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The Role of Specific Subjects in Education Production Functions: Evidence from Morning Classes in Chicago Public High Schools*

Kalena E. Cortes, Jesse Bricker, and Chris Rohlfs

Abstract

Absences in Chicago Public High Schools are 4-7 days per year higher in first period than at other times of the day. This study exploits this empirical regularity and the essentially random variation between students in the ordering of classes over the day to measure how the returns to classroom learning vary by course subject, and how much attendance in one class spills over into learning in other subjects. We find that having a class in first period significantly reduces grades in that course but does not affect grades in related subjects. We also find that having math in first period reduces test scores in all subjects and reduces grades in future math classes. These effects are particularly large for black students. For classes other than math, we find little effect of having the class in first period on test scores or long-term grades.

KEYWORDS: education production, subject-specific, math, english, morning classes, first period, course schedule, quasi-experimental, attendance, absenteeism, chicago, high school

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I. INTRODUCTION

This study provides a more complete picture than has previously been possible of the ways in which days and quality of instruction in specific classes affect learning in specific subjects in high school. To measure the effects of days and quality of instruction, we use a quasi-experimental design based on exogenous differences in course scheduling in Chicago Public High Schools. After students select the courses that they will take in a semester, the ordering of classes over the day is a computerized and essentially random process that is determined based on scheduling constraints.¹ Relative to later periods of the day, classes that meet in first period are associated with higher rates of absenteeism and greater morning grogginess, both of which inhibit student learning. For the typical Chicago Public High School student, attendance is roughly six days per year lower in first period classes than in other periods of the day. To the extent that missing and being groggy in math classes reduces students' test scores in math, we should expect students who take math in first period to perform worse than average on standardized tests in math, and we would expect similar negative effects of having English or social studies first period on reading test scores.

Our empirical strategy does not enable us to disentangle the absenteeism effect from the grogginess effect; however, our detailed set of outcome variables makes it possible to measure many different ways in which the negative shock of having a class in first period affects learning. We examine direct effects of instruction in a given subject on grades and standardized test performance in that subject, spillover effects of instruction in a subject on performance in related subjects, and long-term effects of instruction in a subject on performance in the same subject in a later year.

This research contributes to three active debates in education policy. First, a number of previous studies examined the cognitive and labor market effects of math and technical courses. Most of this work has involved associational studies based upon student-level variation in courses taken (Bishop and Mane, 2004; Jacobson, LaLonde, and Sullivan, 2005; Levine and Zimmerman, 1995; Rose and Betts, 2004; Sebring, 1987), or school-level variation in courses offered (Altonji, 1995; Attewell and Domina, 2008; Bishop and Mane, 2004). Two notable recent studies have used the more plausibly exogenous variation generated by state- and school-level policies requiring additional math instruction in high schools (Goodman, 2008; Joensen and Nielsen, 2009). Both studies find that such policies increased wages; the Goodman study finds that the returns were concentrated

¹ In private discussions, school administrators have indicated that the process is computerized and essentially random. In practice, we observe some correlations between first period course subject and student and classroom characteristics; however, these correlations disappear after controlling for teacher-by-year-by-grade fixed effects.

among black students. Additionally, Dee and Cohodes (2008) find that having a teacher who is state-certified in math improves math scores and having a teacher who is state-certified in social studies improves social studies test scores; they find no effects of having state-certified science or English teachers. This study contributes to this literature by measuring the effects of instruction in multiple subjects and looking at its effects on a variety of outcomes.

In addition to the literature on subject-specific learning, this study also contributes to the literatures on the cognitive returns per day of education and on the cognitive effects of school start times. Previous studies on the returns per day of schooling have examined the effects of summer break (Cooper, et al. 1996), mandatory remedial summer school (Jacob and Lefgren, 2004), variation in testing dates across students (Fitzpatrick, Grissmer, and Hastedt, 2011), snow days and school closures (Hansen, under revision; Marcotte and Hemelt, 2008), and making the school year start earlier, holding the test date constant (Marcotte and Hemelt, 2008). These studies generally find that each day of instruction improves test performance by roughly 0.01 standard deviations.² Delays in school start times are largely believed to operate through the grogginess mechanism more than the absenteeism mechanism (Wahlstrom, 2002); however, it has not been possible to disentangle the two mechanisms empirically. Recent work on these policies has found mixed results, with Carrell, Maghaikan, and West (2011) finding large effects among Air Force cadets and Hinrichs (2011) finding little or no effect in high schools in Minnesota, Kansas, and Virginia. Our study contributes to these two literatures by estimating the effects of a treatment that influences both absenteeism and grogginess. In addition, we construct new upper bound estimates of the effect of an hour of instruction as it varies across subjects.

