020

Student has an Individualized Education Program 0.833 0.778 0.055 (0.029)

981

Student received free or

–0.011 (0.029)

Primary language at home is English 0.536 0.516 1,067 962 0.020 (0.030)

Household has only one adult 0.266 0.236 0.030 (0.038) 920

Family income is less than $15,000 0.191 0.202

Research methods Mathematica Table II.3. (Continued)

valid data

Family income is $15,000 to less than $25,000 Family income is $35,000 to less than $55,000 0.219 0.188 0.031 (0.032)

920 920 920

Family income is $25,000 to less than $35,000 0.230 0.234 –0.004 (0.032)

0.213 0.200 0.013 (0.041)

Family income is greater than $55,000 0.147 0.175 –0.028

920

(0.036)

Mother has less than a high school education education

963 963

0.203 0.267 –0.064* (0.027)

Mother completed high school 0.299 0.240 0.059 (0.033)

Mother has some college education 0.203 0.255 –0.052

963

(0.038)

Mother has at least a college education 0.295 0.238 0.057

963

(0.039)

Note: Standard errors reported in parentheses. The difference between lottery winners and nonwinners is based on a regression of the characteristic on treatment status and site indicators for the original analytic sample of 1,179. Our analytic sample omits two of these students without a valid birthdate for whom postsecondary outcomes could not be obtained. We reran baseline equivalence results for the 1,177-student sample, and the only change is that the difference between the share of students who receive free or reduced-priced lunch became significant with a *p*-value of 0.048 (the difference increased from 0.055 to 0.056 and the standard error fell from 0.029 to 0.028). The lottery nonwinner mean is unadjusted, and the lottery winner mean is the sum of the lottery nonwinner mean and the regression-adjusted difference between groups. Missing data were not imputed: sample sizes differ by row, due to

variation in data availability by site. All of the baseline characteristics in this table are included as covariates in the study's primary impact model. *Significantly different from zero at the 0.05 level, two-tailed test.

Prior to the study, we estimated that this sample of students would provide enough statistical power to detect impacts of around 7 percentage points on the outcome of college enrollment. With this level of statistical power, the analysis may not be able to detect certain effect sizes that many would consider to be policy relevant, since we are limited by the sample size of lottery applicants from the original study. However, this sample size is sufficient to detect an effect on college enrollment that is similar in size to at least one prior study of high-performing charter high schools (Angrist et al. 2016).

C. Data sources

Our data source for measuring students' college outcomes is the National Student Clearinghouse (NSC), which provides data on college enrollment, persistence, and degree completion at colleges and universities enrolling more than 97 percent of all public and private students in the United States (Dundar and Shapiro 2016). We requested data from the NSC on postsecondary enrollment patterns through the spring 2019 semester for 1,177 of the 1,179 students in the original randomized controlled trial study for whom we have valid birthdates. The NSC provides information on whether the students in our sample match a student in their database of those who attended a postsecondary institution. Students for whom there is a match are considered to have enrolled in a postsecondary institution. Students for whom there is no NSC match are defined as not having attended any postsecondary institution. We did not use information on college

graduation as the vast majority of the sample (94 percent) would not have had enough time to complete four years of college under a normal grade progression by spring 2019.

It is possible that some students in our sample attended college but were not successfully matched in the NSC database due to data errors in names or birthdates, or colleges or students withholding enrollment data to the NSC (Dynarski et al. 2015). We used several approaches to mitigate this issue. We crosschecked multiple sources of students' birthdates in our sample, and if there was a discrepancy we submitted both birthdates to the NSC. If a student had a middle name or a hyphenated last name, we submitted permutations of student names to the NSC to ensure that our request included each student's official name in the NSC database. Finally, the NSC reported the number of student matches that were blocked by the school or student and thus not included in our analysis (and coded as not enrolling). The rates were similar in the treatment and control groups (3 percent for the control group and 2 percent for the treatment group). Appendix A provides more information on our NSC data request.

In addition the NSC database, we downloaded administrative data from the Integrated Postsecondary Education Data System, which provides information on college enrollment, graduation, financial aid, and demographics. The KIPP Foundation also provided student rosters for KIPP schools for the duration of the follow-up period, so we could ascertain the number of years that each student in the sample (including those in the treatment and control groups) attended a KIPP school. For baseline data on the sample (measuring the characteristics of students before they applied to a KIPP middle school lottery), we used data collected for the original KIPP study, including lottery application records; a baseline survey of parents, which that included demographic and socioeconomic information; and administrative records from

states, districts, or schools that provided baseline and prebaseline test score data for the study sample.

D. Outcomes

Our analysis focused on two postsecondary outcomes: college enrollment and college persistence.[1] Based on when the students in this sample were expected to graduate from high school, it is too early to observe impacts on degree completion, so the study is limited to examining early persistence patterns over the first four college semesters. We define our primary measures as:

- ***Ever enrolled in a four-year college.*** Student was enrolled in a four-year college within two years following the student's expected high school graduation date.
- ***Persisted through first four semesters in a four-year college.*** Student was enrolled in a four year college for four consecutive semesters, following the student's expected high school graduation date.

[1]Prior to collecting any data or performing data analyses, we preregistered the study's choice of primary outcomes and analytical methods through the Open Science Framework, hosted by the Center for Open Science (Nichols Barrer et al. 2019).

Because the KIPP network seeks to counsel students into selective postsecondary programs with relatively high graduation rates and mainly four-year degrees, the study's primary analyses focus on enrollment and persistence in four-year postsecondary programs (rather than two-year programs).

Since we do not directly observe high school graduation in the data collected for this study, we assume each student followed a standard grade progression from the date of their middle school admissions lottery through to the end of high school.[2] As a result, patterns of grade retention among students in the sample could introduce bias into our outcome measures if grade retention rates differ significantly between the treatment and control groups. This could happen because students who were held back a year in middle or high school would not have the opportunity to enroll full time in college in the fall after their *expected* high school graduation (since they will remain in secondary school for at least one year longer than expected).

In our sample, we can observe grade retention rates among students who attended a KIPP school, using data provided by the KIPP Foundation. Across both middle and high school, 8 percent of students who ever enrolled in a KIPP school repeated a grade while enrolled at KIPP. We cannot directly observe grade retention rates outside of KIPP, and therefore we cannot directly compare the retention rates of the treatment and control groups. However, prior studies have shown that students at KIPP middle schools have tended to repeat a grade at higher rates than students at traditional public schools (Nichols-Barrer et al. 2016; Tuttle et al. 2013; Tuttle et al. 2015). If a larger share of treatment students than control students repeated a grade in their middle or high school years, this could lead to higher rates of on-time college enrollment in the control group even if the two groups were equally likely to proceed to college following their high school

graduation. Our primary enrollment outcome measures any four-year college enrollment within two years of expected high school graduation. The two-year window makes this measure less susceptible to this issue, although we also estimated the effects of KIPP on a measure of on-time college enrollment (as a secondary outcome).

To better understand and contextualize results from the two primary measures, we estimated impacts on secondary measures of college enrollment and persistence as well. This included examining enrollment and persistence at two-year colleges and any type of college. By measuring persistence at any type of college, we capture students who began in a two-year college and transitioned to a four-year college. Other secondary outcomes included on-time college enrollment and several variations of early college persistence. Finally, we conducted an exploratory analysis to measure if KIPP schools affected the types of colleges that students select using measures of college selectivity, graduation rate, and the economic makeup of the student body. Table II.4 lists all secondary outcome measures.

[2]If the NSC data reported that a student was enrolled in college full time one year prior to their expected high school graduation, then we assumed that student graduated high school early and adjusted their expected high school graduation date so that they would count as enrolled in college when they appeared in the NSC data. If a student was only enrolled part time in college prior to their expected high school graduation, we considered that student dual-enrolled in high school and college and did not adjust their expected high school graduation date.

Table II.4. Overview of secondary outcome measures

Ever enrolled in college	Student enrolled in any type of two- or four-year college within two years following expected high school graduation.
Ever enrolled in a two-year college	Student enrolled in a two-year college within two years following expected high school graduation.
Ever enrolled in a four-year college	Student enrolled in a four-year college within two years following expected high school graduation.
Enrolled on-time in college	Student enrolled in college in the fall after their expected high school graduation date. We measured this outcome separately for students who enrolled in any college, in a four-year college, or in a two-year college.
Persisted through first four semesters in a four-year college.	Student enrolled in four-year college programs for four consecutive semesters following expected high school graduation.
Persisted through first four semesters in a two-year college.	Student enrolled in two-year college programs for four consecutive semesters following expected high school graduation.
Number of consecutive semesters enrolled.	This measure counts the number of consecutive semesters a student enrolled, beginning the fall after their expected high school graduation and continuing for four semesters. If a student did not enroll in college in the first semester following high school graduation, they received a zero for this variable. We measured this outcome separately for students who persisted in any college, persisted only in a four-year college, or persisted only in a two-year college.
Enrolled two springs after high school graduation.	
Percent of possible semesters enrolled.	