Overall, we find large negative effects of first period classes on absences, course grades, moderate-sized negative effects on test scores, and some evidence of “spillover” effects of first period classes on performance in other subjects. Specifically, we find that having a class in first period increases absences in that subject by four to seven days over the year and that it decreases class performance in that subject by 0.11 to 0.17 grade points on a four-point grading scale. These results are consistently significant and are stable across specifications and course subjects. The negative effect of first period on course grades may occur because absences and grogginess cause students to learn less than they otherwise would; however, some teachers may explicitly factor attendance into course grades. We find no evidence of negative cross-subject effects of having a class in first period on class performance in similar subjects. We do, however, find a consistently

² One notable exception is Jacob and Lefgren (2004), who find that mandatory summer school (which involves instruction together with the stigma of failing) has little or no effect. Additionally, two recent studies find negative effects of morning classes and positive effects of mandatory attendance at the college level (Dills and Hernández-Julián, 2008; Dobkin, Gil, and Marion, 2010).

negative and sometimes significant long-term effect of having Algebra I in first period on students' later Algebra II grades. We do not find a similar long-term effect for French II or Spanish II, possibly due to small sample sizes. Hence, failure to acquire math skills appears to have a long-term negative effect; however, the lack of evidence on other kinds of spillovers suggests that many of the skills that students failed to acquire due to absenteeism and grogginess were specific to those classes.

Our results provide suggestive evidence of some moderately-sized negative effects of having a class in first period on test performance. For black students, we observe negative effects of having math in first period on spring math test scores. We observe smaller negative effects of having first period math on test scores in the fall, when students had not yet received a full year of treatment in a first period math class. We observe small and insignificant effects for non-black students and for the effects of first period English on reading scores. The analysis at the classroom level, we find negative spillover effects of having math in first period on test score growth in science and reading. We also observe net negative effects of first period science, English, social studies, and foreign language on test score growth; however, these effects do not appear to follow a clear subject-specific pattern.

Our estimated effects on test scores are comparable to previous quasi-experimental estimates. Hansen (under revision) finds that, among third, fifth, and eighth graders in Colorado, Maryland, and Minnesota, a missed day of school reduces test scores by roughly 0.01 standard deviations. If we suppose that all of the negative effects that we find of first period math on math test score growth are attributable to missed days of instruction, we estimate an effect of -0.011 standard deviations per math class missed.

In addition to contributing to the general understanding about subject-specific learning, this study provides policy implications regarding course scheduling. Our analysis suggests that students benefit most from math classes when they are taught after first period. Most teachers teach a single subject and work over the full day; hence, switching math class to the later in the day for some students would typically require switching math class to the morning for other students. The heterogeneous effects of first period suggest, however, that having math in first period is especially harmful for at-risk students, and the gains to moving at-risk students' math classes to the afternoon probably outweigh the losses to moving other students' math classes to the morning.³ Additionally, our results suggest that math teachers' preparation time should be scheduled during first period.

³ In our main specification, we use race to proxy for socioeconomic status and risk. We find similar results (not shown) using past test scores or an index of neighborhood socioeconomic status.

II. EMPIRICAL FRAMEWORK

We can model the effects of absenteeism on course grades and test scores in different subjects according to the following structural equation:

$$(1) \quad Y_{ist} = \sum_{s'} \alpha_s^{s'} * Absences_{is't} + \beta_s' \mathbf{x}_{ist} + \varepsilon_{ist},$$

where Y_{ist} represents a measure of student i 's performance in subject s in academic year t , defined as a function of absences (days missed) in multiple subjects s' , a vector \mathbf{x}_{ist} of observable student-, subject-, and classroom-specific characteristics, and high school and teacher fixed effects. Lastly, ε_{ist} represents the random error.

The parameters of interest are α_s^s , the direct effect of absences in subject s on performance in that subject, and $\alpha_s^{s'}$, the spillover effects of absences in subject s' on performance in subject s . Because absences are not randomly assigned across subjects, estimating Equation (1) using ordinary least squares (OLS) will produce biased estimates of these parameters.

To address this problem, we can model $Absences_{is't}$ as an endogenous variable that is determined by the following first-stage equation:

$$(2) \quad Absences_{is't} = \gamma_{s'} * First_{is't} + \delta_s' \mathbf{x}_{is't} + v_{is't},$$

where $First_{is't}$ is an indicator for whether subject s' is student i 's first period class in year t . Because students often arrive to school late, $\gamma_{s'}$ is expected to be positive. As a maintained assumption, we require that $First_{is't}$ is uncorrelated with the unobserved student-, classroom-, or school-level determinants of subject-specific performance described by ε_{ist} . Given this restriction, we could estimate α_s^s and $\alpha_s^{s'}$ using two-stage least squares, where Equation (2) is the first-stage equation for absences in each subject s' . The corresponding reduced-form equation can be expressed as:

$$(3) \quad Y_{ist} = \sum_{s'} \pi_{1s}^{s'} * First_{is't} + \Pi_{2s}' \mathbf{x}_{ist} + u_{ist}.$$

One major concern in identifying α_s^s and $\alpha_s^{s'}$ is the exclusion restriction that $First_{is't}$ only affects student achievement through the channel of absenteeism. This assumption will be violated if first period is associated with unusually high rates of tardiness or morning grogginess – variables that are likely to have their own negative effects on performance – so that instrumental variables estimates of α_s^s and $\alpha_s^{s'}$ would overstate the magnitude of the true effects of absenteeism. Due to this limitation, we do not present two-stage least squares estimates in this study, and we focus solely on the reduced-form effects of first period classes.