Student enrolled in college in their fourth college semester following expected high school graduation. We measured this outcome separately for students who enrolled in any college, in a four-year college, or in a two-year college.	
Share of semesters that a student was enrolled in college following expected high school graduation for all available years of data. While many cohorts have only four possible semesters of enrollment data, some cohorts are old enough to have six semesters of college enrollment data, and one cohort has enough data for eight semesters. We measured this outcome separately for students enrolled in any college, in a four-year college, or in a two-year college.	
College admissions rate	Four binary variables measuring whether a student enrolled in a college with an admissions rate of (1) 25 percent or lower, (2) greater than 25 percent and less than or equal to 50 percent, (3) greater than 50 percent or less than or equal to 75 percent, or (4) greater than 75 percent. We used the admissions rate at the college in which the student was most recently enrolled.
College graduation rate	Four binary variables measuring whether a student enrolled in a college with a graduation rate falling in different ranges (for the college in which the student most recently enrolled). We used the same cut-points as for the college admissions variables described previously.
Share of Pell Grant recipients	Two binary variables measuring whether a student enrolled in a college with a low or high percentage of students who received Pell Grants during the 2017–2018 school year (College Board 2019). One variable measured whether a student went to a college with above-average recipients of Pell Grants (school average was greater than 32 percent). The second variable measured whether a student went to a college with below-average recipients of Pell Grants (school average was less than or equal to 32 percent).

E. Analytical approach

Intent-to-treat impacts. Our primary impact estimates compare students who received an admissions offer through the lottery to students who did not receive an admissions offer at the time of the lottery. These "intention to treat" (ITT) impact estimates use a conservative approach that includes students in the treatment group even if they declined to enroll in a KIPP school after receiving an admissions offer. We estimated the impacts of KIPP on postsecondary outcomes using the regression model in equation (1), which compares outcomes of treatment and control students while adjusting for differences in their baseline characteristics:

$$y_{ik} = \alpha + \delta T_{ik} + \beta * X_{ik} + \gamma * \lambda_k + \varepsilon_{ik} \quad (1)$$

In this model, i and k index students and school lotteries, respectively, and y is the student-level outcome of interest (either binary or continuous). T is a binary treatment status variable indicating whether the student was offered admission by lottery to the KIPP school to which he or she applied, and X is a set of control variables that capture student-level characteristics. These control variables include baseline and prebaseline math and reading test scores; gender; age (standardized by grade and year to reflect whether a student is young or old for their grade); race/ethnicity; household income; mother's education; whether a household has only one adult; and if the student receives free or reduced-priced lunch, has an individual education plan, and primarily speaks English at home. Missing data in baseline control variables were imputed by

regression as part of the prior study. For any remaining missing data, we set missing cases to a value of zero. The model includes missing-indicator dummy variables for all control variables with missing data. The model also controls for the school to which the student applied, the year, and the grade they were in (sometimes referred to as lottery fixed effects). We estimate a linear probability model for any binary outcomes, and the impact model incorporates sample weights that account for the fact that some students have a higher probability of being offered admission (either based on their inclusion in a particular lottery stratum defined by a student characteristic or because they have a sibling in the lottery).

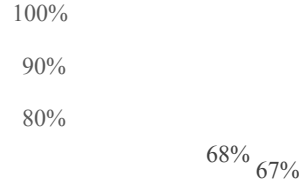
Our regression model is the same one used to estimate achievement impacts in the initial study of KIPP middle schools. We tested the model by successfully replicating the middle school achievement impacts from the earlier study using the follow-up study's updated and merged data files. We also conducted a series of sensitivity analyses to check if our results were sensitive to our model specification. We estimated impacts using models without covariates, without imputation for baseline covariates, and using a logit instead of a linear probability model for binary outcomes. We also checked results using two alternative weighting approaches: (1) inverse-variance weights, which give greater weight to the lottery sites with more precise site level impact estimates; and (2) equal site weights, which estimate the impact of the average KIPP lottery site. Our primary approach weights the KIPP lottery sites according to each site's weighted sample size, so it effectively yields an estimate of the impact of KIPP on the average lottery participant. Results from our sensitivity analyses are included as Appendix B.1.

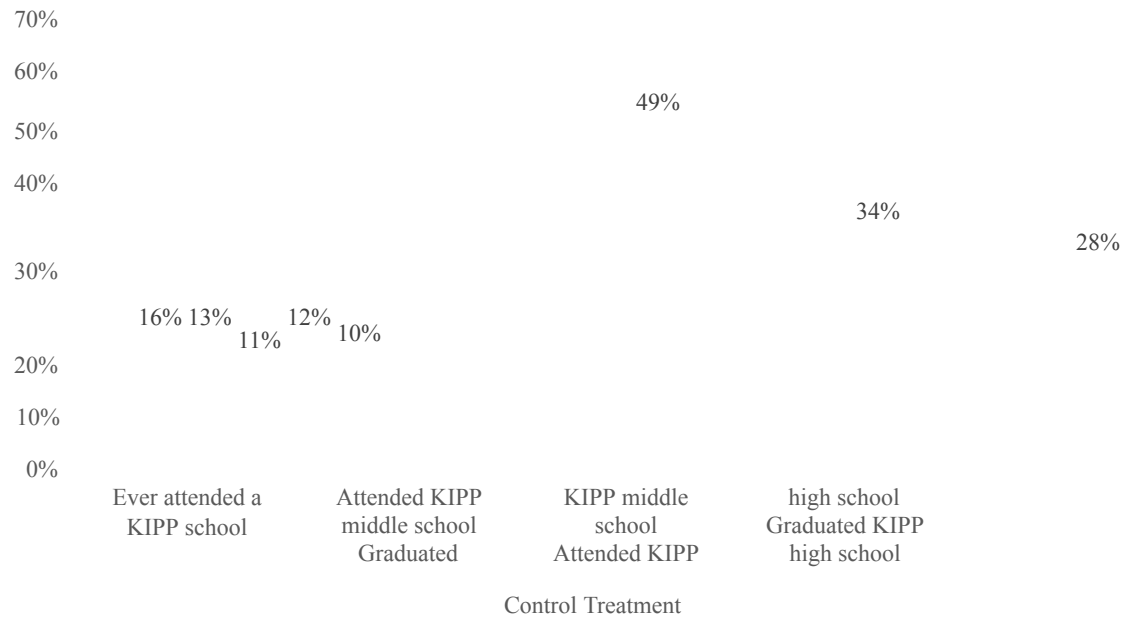
We also explored whether KIPP impacts differed for key subgroups for our primary outcome measures, including subgroups defined by a student's race, gender, socioeconomic status, and

baseline achievement level. Due to small sample sizes, the subgroup analysis is exploratory and discussed in Appendix B.2.

Treatment-on-the-treated impacts. Our primary impact estimates (the intent-to-treat impacts) measure the effect of an offer of admission to a KIPP middle school, regardless of whether treatment group students offered admission actually attended KIPP (or, conversely, whether control group students who were not offered admission attended KIPP anyway). In our sample, not all treatment group students attended either a KIPP middle school or a KIPP high school, and some control group students did attend a KIPP school. However, a significantly larger proportion of treatment students attended a KIPP school (68 percent) than control students (16 percent), as shown in Figure II.1. In terms of years of attendance, the average treatment student attended KIPP schools for 3.45 years, compared to 0.9 years for the average control student. In other words, on average students in the treatment group attended KIPP for about 2.5 years longer than students in the control group.

Figure II.1. Exposure to KIPP schools, by group





Note: We estimated sample means by regressing KIPP attendance on treatment status using inverse probability weights. The control mean is unadjusted, and the treatment mean is the sum of the control mean and the regression-adjusted difference between groups. The difference for each indicator is statistically significant (*p*-value <0.01). Sample size = 1,177.

To estimate the effect of actually attending a KIPP school, we conducted an exploratory analysis that uses the KIPP admissions lottery as an instrument for whether a student ever attended a KIPP school. This "treatment-on-the-treated" (TOT) model requires additional assumptions that are not needed to estimate ITT impacts. Specifically, to adjust for the patterns of KIPP attendance in both the treatment group and the control group, the analysis assumes that the impact of attending KIPP experienced by students in the treatment group is the same on average

as the impact of attending KIPP experienced by students in the control group. Our estimating equation follows the model used in the original KIPP study. We used two-stage least squares to first estimate the effect of winning an admissions lottery on KIPP attendance (IV equation 1), and in the second stage estimated the impact of KIPP attendance (as predicted by the lottery) on student outcomes (IV equation 2).

(IV equation 1) $\square_{\square\square} = \square\square + \square\square * \square\square + \square\square * \square\square_{\square\square\square\square} + \square\square_{\square\square} + \square\square_{\square\square\square\square}$

(IV equation 2) $\square\square_{\square\square\square\square} = \square\square + \square\square * \square\hat{}_{\square\square\square\square} + \square\square * \square\square_{\square\square\square\square} + \square\square_{\square\square} + \square\square_{\square\square\square\square}$

In IV equation 2, $\square\square$ represents the impact of attending a KIPP middle or high school at any point after the initial school lottery. To explore whether the results from this model are robust, in the following chapter of the report we also discuss an alternative TOT adjustment that accounts only for the pattern of KIPP attendance in the treatment group, ignoring the pattern of KIPP attendance in the control group. In an additional analysis, we also estimated the impact of each year of attendance at a KIPP school, based on the model in Angrist et al. (2010) that uses the

admissions lottery as an instrument for the number of years a student attended a KIPP school. In that model, ⁂ provides an estimate of the impact of attending a KIPP school for a single year. Results from that model are included in Appendix B.4.

III. KIPP'S IMPACT ON COLLEGE ENROLLMENT AND PERSISTENCE

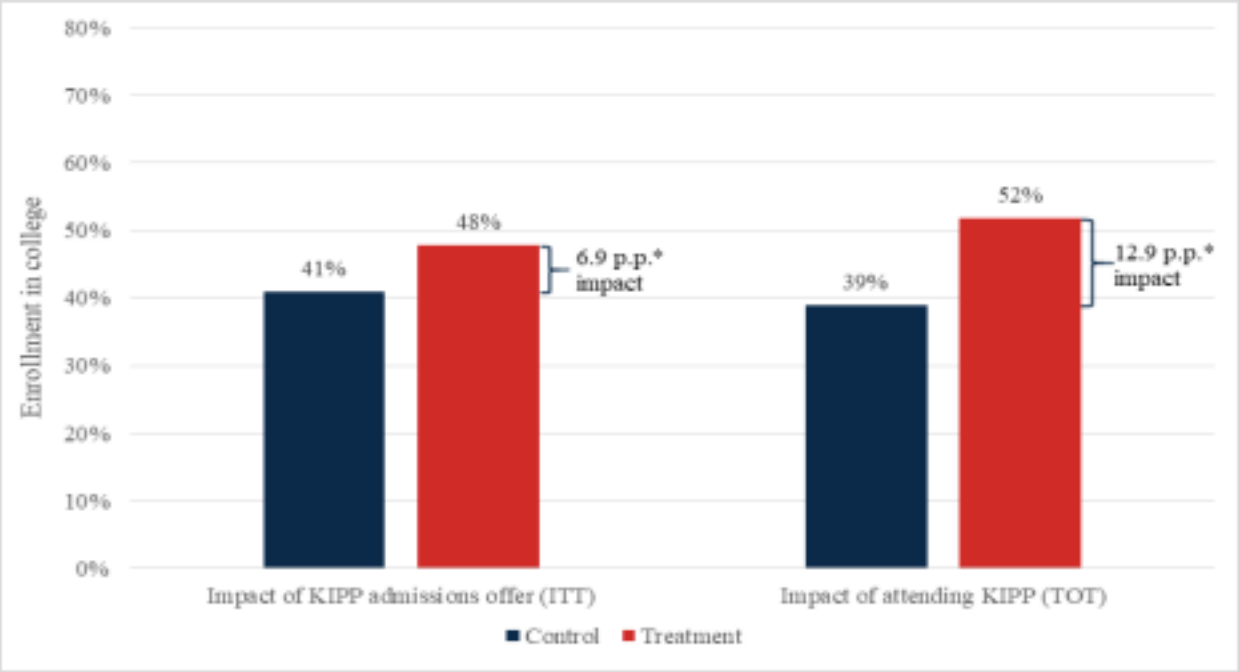
In this chapter, we begin by describing the primary results of the study: the estimated impacts of KIPP middle schools on enrollment and persistence in four-year college degree programs. We then discuss findings from our secondary measures in those domains. Finally, we describe the types of colleges that students in our sample are attending and assess whether KIPP is affecting the type of college programs students select.

A. Primary impacts of KIPP on enrollment and persistence in four-year colleges

KIPP middle schools had a positive and statistically significant impact on enrollment in four year colleges. On average, students who received a lottery-based admissions offer to a KIPP middle school were 6.9 percentage points more likely to enroll in a four-year college than students who applied to KIPP but were not offered admission (*p*-value 0.047). In particular, 47.8 percent of treatment students enrolled in a four-year college within two years of high school

graduation, compared with 41.0 percent of control students (Figure III.1).

Figure III.1. Impact of KIPP middle schools on four-year college enrollment



Note: Study includes 1,177 students who applied to enter KIPP through a middle schools via admissions lotteries, and compares the outcomes of students offered admission to KIPP (treatment group) to those not offered admission (control group) at the time of the lottery. Exploratory estimates of the impact of KIPP attendance use the lottery as an instrument for whether a student ever attended a KIPP middle or high school. The model pools all 13 lottery schools, controls for baseline covariates, and includes weights to account for probability of assignment to the treatment or control group.

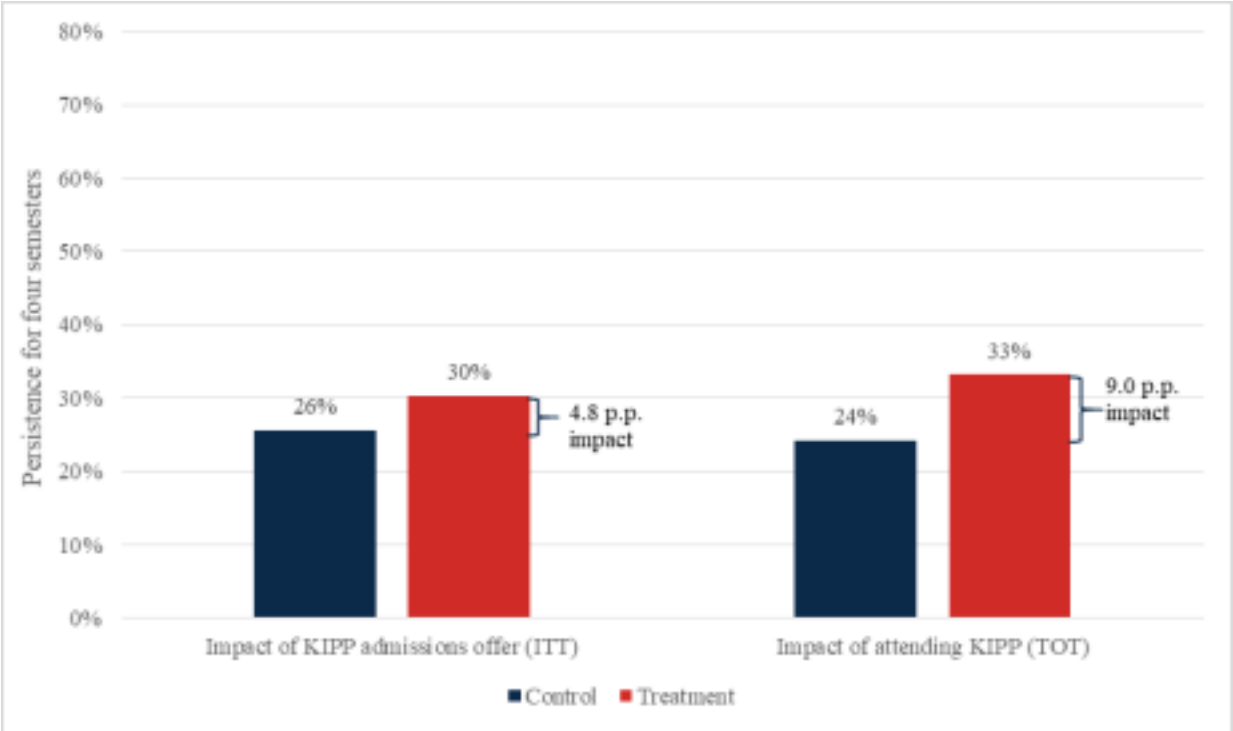
p.p. = percentage points; ITT = intent-to-treat; TOT = treatment-on-the-treated

*Impact estimate is significantly different from zero at the .05 level, two-tailed test.

**Impact estimate is significantly different from zero at the .01 level, two-tailed test.

In our exploratory analysis estimating impacts among students who ended up attending a KIPP school, we find that the impact estimate is almost twice the size of the impact estimate based on admission offers alone.[3] The impact of attending a KIPP school on enrolling in a four-year college is a statistically significant 12.9 percentage points, and represents an increase from 39.0 percent in the control group to 51.8 percent in the treatment group. An effect of this size represents a meaningful change in college enrollment rates. In 2017, the national gap in college enrollment rates between white students and black or Hispanic students for any college type was approximately 14 percentage points among 20- and 21-year-olds (U.S. Census Bureau 2018). In other words, the impact of attending a KIPP school (12.9 percentage points) would be almost large enough to erase the nationwide racial disparity in college enrollment rates.

Figure III.2. Impact on persistence through four semesters of a four-year college



Note: Study includes 1,177 students who applied to enter KIPP through a middle schools via admissions lotteries, and compares the outcomes of students offered admission to KIPP (treatment group) to those not offered admission (control group) at the time of the lottery. Exploratory estimates of the impact of KIPP attendance use the lottery as an instrument for whether a student ever attended a KIPP middle or high school. The model pools all 13 lottery schools, controls for baseline covariates, and includes weights to account for probability of assignment to the treatment or control group.

p.p. = percentage points; ITT = intent-to-treat; TOT = treatment-on-the-treated

*Impact estimate is significantly different from zero at the .05 level, two-tailed test.