Even if first period does have these auxiliary effects, comparing the reduced-form coefficients π_{1s}^s and π_{1s}' across different subjects can still provide useful evidence on the relative importance of instruction in different subjects. The reduced-form parameters can in this case be interpreted as the combined effects of first period through these multiple channels.

III. DATA AND SAMPLE CHARACTERISTICS

A. DATA SOURCES

The data for this study cover the universe of students in Chicago Public Schools from 1993-94 to 2005-06. These data have been used to study a variety of topics including high stakes testing (Jacob, 2005; Jacob and Levitt, 2003), school choice (Cullen, Jacob, and Levitt, 2005, 2006), teacher hiring (Aaronson, Barrow, and Sander, 2007), student crime (Jacob and Lefgren, 2003), and remedial summer school (Jacob and Lefgren, 2004, 2009). Our analysis focuses on students in grades 9-12, for which high school transcript data are available. These transcript data include course title and number, absences, period of the day, and a teacher identifier for each class taken. Course subject is taken from the course number. We classify some courses as advanced, low-level, or bilingual based on their titles.⁴ The data also include multiple standardized test scores and a detailed set of descriptive variables about the individual students and the Census blocks in which they live.⁵

Our main samples consist of student by year datasets of all students who took at least one math course and all students who took at least one English course in that year. For each student, we construct dummies for whether math or English was taken in first period. The vast majority of students had the same course in first period in the fall and spring. For cases in which the period changed or the student took more than one course in that subject in a semester, the first

⁴ A course is classified as advanced if the title includes any of the words “Advanced,” “Honors,” “College,” “Accelerated,” “Advanced Placement,” “International Baccalaureate,” or abbreviations thereof. The course “Algebra II” is sometimes listed as “Advanced Algebra” and is not counted as advanced. A course is classified as low-level if the title includes any of “Learning Disabled,” “Remedial,” “Introductory,” “With Support,” “Emotional Behavioral Disability,” or abbreviations thereof. A course is classified as bilingual if its title includes “Bilingual,” “English as a Second Language,” “Second Language,” the name of a non-English language (for courses not in the foreign language subject category), or abbreviations thereof.

⁵ Since the circulation of an early version of this paper and a detailed proposal sent to the Office of Research and Accountability in 2007 to request additional data, a brief working paper has been released by that office that examines the relationships between period of the day, absences, and passing rates in the 2005-06 school year (Schmidt, 2008).

period dummy is constructed for a randomly selected one of the courses taken that year.⁶ We measure yearly absences in each subject as the sum of fall and spring absences. Students' letter grades are converted into a four-point numerical scale (4.0 = A). A student's yearly grade in a subject is measured as the average of the fall and spring grades. For cases in which the student took more than one course in that subject in a semester, absences and grades in that subject are based on the randomly selected course, described above. Charter, magnet, and vocational schools are dropped from the sample as are schools that did not appear to use a seven-period daily schedule and after-school classes meeting in period eight or later. The remaining sample includes 82 high schools.⁷ The numbers of classes taken that year in each subject are included as control variables in all of the regressions. The standard errors in all of the regressions and t-statistics are clustered at the level of the school and year combination.

To measure cross-subject effects, we use a dataset of classroom-level averages. To construct these data, we begin with a dataset that includes about six or seven observations per student per semester, one for each course taken. We then take averages of each student characteristic for each teacher, period, and semester combination. Averages for each student characteristic are computed from the students with non-missing values for those variables. Most students do not take all eight of the subjects examined here in a given year, and the use of classroom-level data provides a straightforward and intuitive way to examine cross-subject effects without losing a large fraction of the sample. Variables such as "average absences in math" and "average grade in social studies" are computed for every classroom, regardless of subject. Each student who took at least one class in a subject that semester is counted once in the classroom-level average with absences or grade level equal to that student's personal average across the different classes in that subject taken that semester.

The standardized tests that were administered and the grades of students who took them vary from year to year in our sample. Consequently, the samples of students and classrooms for which test score data are available are smaller than the sample for which absences and course grades are observed. For the majority of students, 8th grade math and reading scores are available from the Iowa Test of Basic Skills (ITBS). For the more recent years, math, science, reading, and

⁶ Supplemental tutoring classes were not included in these averages and were classified as "other subject."