**Impact estimate is significantly different from zero at the .01 level, two-tailed test.

[3] Our treatment-on-the-treated model adjusts for the rate of KIPP attendance in both the treatment group (68%) and the control group (16%). Implicitly, this model assumes that attending KIPP has the same impact on college outcomes for the students from the treatment group and control group who attended a KIPP school. An alternative, outlined in Bloom (2006), avoids this assumption by adjusting only for rates of KIPP attendance in the treatment group, and ignoring attendance patterns in the control group. Under this alternative model, the estimated impact of attending KIPP would be 10.1 percentage points for enrollment in four-year colleges (*p*-value of 0.047).

In addition to enrolling in college, we also measured whether KIPP middle schools had an impact on students entering and persisting over time in four-year college programs (Figure III.2). Here we define persistence as enrollment in a four-year college beginning in the fall after high school graduation and continuing without interruption for two years (or four semesters). We find that an offer of admission to a KIPP middle school does not have a statistically significant effect on early college persistence: 30.4 percent of students who were offered admission to a KIPP middle school ended up entering and persisting in a four-year college through their first four semesters, compared to 25.6 percent of students not offered admission to a KIPP middle school. However, the difference of 4.8 percentage points is not significantly different from zero (*p*-value 0.135).

In our exploratory analysis accounting for which students ended up attending a KIPP school,

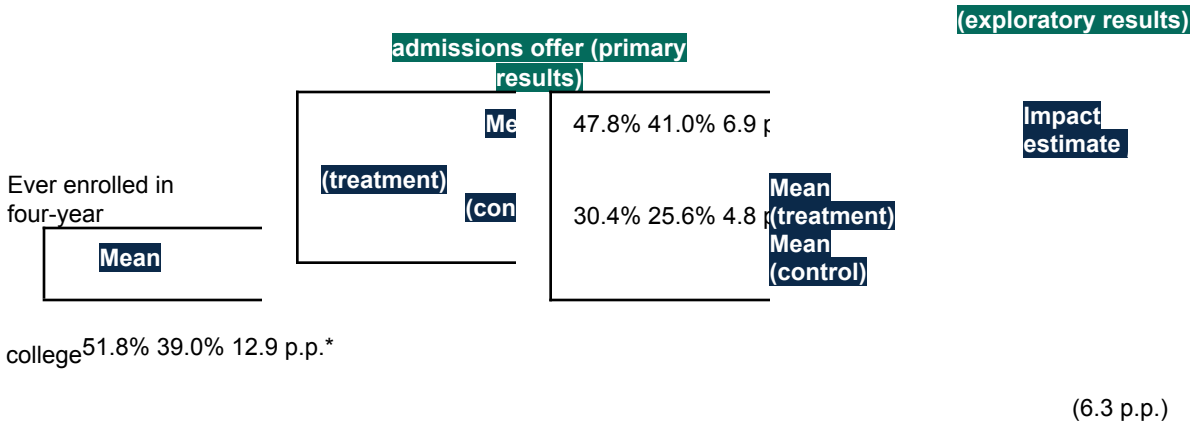
there is a large difference between the treatment and control group but it remains statistically insignificant. We estimated that 33.2 percent of the students who attended KIPP were enrolled in four consecutive semesters at a four-year college following high school graduation, compared to 24.2 percent of students who did not attend KIPP, although the 9 percentage point difference is not statistically significant (*p*-value 0.129).

The magnitude of the impact estimate for four-year college enrollment (12.9 percentage points) is larger than the impact estimate for our measure of college persistence (9.0 percentage points). This could be due to multiple factors. The initial enrollment effect could be slightly fading out over time if students in the treatment group drop out of four-year programs at a higher rate than students in the control group. However, it is also possible that the difference in magnitude is driven by treatment group students enrolling in college later or transferring from two-year programs to four-year programs at higher rates relative to the control group. Both of these patterns would prevent a student from persisting for a full four semesters in a four-year college during the two years following expected high school graduation. To fully disentangle these effects, we will need to track these students for a longer period of time and ultimately estimate impacts on college graduation.

We summarize the study's primary intent-to-treat and exploratory treatment-on-the-treated impact estimates for college enrollment and persistence in Table III.1, with standard errors for each impact estimate reported in parentheses. We examined whether our findings on four-year college enrollment and early persistence were sensitive to the specifications of our impact model. The impact estimates remained consistent when using alternative estimation models, different approaches to dealing to with missing baseline data, and alternative sample weights. Results from our sensitivity analyses can be found in Appendix B.1.

We also investigated whether the impact of KIPP middle schools differed for particular subgroups of students who may be disadvantaged, such as students with lower family income or lower baseline academic achievement. While subgroup sample sizes were small, we found no evidence that the long-term effects of KIPP middle schools differed for groups of students based on race, gender, income, mother's education, or baseline test scores. Appendix B.2 provides the complete subgroup results.

Table III.1. Primary impact estimates for college enrollment and persistence



Persisted through first four semesters	(four-year college) 33.2% 24.2% 9.0 p.p. (5.9 p.p.)	0.129

Note: Standard errors are reported in parentheses under each impact estimate. Study includes 1,177 students who applied to enter KIPP through a middle schools via admissions lotteries, and compares the outcomes of students offered admission to KIPP (treatment group) to those not offered admission (control group) at the time of the lottery. Estimates of the impact of KIPP attendance use the lottery as an instrument for whether a student ever attended a KIPP middle or high school. The model pools all 13 lottery schools, controls for baseline covariates, and includes weights to account for probability of assignment to the treatment or control group.

p.p. = percentage points

*Impact estimate is significantly different from zero at the .05 level, two-tailed test.

**Impact estimate is significantly different from zero at the .01 level, two-tailed test.

B. Impacts on secondary measures of enrollment and persistence

We now turn to examine secondary measures of college enrollment and persistence. In addition to looking at whether a student ever enrolled in a four-year college, we estimate the impact of KIPP on students enrolling in any college and in two-year colleges. We also measure impacts on measures of on-time college enrollment by school type. We do not find evidence that being offered admission to a KIPP middle school affects these other measures of college enrollment. The estimated impacts on these secondary outcomes are positive but not statistically significant (Table III.2).

For example, 65.0 percent of the treatment group and 59.560 percent of the control group enrolled in any type of college within two years of high school graduation, but the difference is not statistically significant (p-value = 0.130). In the case of on-time college enrollment, the estimated impact is again not significant at the 5 percent level (p-value 0.069), although the magnitude of the estimated impact is similar to the estimated impact on our primary enrollment outcome: 6.2 percentage points for on-time enrollment and 6.9 percentage points for any enrollment.

We also find no significant impacts of KIPP on measures of enrollment in two-year college programs (typically community college or associates degree programs). The rate of enrollment in a two-year college within two years of expected high school graduation was similar among students offered admission to a KIPP middle school (24.1 percent) and those not offered admission (22.4 percent).

Table III.2. Impact estimates on secondary college enrollment measures

		Ever enrolled	
Any college	65.0%	59.5%	5.4 p.p. (3.6 p.p.)
Four-year colleges	47.8%	41.0%	6.9 p.p.* (3.4 p.p.)
Two-year colleges	24.1%	22.4%	1.7 p.p. (3.0 p.p.)
On-time college enrollment			

Two-year colleges 14.2% 13.5% 0.7 p.p. (2.5 p.p.)
0.130 0.047 0.572

0.055 0.069 0.768

Any college 57.3% 50.4% 6.9 p.p. (3.6 p.p.)

Four-year colleges 43.1% 36.9% 6.2 p.p. (3.4 p.p.)

Note: Standard errors are reported in parentheses under each impact estimate. Study includes 1,177 students who applied to enter KIPP through a middle schools via admissions lotteries, and compares the outcomes of students offered admission to KIPP (treatment group) to those not offered admission (control group) at the time of the lottery. Impacts are based on a regression model that pools all lottery schools, controls for baseline covariates, and includes weights to account for probability of assignment to the treatment or control group.

p.p. = percentage points

*Significantly different from zero at the .05 level, two-tailed test.

**Significantly different from zero at the .01 level, two-tailed test.

Because we had a limited follow-up period in which to observe persistence patterns among the study sample, we examined persistence in multiple ways that allowed for flexibility around late entry into college or noncontinuous enrollment. For one measure (the percentage of possible semesters that a student enrolled in college), we also included more than two years of college data from older cohorts that have had a chance to persist in college for up to four years. We estimated impacts separately for two-year and four-year colleges, as well as for overall college persistence, since students may shift from two-year to four-year programs. We also estimated the average number of consecutive semesters enrolled, the share of students enrolled in the second spring semester following high school graduation, and the number of semesters enrolled out of the number of total possible semesters a student could attend college depending on their grade cohort.

Similar to the finding for our primary college persistence measure, we find a pattern of positive impacts of KIPP on these secondary persistence measures, but the effects are not statistically significant (Table III.3). For example, students offered admission to KIPP persisted continuously for one-fifth of a semester longer at four-year colleges (1.44 semesters versus 1.25 semesters in the control group), although this impact was not significantly significant (*p*-value 0.117).