⁷ By examining paper records available at the Office of Research and Accountability, we were able to measure the times in which first period began for most schools and whether the different schools used "block scheduling" (that varies by day of the week) in the 2005-06, 2006-07, and 2008-09 school years. Among the schools with data, start times range from 6:40 A.M. to 9:08 A.M.; however, 91% of these schools begin between 7:30 A.M. and 8:00 A.M., 60% begin at exactly 8:00 A.M., and only 4% begin at 8:30 A.M. or later. We find that the effects of first period do not vary in a systematic way with the time of morning at which first period starts (results not shown).

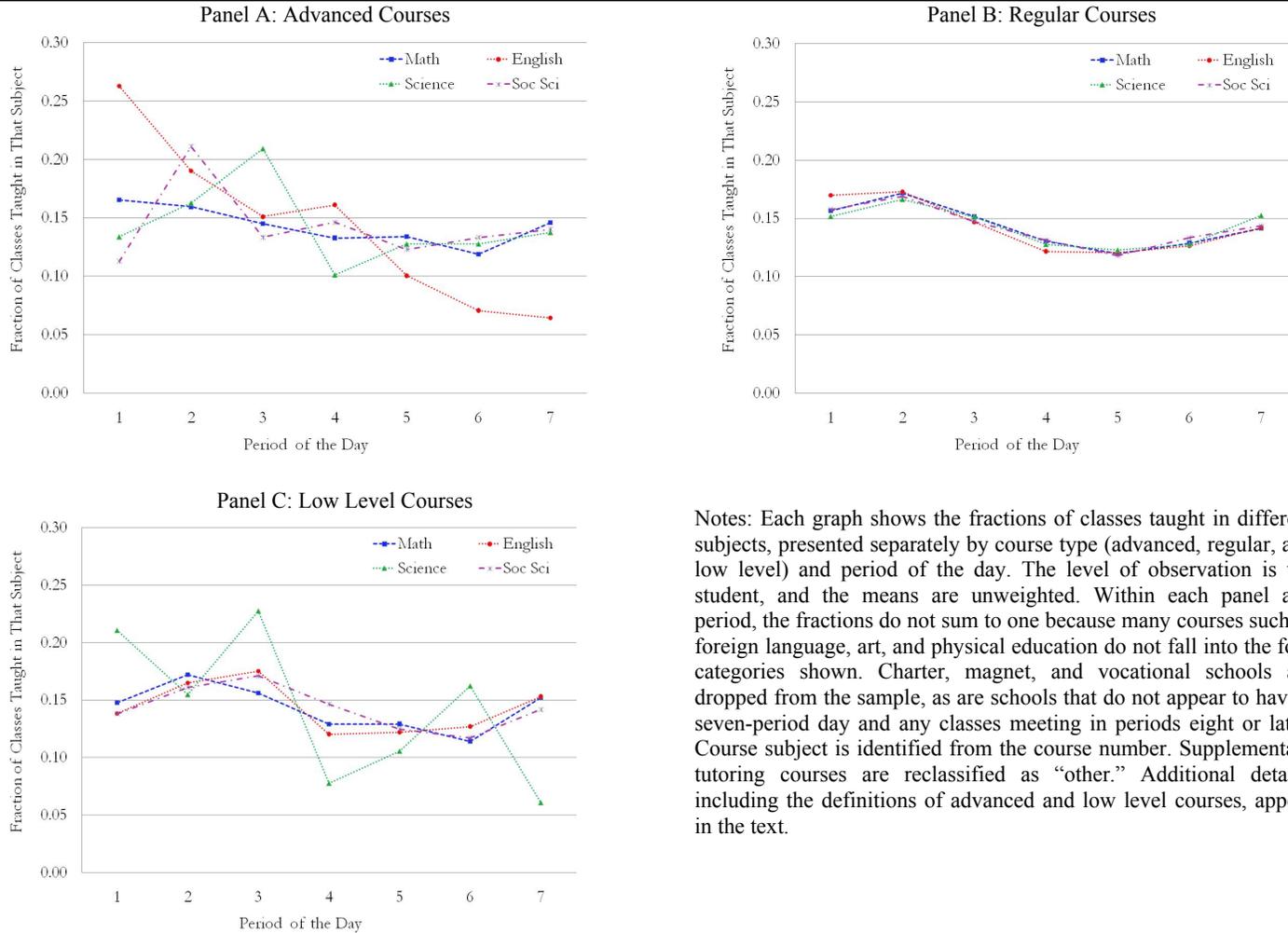
English scores are available from the 9th grade EXPLORE and the 10th and 11th grade PLAN exams, both administered in October. Math and reading scores are also available at varying grade levels from the Test of Achievement and Proficiency (TAP) in the earlier years of the sample and from the American College Test (ACT) in later years, and math, science, reading, and English scores are also available in the later years from the Prairie State Achievement Exam (PSAE). The TAP, PSAE, and ACT are administered in late April and early May. The ACT, EXPLORE, PLAN, and PSAE are produced by the same company and have similar test formats. To compare observations from different years in our sample, all of the test scores are converted into z-scores. For the student and classroom samples, the fall and spring test scores in different subjects are computed as the averages of the available scores. The fractions of scores from each type of test are included as control variables in the regressions.