Since entering and persisting in college would result in students being enrolled at the end of the two-year follow-up period, we also examined impacts on enrollment in the second spring after expected high school graduation. Again, the estimated impact of KIPP was positive but not

statistically significant—34.8 percent of the treatment group and 29.8 percent of the control group was enrolled in a four-year college in that semester (*p*-value 0.130).

While all previous measures are restricted to the first two years after high school graduation to ensure comparability across grade cohorts in our sample, our final measure—the percentage of possible semesters enrolled—takes advantage of all available years of data for each cohort. However, we find the same persistence story on this measure as well. We find no significant

differences between the treatment and the control group. For example, students offered admission to a KIPP middle school enrolled in a four-year college in 37.1 percent of possible college semesters compared with 32.7 percent among the control group (*p*-value 0.142).

Table III.3. Impact estimates on secondary college persistence measures

Outcome	Mean (treatment)	Mean (control)	Impact estimate
Enrolled in all four semesters			
Any college	38.2%	34.1%	4.1 p.p. (3.4 p.p.)
Four-year colleges	30.4%	25.6%	4.8 p.p. (3.2 p.p.)
Two-year colleges	3.3%	5.8%	–2.5 p.p. (1.3 p.p.)
Consecutive semesters enrolled			
Any college	1.87	1.70	0.17 (0.130)
Four-year colleges	1.44	1.25	0.20 (0.124)
Two-year colleges	0.33	0.39	–0.06 (0.070)
Any college	49.3%	45.9%	3.4 p.p. (3.0 p.p.)
Four-year colleges	37.1%	32.7%	4.4 p.p. (3.0 p.p.)
Any college	45.8%	43.0%	2.8 p.p. (3.6 p.p.)
Four-year colleges	34.8%	29.8%	5.0 p.p. (3.3 p.p.)
Two-year colleges	11.0%	13.1%	–2.2 p.p. (2.2 p.p.)
Percent of possible semesters enrolled			
Two-year colleges	12.2%	13.2%	–1.0 p.p. (1.7 p.p.)

Outcome	*p*-value	*q*-value	FDR cut-off
Enrolled in all four semesters			
Any college	0.237	0.135	0.058
Four-year colleges	0.179	0.117	0.394
Two-year colleges	0.437	0.130	0.319
Consecutive semesters enrolled			
Any college	0.267		
Four-year colleges	0.559		
Two-year colleges	0.142		

Note: Standard errors are reported in parentheses under each impact estimate. Study includes 1,177 students who applied to enter KIPP through a middle schools via admissions lotteries, and compares the outcomes of students offered admission to KIPP (treatment group) to those not offered admission (control group) at the time of the lottery. Impacts are based on a regression model that pools all lottery schools, controls for baseline covariates, and includes weights to account for probability of assignment to the treatment or control group.

p.p. = percentage points

*Significantly different from zero at the .05 level, two-tailed test.

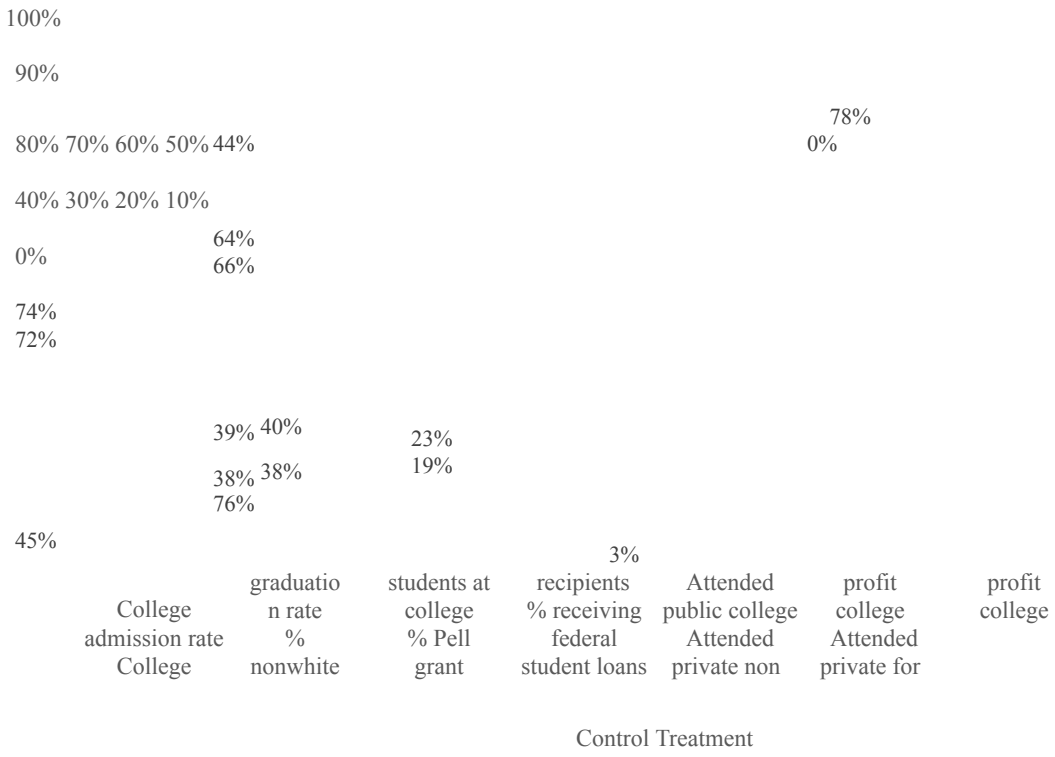
**Significantly different from zero at the .01 level, two-tailed test.

We also estimated the impact of attending a KIPP middle school (treatment-on-the-treated impacts) for each of the study's secondary outcome measures related to college enrollment and initial college persistence. Those results can be found in Appendix B.3.

C. Impacts on the types of colleges that students are attending

Among students in the study sample who went to college, we did not find significant differences between the treatment and control groups in the attributes of the schools they selected (Figure III.3).

Figure III.3. Types of colleges attended for the treatment and control groups



Note: Sample size contains students who ever attended a postsecondary institution. All measures have a sample size of 741 except college graduation rate, where the rate was missing from colleges attended by four students. No differences between treatment and control group are statistically significantly at the 5 percent level.

For treatment students who enrolled in college, the average school had an admissions rate of 72 percent and a graduation rate of 45 percent, with similar rates among students in the control group (74 percent and 44 percent respectively). Sixty-six percent of students at schools attended by the treatment group were nonwhite and 39 percent received Pell Grants—almost identical to the same rates in the control group. The two groups were also comparable on the share of students at their colleges who received any federal student loans. Finally, treatment and control group students who enrolled in college chose a similar mix of public, private, and for-profit colleges. Among the treatment group, 76 percent enrolled in a public institution, 23 percent in a private nonprofit college, and less than 1 percent in a private for-profit colleges.

To more directly examine the impact of KIPP on the types of colleges students selected, we defined a set of binary outcomes for these attributes. We find no evidence that KIPP affected the type of colleges attended by students in the sample, with respect to college admissions or graduation rates (Table III.4). Admission to a KIPP middle school did not result in students being more likely to enroll in highly selective colleges or colleges with higher graduation rates. However, there is suggestive evidence that KIPP led students to be more likely to go to a college where the percentage of Pell Grant recipients exceeds the national average (*p*-value 0.051).

Table III.4. Secondary impact estimates on college type

Went to college with admin rate <=25%	2.8%	1.7%	1.1 p.p. (1.0 p.p.)	0.250
Went to college with admin rate >25% and <=50%	10.3%	9.4%	0.9 p.p. (1.8 p.p.)	0.641
Went to college with admin rate >50% and <=75%	24.4%	19.3%	5.2 p.p. (3.1 p.p.)	0.093
Went to college with admin rate >75%	29.0%	30.7%	−1.7 p.p. (3.6 p.p.)	0.646
Went to college with grad rate <=25%	18.3%	17.1%	1.2 p.p. (2.6 p.p.)	0.636
Went to college with grad rate >25% and <=75%	21.4%	18.8%	2.7 p.p. (3.0 p.p.)	0.370
Went to college with grad rate >50% and <=75%	17.4%	14.8%	2.5 p.p. (2.5 p.p.)	0.313
Went to college with grad rate >75%	9.2%	10.3%	−1.1 p.p. (2.4 p.p.)	0.651
Went to college with above average rate of recipients	44.2%	37.2%	7.0 p.p. (3.6 p.p.)	0.051
Went to college with below average rate of recipients	22.4%	23.9%	−1.6 p.p. (3.2 p.p.)	0.634

Note: Standard errors are reported in parentheses under each impact estimate. Study includes 1,177 students who applied to enter KIPP through a middle schools via admissions lotteries, and compares the outcomes of students offered admission to KIPP (treatment group) to those not offered admission (control group) at the time of the lottery. Impacts are based on a regression model that pools all lottery schools, controls for baseline covariates, and includes weights to account for probability of assignment to the treatment or control group.

p.p. = percentage points

*Significantly different from zero at the .05 level, two-tailed test.