B. DESCRIPTIVE STATISTICS

In order for this study to identify the causal effects of first period on absences and on academic performance, students who have key subjects such as math and English in first period must be otherwise similar to students who take those classes later in the day. One testable implication of this assumption is that the student and classroom characteristics should be similar between classes that meet in first period and those that meet later in the day. In Figure 1 and Tables 1, 2, and 3, we present evidence to evaluate this premise.

Table 1 shows separately by period of the day the fraction of classes in each subject. This table is calculated from unweighted student-level data and the fractions in each column sum to one. As Table 1 shows, the breakdown of classes by subject is generally stable over the course of the day, and schools do not appear to systematically schedule academic subjects in low-absence periods.

Figure 1: Distribution of Course Subjects by Course Level and Period of the Day



Notes: Each graph shows the fractions of classes taught in different subjects, presented separately by course type (advanced, regular, and low level) and period of the day. The level of observation is the student, and the means are unweighted. Within each panel and period, the fractions do not sum to one because many courses such as foreign language, art, and physical education do not fall into the four categories shown. Charter, magnet, and vocational schools are dropped from the sample, as are schools that do not appear to have a seven-period day and any classes meeting in periods eight or later. Course subject is identified from the course number. Supplementary tutoring courses are reclassified as “other.” Additional details, including the definitions of advanced and low level courses, appear in the text.

Panels A, B, and C of Figure 1 also show separately by course level (advanced, regular, or low-level) and period of the day the fraction of classes in each subject. This figure is also calculated from student-level data. Only four subjects (English, social studies, science, and math) are shown; consequently, within each course level and period, the sum of the numbers on the graph is less than one. As the figure shows, the breakdown of course level by these four subjects is generally stable over the course of the day. We do, however, observe that advanced English classes and low-level science classes are particularly likely to meet in first period.⁸

Table 1: Distribution of Course Offerings by Period of the Day, Chicago Public High Schools, 1993-94 to 2005-06

Course Subject	Period is ...						
	1st	2nd	3rd	4th	5th	6th	7th
Mathematics	0.14	0.14	0.14	0.14	0.14	0.14	0.13
Science	0.11	0.12	0.12	0.12	0.12	0.12	0.12
English	0.21	0.21	0.19	0.19	0.20	0.20	0.19
Social Studies	0.14	0.15	0.14	0.15	0.14	0.15	0.14
Foreign Language	0.06	0.06	0.06	0.06	0.07	0.07	0.06
Business	0.06	0.06	0.06	0.06	0.06	0.06	0.07
Art, Music, & P.E.	0.21	0.22	0.23	0.22	0.22	0.22	0.21
Shop	0.03	0.03	0.03	0.03	0.03	0.03	0.03
Vocational	0.02	0.02	0.02	0.02	0.02	0.02	0.02
Other	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Source: Chicago Public Schools High School Transcripts Data, 1993-94 to 2005-06.

Notes: Course distribution computed with student-level data. Each column sums to one. Charter, magnet, and vocational schools are dropped from the sample, as are schools that do not appear to have a seven-period day and any classes meeting in periods eight or later. Course subject is identified from the course number. Supplementary tutoring courses are reclassified as "other."

Next, we examine whether the students or classrooms with mathematics or English in first period are systematically different from other students and classrooms. Table 2 presents means of the control variables for the mathematics and English classrooms in our data, shown separately by period of the day. All

⁸ These correlations between first period and course level are likely to bias our estimated effects of first period English and science; we adjust for these biases in the regressions by including classroom characteristics, teacher-by-year fixed effects, and previous test scores as controls. Additionally, we control for fixed student and classroom effects in the individual and classroom-level test scores regressions by analyzing fall to spring test score growth as a dependent variable.

of the statistics reported in Table 2 and in later tables are computed from student-level data. Math classes are shown in columns (1) to (5) and English classes are shown in columns (6) to (10). Columns (1) and (6) show means for classes that meet in first period, columns (2) and (7) show means for classes that meet in second period, and columns (3) and (8) show means for classes meeting anytime from period three through seven. The t-statistics in columns (4) and (9) test the null hypothesis that the first period and second period means are equal, and the t-statistics in columns (5) and (10) test the null hypothesis that the first period means are equal to the third through seventh period means.