**Significantly different from zero at the .01 level, two-tailed test.

IV. DISCUSSION

This reports presents the first experimental findings on the long-term impacts of KIPP middle schools, focusing on enrollment and early persistence in college. We find that KIPP had a positive and statistically significant impact on enrollment in four-year colleges. Our primary analysis revealed that receiving an admissions offer to a KIPP middle school increased a student's likelihood of enrolling in a four-year college by 6.9 percentage points. In an exploratory analysis that adjusted for which students actually enrolled at a KIPP middle or high school after receiving an admissions offer, we find that attending a KIPP school had an impact of 12.9 percentage points on the four-year college enrollment rate.

The magnitude of this impact falls within the range of estimates from other research on the long term effects of charter schools. Place and Gleason (2019) find only a small (and not statistically significant) impact of 3 percentage points on four-year college enrollment for students admitted to a nationwide sample of 30 oversubscribed charter middle schools. For one high-performing charter school in New York City, by contrast, admission offers led to a statistically significant increase of 9 percentage points in four-year college enrollment, and attendance at the school led to a 16 percentage point increase in college enrollment (Dobbie and Fryer 2015). Studies of charter high schools, where students are much closer to the point of college entry, also show positive impacts on four-year college enrollment (Angrist et al. 2016; Davis and Heller 2019). Angrist et al. (2016) found that students who attended a set of six charter high schools in Boston were 18 percentage points more likely to enroll in a four-year college. Unlike other studies of the long-term effects of charter schools, that study also found that Boston charter high schools led to a substantial (11 percentage point) decrease in the two-year college enrollment rate. In other words, attending one of those schools led a substantial proportion of students to shift from two year to four-year colleges. We find no such pattern among KIPP middle schools.

Our findings are less clear with respect to patterns of persistence in four-year college programs. During the first two years after high school graduation, we do not find clear evidence that KIPP has an effect on college persistence. Specifically, students admitted to a KIPP middle school were 4.8 percentage points more likely to enter and persist in a four-year college for four consecutive semesters. However, this difference in persistence rates was not statistically significant. Moreover, the difference we did observe was driven by treatment group students being more likely to enroll in four-year colleges, rather than any difference in persistence among the subset of students who entered a four-year school.

Despite the absence of statistically significant impacts on our measures of early college persistence, it is worth considering the policy relevance of the magnitudes of the estimated impacts from our exploratory analysis of the effects of attending a KIPP school on both enrollment (12.9 percentage points) and persistence through the first four semesters after high school graduation (9.0 percentage points). Imagine, for example, that the hypothetical impact of attending a KIPP school on college graduation was the same as our estimate of the impact on persistence—9 percentage points. An impact of this size would be substantial in relation to differences between groups in college graduation rates. Nationally among 25- to 29 year-olds, 44

percent of white Americans have a bachelor's degree compared to only 23 percent of black Americans and 21 percent of Hispanic Americans—a degree completion gap of 21 and 23 percentage points respectively (National Center for Education Statistics 2019). If a future study revealed that KIPP middle schools ultimately do have an effect of approximately 9 percentage points on college completion, that effect would be equal to more than a third of the degree completion gap for black and Hispanic students.

Given the size of these potential effects, it will be important to examine the next chapter in the lives of these students and assess how many of them ultimately complete college degrees. In addition, it would also be valuable to include a larger sample of KIPP schools and students in future studies to obtain a more precise set of estimates of KIPP's effects on college outcomes. For the students in this study, it is too early to say whether the strong effect of KIPP middle schools on college enrollment will ultimately improve rates of college graduation or lead to long term improvements in employment and earnings after college. We look forward to future tracking studies that could reveal what happens next.

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Appendix A. Additional information on study

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The study sample consists of 1,177 students who applied to enter grade 5 or grade 6 at an oversubscribed KIPP middle school for either the 2008–2009 or 2009–2010 school year. The study includes data from 13 KIPP middle schools holding a total of 19 admission lotteries, with each lottery representing a distinct combination of school, cohort, and entry grade. Sample sizes differ considerably by school as some schools had more eligible seats for the lottery and/or had more applicants to those seats. Because of this, we present aggregated impact estimates for the overall sample rather than estimating school-level impacts. Table A.1 provides treatment and control group sample sizes for each KIPP middle school that students applied to in our study sample.

Table A.1. Sample sizes by KIPP middle schools

CA Los Angeles Academy of Opportunity 17 66 CA Los Angeles Los Angeles College Prep 13 96 CA San Lorenzo Summit Academy 86 39 DC Washington DC KEY Academy 16 17 DC Washington DC WILL Academy 6 11 GA Atlanta WAYS Academy 17 4 GA East Point South Fulton Academy 172 74 MA Lynn Academy Lynn 63 102 NY New York City Academy New York 70 15 TX Austin Austin College Preparatory 11 45 TX Dallas TRUTH Academy 9 8 TX Houston Academy Middle 46 153 TX San Antonio Aspire Academy 9 12 **Total** 535 642

Table A.2 provides additional information on our data request to the National Student Clearinghouse (NSC). In particular, we provide the postsecondary data opt-out rates reported by the NSC for the treatment and control groups. Students and institutions have the option of refusing to share records through the NSC database; these cases consist of students who were matched to a college according to the NSC but either they or their school opted out of allowing their data to be shared. In our data set, we cannot differentiate between a student who blocked their enrollment data from being shared and a student who did not go to college. However, we can identify if opt-out rates are biasing our impact estimates by examining the overall opt-out rates separately by the treatment and control group. We find that the rates are similar and thus should not be biasing our impact estimates: 1.9 percent of the treatment group and 3.1 percent of the control group opted-out of sharing their college enrollment data.

We also examined match rates separately by treatment and control group for the cases when we included a single record request for a student, as compared to cases when we included multiple record requests for the same student. We used the same process for the treatment and control groups to determine which students warranted submitting multiple records. Specifically, when a student had a hyphenated name or a middle name, we submitted a batch of multiple records for

that student with the different name permutations in the NSC submission file. There were also a few students in both the treatment and control groups that listed multiple birthdates in our sample file. In those cases, we submitted records with each of the recorded birthdates in our data set. If the NSC provided any matched postsecondary records for a student with multiple submissions,

we classified the student as having enrolled in a postsecondary program.

We find that the match rates were similar for the students with a single record submission and for students with multiple record submissions. Overall, for any college type and all years of available data, 66.6 percent of students with single record submissions matched to the NSC, compared to 67.2 percent of students with multiple submitted records (this overall match rate is considerably higher than the enrollment rate in four-year college programs cited in the study's primary findings, because a considerable portion of the matched students only enrolled in two year college programs and because some of the students in older cohorts enrolled in college after the study's two-year follow-up period). The match rate is also similar between single and multiple record submissions when examining the results separately by treatment status (the differences are less than 1 percentage point). In other words, submitting multiple or single records for students did not appreciably affect the match rates used to define the study's outcomes in the treatment and control group.

Table A.2. Summary of postsecondary data requested and returned

Percentage of students who opted out of sharing data with the NSC	2.5%	1.9%	3.1%			
Match rate from submitting single records	66.6%	70.5%	63.7%	Number of single record requests	988	423
565						
Match rate from submitting multiple records	67.2%	69.6%	64.4%	Number of multiple record requests		
127	78	49				
Overall sample size	1,177	535	642			

Note: The NSC reports a count of the number of students in the data request file who were found in NSC records but had opted out of sharing their data. The means presented in the first row of this table represent the raw proportion of the treatment or control group students in the data request who opted out and are not adjusted for selection probability or site. The match rates reported cover any college type and all years of available data. They are also raw means that are not adjusted for selection probability or site.

Appendix B. Supplementary analyses

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Appendix B Mathematica

1. Sensitivity analyses

To test our modeling decisions on model form, covariate selection, baseline imputation, and

sample weights, we conducted a series of sensitivity analyses.

Logit model. To estimate our primary impacts, we used an ordinary least squares (OLS) regression model. When estimating binary outcomes (that is, the outcome has two possibilities, such as enrolled in college or not enrolled in college), an OLS regression model is also known as a linear probability model (LPM). Different model forms can plausibly produce different results, so we reestimated impacts on our primary outcomes using a logit model to test how sensitive our findings are to the model type. We used the same covariates as our primary impacts model. To compare findings between logit and LPM results, we converted our logit results to treatment and control means and report the marginal impact.

We find that the logit impact estimates are almost identical to the LPM impact estimates (Table B.1, which also includes the estimates from our primary impact model for ease of comparison). For our logit results, we find a positive and statistically significant impact on four-year college enrollment of 6.8 percentage points (compared to 6.9 percentage points for the LPM result) and a positive but not significant difference of 5.0 percentage points for persisting through a student's first four college semesters (compared to 4.8 percentage points for the LPM result).

Baseline imputation. Our primary impact model uses baseline data on test scores, socioeconomic, and demographic characteristics as covariates. For students missing data for some of the covariates, we imputed values by conducting single stochastic regression imputation. Imputation was conducted separately by treatment status and each imputation included a stochastic component randomly selected from the set of all residuals in the imputation equations to ensure that the variance of the imputed values are the same as that of the observed values (see Tuttle et al. 2013 for more information on the imputation process).