As Table 2 shows, first period math and English classes are fairly similar to math and English classes meeting later in the day. Due to the large number of observations, we reject the null hypothesis of equality across periods of the day for some of the control variables, in particular for English classes. Nevertheless, most of these significant differences are very small. For example, first period English classes tend to have somewhat more black students, fewer Hispanic students, and students who are slightly older. It is worth mentioning, that there are no differences in both previous test score achievement, as measured by students 8th grade math and reading ITBS scores, for first period math and English classes compared to math and English classes that meet later in the day (shown in the first two rows of Table 1). The lack of existing differences in 8th grade test scores between math and English classes meeting in first period and those meeting later in the day helps to rule out the possibility that other unobservable indicators of skill may also be correlated with having math and English in first period. That said, while the differences in the other covariates are not economically significant, the statistically significant relationships between first period classes and some of the control variables raises the possibility that having math or particularly English in first period may be correlated with a wide range of other confounding factors. To address these possible sources of bias, we examine in Table 3 the degree to which including teacher fixed effects can mitigate these biases.

Table 2: Sample Means of Control Variables by Math or English Period, Chicago Public High Schools, 1993-1994 to 2005-2006

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
		Math is Period . . .			T-Tests for Equality		English is Period . . .			T-Tests for Equality	
		1	2	3-7	1 vs. 2	1 vs. 3-7	1	2	3-7	1 vs. 2	1 vs. 3-7
Student Characteristics:											
1.	Math ITBS Z-Score (8th Grade)	0.03	0.02	0.03	0.63	-0.30	0.01	0.00	0.01	0.57	0.26
2.	Reading ITBS Z-Score (8th Grade)	0.01	0.02	0.02	-0.34	-0.61	0.01	0.00	0.00	1.21	0.85
3.	Male	0.47	0.48	0.48	-1.09	-1.30	0.47	0.47	0.47	-0.51	-1.62
4.	White	0.08	0.07	0.08	2.68**	0.07	0.08	0.08	0.08	-0.26	-2.18**
5.	Black	0.61	0.61	0.60	-0.92	1.32	0.60	0.60	0.59	1.04	2.86**
6.	Asian	0.03	0.03	0.03	0.24	-0.66	0.03	0.03	0.03	-1.24	-0.41
7.	Hispanic	0.28	0.28	0.29	-0.18	-1.50	0.29	0.29	0.30	-0.83	-2.53**
8.	American Indian	0.00	0.00	0.00	0.04	1.53	0.00	0.00	0.00	-0.23	-0.70
9.	Free Lunch Eligible	0.82	0.81	0.81	0.53	1.95*	0.81	0.81	0.81	0.31	0.82
10.	Special Education	0.13	0.14	0.14	-2.25**	-1.88*	0.15	0.14	0.14	0.55	3.48**
11.	Lives with Biological Parent	0.91	0.91	0.91	0.29	0.54	0.91	0.91	0.91	-1.55	-3.25**
12.	Enrolled in Bilingual Education	0.33	0.33	0.34	0.86	-1.02	0.34	0.34	0.35	-0.58	-2.22**
13.	Age	15.54	15.52	15.54	1.13	0.12	15.81	15.79	15.77	1.01	2.42**
14.	Grade 9	0.32	0.33	0.33	-1.08	-1.02	0.28	0.28	0.28	0.09	-0.10
15.	Grade 10	0.30	0.29	0.29	0.38	1.10	0.24	0.25	0.25	-0.98	-1.90*
16.	Grade 11	0.26	0.26	0.27	-0.32	-1.09	0.26	0.25	0.26	0.67	-1.67*
17.	Grade12	0.12	0.11	0.12	1.78*	1.62	0.22	0.22	0.21	0.11	3.83**
Classroom Characteristics:											
18.	Advanced Classroom	0.09	0.08	0.08	2.01**	0.99	0.04	0.03	0.02	3.52**	6.39**
19.	Low-level Classroom	0.06	0.07	0.07	-0.92	-1.29	0.04	0.04	0.05	-2.23**	-3.91**
20.	Bilingual Classroom	0.01	0.01	0.01	1.39	0.36	0.05	0.04	0.04	4.40**	3.64**
21.	Class Size	23.24	23.50	23.15	-1.81*	0.78	23.37	23.70	23.41	-3.05**	-0.38

Table 2: Continued

Neighborhood Characteristics:											
22.	Mean Education	11.89	11.94	11.92	-3.35**	-2.08**	11.89	11.93	11.91	-2.78**	-2.00**
23.	Percent in Poverty	0.26	0.26	0.26	-0.08	1.02	0.26	0.26	0.26	1.21	2.63**
24.	Median Family Income	30,654	30,853	31,012	-1.08	-2.11**	30,678	30,993	31,120	-1.68*	-2.78**
25.	Percent Hispanic	0.23	0.22	0.23	1.22	-0.21	0.23	0.23	0.24	0.45	-1.50
26.	Percent Black	0.56	0.56	0.55	-0.86	1.55	0.55	0.55	0.54	0.96	2.93**
27.	High School Graduate	0.64	0.65	0.64	-3.07**	-1.48	0.64	0.64	0.64	-3.04**	-1.91*
28.	College Graduate	0.14	0.14	0.14	-2.54**	-2.24**	0.14	0.14	0.14	-3.12**	-2.81**
	Obs (students)	60,971	66,483	260,236	127,097	320,848	81,470	82,876	312,512	164,039	393,647
	School x year clusters	645	682	769	717	790	671	700	779	738	806

Notes: Individual-level data are used and sample restrictions are the same as Table 1. Only students who took at least one math class are included in the sample in columns (1) to (5), and only students who took at least one English class are included in columns (6) to (10). For each student who took more than one math class or more than one English class, the math period and English period variables pertain to a randomly selected class taken that year. ITBS indicates students' scores on the Iowa Test of Basic Skills. Standard errors are clustered at the interaction of school and academic year for the t-tests. ** and * indicate 5% and 10% significance, respectively.

Table 3: Determinants of First Period Math and English

	Panel A: Math is Period 1					
	Black Students			Non-Black Students		
	(1)	(2)	(3)	(4)	(5)	(6)
Male	-0.001 (0.002)	-0.001 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.001 (0.003)	0.001 (0.002)
Asian	--	--	--	-0.010* (0.005)	-0.004 (0.005)	-0.005 (0.005)
Hispanic	--	--	--	-0.015** (0.004)	-0.005 (0.004)	-0.004 (0.003)
Free Lunch	0.000 (0.002)	0.001 (0.002)	0.002 (0.002)	0.003 (0.003)	0.003 (0.003)	0.001 (0.003)
Special Education	-0.005 (0.004)	0.006 (0.004)	0.008** (0.004)	-0.012** (0.004)	-0.002 (0.005)	-0.003 (0.005)
Parent Guardian	0.002 (0.002)	0.002 (0.002)	0.001 (0.002)	0.003 (0.004)	0.001 (0.005)	-0.001 (0.005)
Bilingual Education	-0.003 (0.007)	0.000 (0.007)	-0.005 (0.007)	0.002 (0.003)	-0.001 (0.003)	0.000 (0.003)
Age	0.002 (0.002)	0.001 (0.002)	-0.001 (0.001)	0.000 (0.002)	-0.001 (0.002)	0.000 (0.001)
Mean Education	-0.002 (0.002)	-0.002 (0.002)	0.000 (0.002)	0.000 (0.002)	0.000 (0.002)	0.001 (0.002)
Percent in Poverty	-0.024** (0.011)	0.001 (0.007)	0.002 (0.006)	-0.017 (0.012)	-0.013 (0.010)	-0.019* (0.010)
Mean Family Income	0.000** (0.000)	0.000 (0.000)	0.000* (0.000)	0.000** (0.000)	0.000 (0.000)	0.000 (0.000)
Percent Hispanic	0.011 (0.012)	-0.004 (0.011)	-0.009 (0.010)	-0.009 (0.008)	-0.002 (0.006)	0.000 (0.006)
Percent Black	0.013 (0.008)	-0.006 (0.007)	-0.009 (0.007)	-0.004 (0.009)	-0.002 (0.008)	-0.005 (0.008)
High School Graduate	0.014 (0.012)	0.017* (0.010)	0.012 (0.009)	-0.051** (0.017)	-0.022* (0.013)	-0.017 (0.012)
College Graduate	0.020 (0.014)	-0.001 (0.013)	-0.009 (0.012)	0.006 (0.018)	0.018 (0.012)	0.011 (0.011)
Additional Controls:						
Teacher x year F.E.	No	Yes	No	No	Yes	No
Teacher x year x grade F.E.	No	No	Yes	No	No	Yes
R ²	0.00	0.18	0.34	0.00	0.21	0.37
F-statistic	1.84	1.10	1.29	4.25	0.74	0.84
P-value for F-statistic	0.03	0.35	0.21	0.00	0.73	0.63