To test whether our imputation method may be driving our findings, we estimated results using two alternative models. First, we replaced all imputed baseline values with 0 (zero imputation model) but kept the model identical to our primary approach otherwise (including using imputation flags as covariates). Second, we estimated impacts without including any baseline covariates (no covariates model). These models still controlled for the school to which the student applied, the year, and the grade they were in.

We find that the magnitude and directionality of the impacts are similar between our primary model and our alternative imputation models. There were some modest shifts in the coefficient estimate and/or the precision of the estimates that led to the estimated impacts on college enrollment to be not statistically significant without including a full set of control variables or a more sophisticated imputation approach. With the zero imputation model, the point estimates drop by about 1 percentage point compared to our primary model. For the no covariates model, we see a similar drop for our enrollment measure but no change in the magnitude of the impact for the persistence measure.

Sample weights. Our primary impact model includes sample weights, known as inverse probability weights, to account for the fact that not all students in the lottery have the same probability of being offered admission to the KIPP school (that is, being selected into the treatment group). Some students have a higher probability of being offered admission, either

based on their inclusion in a particular stratum defined by a student characteristic or because they have a sibling in the lottery. The primary sample weights follow the original RCT study (Tuttle et al. 2013) and are based on the procedure used in Gleason et al. (2010).

In the simple case, where all students interested in attending a particular KIPP school enter the lottery and no preferences are given for siblings or other characteristics, the sample weight for a given student is based upon the probability that he or she ended up in the treatment or control group. This probability is used in the calculation of each student's base weight. In particular, the base weight assigned to treatment (or control) group members is set to the inverse of the probability of being selected into the treatment (or control) group. We then normalize this weight to account for the fact that the sample will be representative of the set of all consenting lottery participants at that site. We set this normalization factor such that the weights of each experimental group sum to one-half of the total sample size within the site. Thus, the sum of all students' weights within a site will be equal to the overall sample size in that site (that is, the number of consenting lottery participants), with the sum of weights among treatments equal to that among controls.

In sites with sibling preference rules (that is, siblings of students already enrolled in a KIPP school have a higher likelihood of winning the lottery), the basic approach to calculating sample weights is the same as in the simple case above. The difference, however, is in the calculation of the probability of admission. No longer can we simply use the number of students offered admission divided by the number of lottery participants. The exact probabilities of admission depend on the number of sets of siblings who participate in the lottery at the school as well as the number of students within each sibling set.

To test whether our impact results are being driven by our choice of sample weights, we reestimated our primary impacts using two alternative weighting approaches that alter the relative weight of sites in the estimation—the treatment and control groups continue to contribute equally within site, but these normalizations affect the relative weight of different sites in the analysis. First, we weight school impacts equally (known as equal site weights). Second, we weight schools by the treatment group sample size, a version of inverse-variance weights that provides greater weight to the lottery sites with more precise site-level impact estimates. Within each site, the alternative weights still account for differences between applicants in their probability of being offered admission (based on factors such as whether they have siblings applying to the school). Besides the change in weighting approach, the model is identical to our primary impacts model.

We find that the alternative weighting approaches slightly increase the magnitude of the impact estimates and result in a significant finding for the primary persistence measure when weighting schools equally. The difference between the main sample weights and the alternative ones is how sites are weighted relative to one another. Thus, the change in estimated impacts implies that

sites with larger impacts are being weighted more heavily when using the alternative normalizations (either weighting by school or by treatment group sample size).

Table B.1. Impact estimates on primary outcome using alternative model specifications

(four-year college) (3.5 p.p.)

Ever enrolled in four-year college Zero imputation 29.9% 26.0% 4.0 p.p. 0.043

Persisted through first four

(treatment) Logit 47.8% 41.0% 6.8 p.p.* (3.4 p.p.) 0.109 0.085

(control) Logit 30.4% 25.4% 5.0 p.p. (3.1 p.p.)

Ever enrolled in four-year college Zero imputation 47.4% 41.4% 6.0 p.p.

Persisted through first four semesters No covariates 47.3% 41.5% 5.9 p.p. (3.8 p.p.) 0.121

semesters (four-year college)

Ever enrolled in four-year college No covariates 30.3% 25.6% 4.8 p.p. (3.5 p.p.) 0.173

Persisted through first four semesters (four-year college) Weight schools (3.3 p.p.) 0.233

Ever enrolled in four-year college p.p.)

equally46.4% 38.7% 7.7 p.p.* (3.9 0.050

Persisted through first four semesters (four-year college) Persisted through first four semesters (four-year college) Weight schools by treatment group sample size 33.3% 27.2% 6.0 p.p. (3.5 p.p.)

Weight schools equally 29.9% 22.9% 7.0 p.p.* (3.5 p.p.) 0.044 0.025

Ever enrolled in four-year college Weight schools by treatment group sample size 53.0% 44.6% 8.4 p.p.* (3.7 p.p.) 0.087

model47.8% 41.0% 6.9 p.p.* (3.4 p.p.) 0.047

Primary impacts

Ever enrolled in four-year college semesters (four-year college) Primary impacts model p.p.)

Persisted through first four 30.4% 25.6% 4.8 p.p. (3.2 0.135

Note: Standard errors are reported in parentheses under each impact estimate. The study includes 1,177 students who applied to enter KIPP through a middle schools via admissions lotteries, and compares the outcomes of students offered admission to KIPP (treatment group) to those not offered admission (control group) at the time of the lottery.

p.p. = percentage points

*Significantly different from zero at the .05 level, two-tailed test.

**Significantly different from zero at the .01 level, two-tailed test.

2. Subgroup analyses

We also explored whether KIPP impacts differed for key subgroups for our primary outcome measures. We examined subgroups defined for students with low baseline math or reading scores (below district mean); low household income (less than $35,000 a year); low mother's education (completed high school or less); as well as for students that are male, mainly speak another

language than English at home, Hispanic, black, or eligible for free or reduced-price lunch. Given our sample size, the subgroup sizes are small and results should be interpreted with caution. Our overall sample size is only powered to detect college enrollment impacts of around 7 percentage points. Most of the subgroups are less than half the size of our overall sample and

so the impacts would need to be much larger for us to be able to reliably detect these effects as being statistically different from zero.

To estimate subgroup impacts, we use the same model as our primary specification (equation 1 in Section II.E) but add in a term that represents the interaction between a subgroup indicator and the treatment variable. The coefficient on the interaction term represents how the estimated effect of KIPP on a given outcome among that subgroup differs from the effect among students who are not in the subgroup. A positive estimate for the interaction effect indicates that KIPP has a more positive effect on college enrollment or persistence among the subgroup than among other students. Similarly, a negative estimate indicates that KIPP has a more negative effect among the subgroup than among other students. Estimates that are indistinguishable from 0 imply that KIPP's effect is no different for students in the subgroup than for those not in the subgroup. As with our primary model, we control for baseline student achievement, demographics, socioeconomic factors, the school to which the student applied, the year, and the grade they were in. We also use inverse probability weights to account for students having different probabilities of being offered admission to a KIPP middle school.

Table B.2 presents our subgroup findings. For all subgroups on both primary outcomes, we do not find any significant differences between KIPP's effect on the two primary outcomes among the subgroup and KIPP's effect among other types of students. In other words, there is no evidence that KIPP's overall positive impact on enrollment in four-year colleges is different for any of the subgroups we tested. The magnitudes and directionality of the subgroup interaction effects vary widely, but this appears to be a byproduct of "noise" in the data related to the small sample sizes in these subgroup analyses. These results imply that the effect of KIPP middle schools does not appear to be concentrated among particular subgroups of students.

Table B.2. Impact estimates on primary outcome using alternative model specifications

	effect	subgroup size
Ever enrolled in four-year college Low baseline math scores –13.3 p.p. (8.0 p.p.)		0.098 108
Persisted through first four semesters (four-year college) Low baseline math scores –13.2 p.p. (8.0 p.p.)		0.099 108
Ever enrolled in four-year college Low baseline reading scores –6.5 p.p. (7.9 p.p.)		0.410 108
Persisted through first four semesters (four-year college) Low baseline reading scores –9.5 p.p. (7.4 p.p.)		0.199 108
Ever enrolled in four-year college Low household income 12.1 p.p. (7.9 p.p.)		0.125 232

Appendix B Mathematica Table B.2. (Continued)

	effect	subgroup size
Persisted through first four semesters (four-year college) Low household income 3.7 p.p. (6.7 p.p.)		0.584 232

			education 10.2 p.p. (7.3 p.p.)	0.163 154
Ever enrolled in four-year college Low mother's				
		semesters (four-year college)		p.p.)
		Low mother's education 9.2 p.p. (6.5		0.155 154
Persisted through first four				
			p.p.)	0.361 261
Ever enrolled in four-year college Male 6.0 p.p. (6.5				
		semesters (four-year college)		0.184 261
		Male 7.8 p.p. (5.9 p.p.)		
Persisted through first four				
			home not English 13.4 p.p. (7.1 p.p.)	0.061 151
Ever enrolled in four-year college Main language at				
		semesters (four-year college)		4.0 p.p. (6.5 p.p.)
		Main language at home not English		0.541 151
Persisted through first four				
			p.p.)	0.550 185
Ever enrolled in four-year college Hispanic 4.1 p.p. (6.9				
		semesters (four-year college)		0.907 185
		Hispanic –0.8 p.p. (6.5 p.p.)		
Persisted through first four				
			p.p.)	0.139 269
Ever enrolled in four-year college Black –10.3 p.p. (7.0				
		semesters (four-year college)		0.293 269
		Black –7.0 p.p. (6.7 p.p.)		
Persisted through first four				
			–2.6 p.p. (8.1 p.p.)	0.743 321
Ever enrolled in four-year college Eligible for FRPL				
		semesters (four-year college)		0.961 321
		Eligible for FRPL –0.3 p.p. (7.0 p.p.)		
Persisted through first four				
			p.p. (7.0 p.p.)	0.354 127
Ever enrolled in four-year college Black males –6.5				
		semesters (four-year college)		0.363 127
		Black males –5.8 p.p. (6.4 p.p.)		
Persisted through first four				
			p.p. (7.5 p.p.)	0.262 96
Ever enrolled in four-year college Hispanic males 8.4				
		semesters (four-year college)		0.190 96
		Hispanic males 8.8 p.p. (6.7 p.p.)		
Persisted through first four				

Note: We used nonimputed baseline data to identify each subgroup. Standard errors are reported in parentheses. The study compares the outcomes of students offered admission to KIPP (treatment group) to those not offered admission (control group) at the time of the lottery. Impacts are based on a regression model that pools all lottery schools, controls for baseline covariates, and includes weights to account for probability of assignment to the treatment or control group. Low math and reading scores = below average test scores (z scores<0); FRPL = free or reduced-price lunch; Low income = less than $35,000 in household income a year; Low mother's education = high school degree or lower; p.p. = percentage points *Significantly different from zero at the .05 level, two-tailed test.

**Significantly different from zero at the .01 level, two-tailed test.

3. Impacts of attending a KIPP school for secondary outcome measures

Table B.3 presents the exploratory treatment-on-the-treated (TOT) impact estimates for our secondary outcome measures (for completeness we also include our two primary outcome measures in the table as well). The TOT impacts use the KIPP admission lottery as an instrument

for whether a student ever attended a KIPP school. Thus, the effect estimates represent the impact of *attending* a KIPP middle or high school, rather than the intent-to-treat (ITT) impacts that represent the impact of an *admissions offer* to a KIPP middle school. Since only 68 percent of treatment group students attended a KIPP school, while 16 percent of control students ended up attending a KIPP school, the TOT impacts increase the magnitude of the effect size compared to the ITT impacts.

For secondary enrollment measures, we estimated the impact of on-time college enrollment by school type as well as whether a student ever enrolled in a two-year college or any type of college. We also examined persistence in multiple ways that allowed for flexibility around late entry into college (or noncontinuous enrollment). We estimated the average number of consecutive semesters enrolled, the share of students enrolled in the second spring semester following high school graduation, and the number of semesters enrolled out of the number of total possible semesters a student could attend college depending on their grade cohort.

While an admissions offer to a KIPP middle school (ITT model) did not have a statistically significant impact on any of our secondary enrollment and persistence measures, we did find that two of the outcomes became significant when measuring the impact of *attending* a KIPP school (TOT model) resulting from small changes in *p*-values. Under the TOT model, attending a KIPP school increased the likelihood of on-time college enrollment in any type of college by a statistically significant 13 percentage points. KIPP has a similar effect on students enrolling in colleges where the student body has an above average share of Pell Grant recipients. No other secondary outcome measure is statistically significant at the 5 percent level.

Table B.3. Impact estimates on secondary outcome measures (treatment-on-the-treated)

	(treatment)	attending KIPP (control)	
Four-year colleges	46.7%	35.1%	11.7 p.p. (6.3 p.p.)
Two-year colleges	14.6%	13.3%	1.4 p.p. (4.6 p.p.)
Any college	68.2%	57.9%	10.2 p.p. (6.6 p.p.)
Four-year colleges[a]	51.8%	39.0%	12.9 p.p.* (6.3 p.p.)
Two-year colleges	25.1%	21.9%	3.2 p.p. (5.5 p.p.)
Any college	61.4%	48.3%	13.0 p.p.* (6.6 p.p.)

Appendix B Mathematica Table B.3. (Continued)

Enrolled in all four semesters	(treatment)	attending KIPP (control)	
Two-year colleges	1.9%	6.5%	–4.6 p.p. (2.4 p.p.)
Any college	40.6%	33.0%	7.6 p.p. (6.3 p.p.)
Four-year colleges[a]	33.2%	24.2%	9.0 p.p. (5.9 p.p.)

0.119 0.042 0.563

0.049 0.063 0.762

Any college 1.98 1.65 0.33 (0.239)
Four-year colleges 1.56 1.19 0.37 (0.229)
Two-year colleges 0.30 0.41 –0.11 (0.129)

Any college 47.4% 42.1% 5.3 p.p. (6.6 p.p.)
Four-year colleges 37.7% 28.4% 9.3 p.p. (6.0 p.p.)
Two-year colleges 9.7% 13.7% –4.1 p.p. (4.0 p.p.)

Went to college with admin rate <=25% 3.4% 1.4% 2.1 p.p. (1.8 p.p.)
0.228 0.129 0.055

0.169 0.110 0.385

0.426 0.123 0.311

0.254 0.134 0.551

0.238

Any college 51.3% 44.9% 6.4 p.p. (5.6 p.p.)
Four-year colleges 39.7% 31.4% 8.2 p.p. (5.5 p.p.)
Two-year colleges 11.6% 13.5% –1.9 p.p. (3.2 p.p.)

	>50% and <=75%		0.632 0.087
Went to college with admin rate	10.8% 9.2% 1.6 p.p. (3.3 p.p.)		
>25% and <=50%	27.5% 17.8% 9.7 p.p. (5.7 p.p.)		
Went to college with admin rate			
Went to college with admin rate >75% 28.0% 31.2% –3.2 p.p. (6.7 p.p.)		Went to college with grad rate >50% and <=75% 18.8% 14.1% 4.7 p.p. (4.6 p.p.)	
		Went to college with grad rate >75% 8.6% 10.6% –2.0 p.p. (4.3 p.p.)	
		0.639	
Went to college with grad rate <=25% 19.1% 16.8% 2.3 p.p. (4.7 p.p.)		0.627 0.360 0.301 0.644	
Went to college with grad rate >25% and <=50% 23.0% 18.0% 5.0 p.p. (5.5 p.p.)			

Appendix B Mathematica Table B.3. (Continued)

	(treatment)	(control)	attending KIPP
	Went to college with below average rate of recipients		21.5% 24.3% –2.9 p.p. (5.9 p.p.) 0.046 0.627
Went to college with above average rate of recipients	48.3% 35.1% 13.1 p.p.* (6.6 p.p.)		

Note: Study includes 1,177 students who applied to enter KIPP middle schools via admissions lotteries, and compares the outcomes of students offered admission to KIPP (treatment group) to those not offered admission (control group) at the time of the lottery. Estimates of the impact of KIPP attendance use the lottery as an instrument for whether a student ever attended a KIPP middle or high school. The regression model pools all 13 lottery schools, controls for baseline covariates, and includes weights to account for probability of assignment to the treatment or control group. Standard errors are reported in parentheses under each impact estimate. Sample for college graduation rate variables = 1,173.

[a] Primary outcome measure

p.p. = percentage points

*Significantly different from zero at the .05 level, two-tailed test.

**Significantly different from zero at the .01 level, two-tailed test.

4. Annual impacts of KIPP attendance for primary outcome measures

In addition to estimating postsecondary impacts for students who attended a KIPP middle school, we also estimated the impact of attending KIPP for a single year. This model assumes that the impact of attending KIPP is equal across years, and that these annual impact accumulate in a linear fashion across both middle and high school. To estimate these annual impacts, we used a model similar to the exploratory TOT model presented in the main text (IV equations 1 and 2), but used the KIPP admission lottery as an instrument for *the number of years* that a student attended a KIPP school. We find that attending a KIPP school for one year increases the likelihood that a student will enroll in a four-year college by a statistically significant 2.5 percentage points (Table B.4). The impact estimate for college persistence through four semesters is 1.7 percentage points but is not statistically significant.

Table B.4. Annual impact of KIPP attendance on college enrollment and persistence

Primary outcome	Impact estimate	*p*-value	Sample size

Ever enrolled in four-year college	2.5 p.p.* (1.2 p.p.)	0.039	1,177
Persisted through first four semesters (four year college)	1.7 p.p. (1.1 p.p.)	0.125	1,177

Note: Standard errors are reported in parentheses under each impact estimate. Estimates of the impact of one year of KIPP attendance use the lottery as an instrument for the number of years that a student attended a KIPP school. The regression model pools all 13 lottery schools, controls for baseline covariates, and includes weights to account for probability of assignment to the treatment or control group. p.p. = percentage points.

*Impact estimate is significantly different from zero at the .05 level, two-tailed test.

**Impact estimate is significantly different from zero at the .01 level, two-tailed test.

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