Table 3: Continued

	Panel B: English is Period 1					
	Black Students			Non-Black Students		
	(7)	(8)	(9)	(10)	(11)	(12)
Male	-0.003* (0.001)	-0.002 (0.002)	0.001 (0.001)	-0.005** (0.002)	-0.005** (0.002)	-0.003** (0.002)
Asian	--	--	--	-0.011** (0.004)	0.002 (0.003)	0.004 (0.003)
Hispanic	--	--	--	-0.006** (0.003)	-0.004 (0.002)	-0.002 (0.002)
Free Lunch	0.001 (0.002)	0.000 (0.002)	0.001 (0.002)	-0.009** (0.002)	-0.003 (0.002)	-0.003 (0.002)
Special Education	0.010** (0.003)	-0.008** (0.003)	-0.004 (0.003)	-0.008** (0.004)	-0.010** (0.004)	-0.003 (0.003)
Parent Guardian	-0.003* (0.002)	-0.001 (0.002)	-0.001 (0.002)	-0.006* (0.003)	0.000 (0.003)	-0.005 (0.003)
Bilingual Education	0.013** (0.005)	0.004 (0.006)	0.005 (0.005)	0.005** (0.002)	0.000 (0.002)	-0.001 (0.002)
Age	0.012** (0.001)	0.005** (0.002)	-0.003** (0.001)	0.015** (0.001)	0.010** (0.002)	0.001 (0.001)
Mean Education	0.000 (0.002)	0.003* (0.002)	0.003* (0.001)	0.000 (0.002)	-0.001 (0.001)	0.000 (0.001)
Percent in Poverty	-0.003 (0.010)	0.005 (0.005)	0.002 (0.005)	0.006 (0.011)	-0.001 (0.008)	0.004 (0.008)
Mean Family Income	0.000* (0.000)	0.000 (0.000)	0.000 (0.000)	0.000** (0.000)	0.000 (0.000)	0.000 (0.000)
Percent Hispanic	0.033** (0.011)	0.009 (0.008)	0.010 (0.008)	-0.005 (0.007)	0.000 (0.004)	0.000 (0.004)
Percent Black	0.033** (0.007)	0.002 (0.006)	0.002 (0.005)	0.027** (0.008)	0.007 (0.006)	0.005 (0.006)
High School Graduate	-0.005 (0.010)	0.003 (0.009)	0.002 (0.008)	-0.036** (0.015)	-0.001 (0.008)	-0.007 (0.008)
College Graduate	-0.011 (0.016)	-0.023** (0.010)	-0.012 (0.009)	0.020 (0.016)	0.007 (0.008)	0.005 (0.008)
Additional Controls:						
Teacher x year F.E.	No	Yes	No	No	Yes	No
Teacher x year x grade F.E.	No	No	Yes	No	No	Yes
R ²	0.00	0.20	0.37	0.01	0.20	0.36
F-statistic	14.30	1.75	1.23	20.39	3.37	1.14
P-value for F-statistic	0.00	0.04	0.25	0.00	0.00	0.32

Notes: The regression results presented here test the null hypothesis that having a key subject in first period is uncorrelated with other key determinants of academic performance. Each column shows results from a different regression. The dependent variable in columns (1) to (6) is "Math is Period 1," and the dependent variable in columns (7) to (12) is "English is Period 1." The dataset is the same as in Table 2; however, estimates are presented separately for black and non-black students. The F-statistic shown at the bottom of the table tests the joint significance of all of the regressors shown.

Table 3 shows regressions of “math is period 1” and “English is period 1” on all of the control variables presented in Table 2. Each column shows results from a different ordinary least squares (OLS) regression; the F-statistics at the bottom of the table test the null hypothesis that the coefficients on all of the controls are simultaneously equal to zero. The F-test can be viewed as a formal test of the hypothesis of quasi-random assignment of first period that was examined in Table 2. In columns (1) to (6), the dependent variable is an indicator for having math in first period, and in columns (7) to (12), the dependent variable is an indicator for having English in first period. The sample is restricted to black students in columns (1) to (3) and (7) to (9) and is restricted to non-black students in the remaining columns. Columns (1), (4), (7), and (10) regress first period on all of the control variables and a constant term. The F-statistics in these columns test the null hypothesis that first period is uncorrelated with all of the control variables. Columns (2), (5), (8), and (11) add teacher-by-school year fixed effects, and columns (3), (6), (9), and (12) replace those two-way interactions with teacher-by-school year-by-grade fixed effects. The F-statistics in these columns test the null hypothesis that, after controlling for teacher-by-year or teacher-by-year-by-grade effects, the remaining variation in first period is uncorrelated with the control variables

The results from Table 3 indicate that the two- and three-way fixed effects appear to eliminate the correlations between having a subject in first period and the control variables. Columns (1), (4), (7), and (10) of Table 3 confirm the general findings from Table 2. With some exceptions, the coefficients on the control variables are generally small; however, they are precisely estimated, and many are statistically significant, especially for English classes. As we see from the F-statistics and p-values at the bottom of Table 3, we reject the null hypothesis in all four cases; hence, first period math and English are significantly correlated with the control variables for both black and non-black students. When teacher-by-year fixed effects are added in columns (2), (5), (8), and (11), nearly all of the coefficients become insignificant in the math regressions, and for both black and non-black students, we fail to reject the null hypothesis that these coefficients are all simultaneously equal to zero. In the English regressions in columns (8) and (11), adding the teacher-by-year fixed effects reduces the significance of the coefficients on the controls. The F-test for the full set of coefficients is marginally significant for black students and still significant for non-black students. When the teacher-by-year-by-grade fixed effects are added in columns (3), (6), (9), and (12), all four F-statistics become insignificant. Hence, after controlling for teacher-by-year-by-grade fixed effects, we fail to reject the null hypothesis that having a class in first period is uncorrelated with all of the other controls.

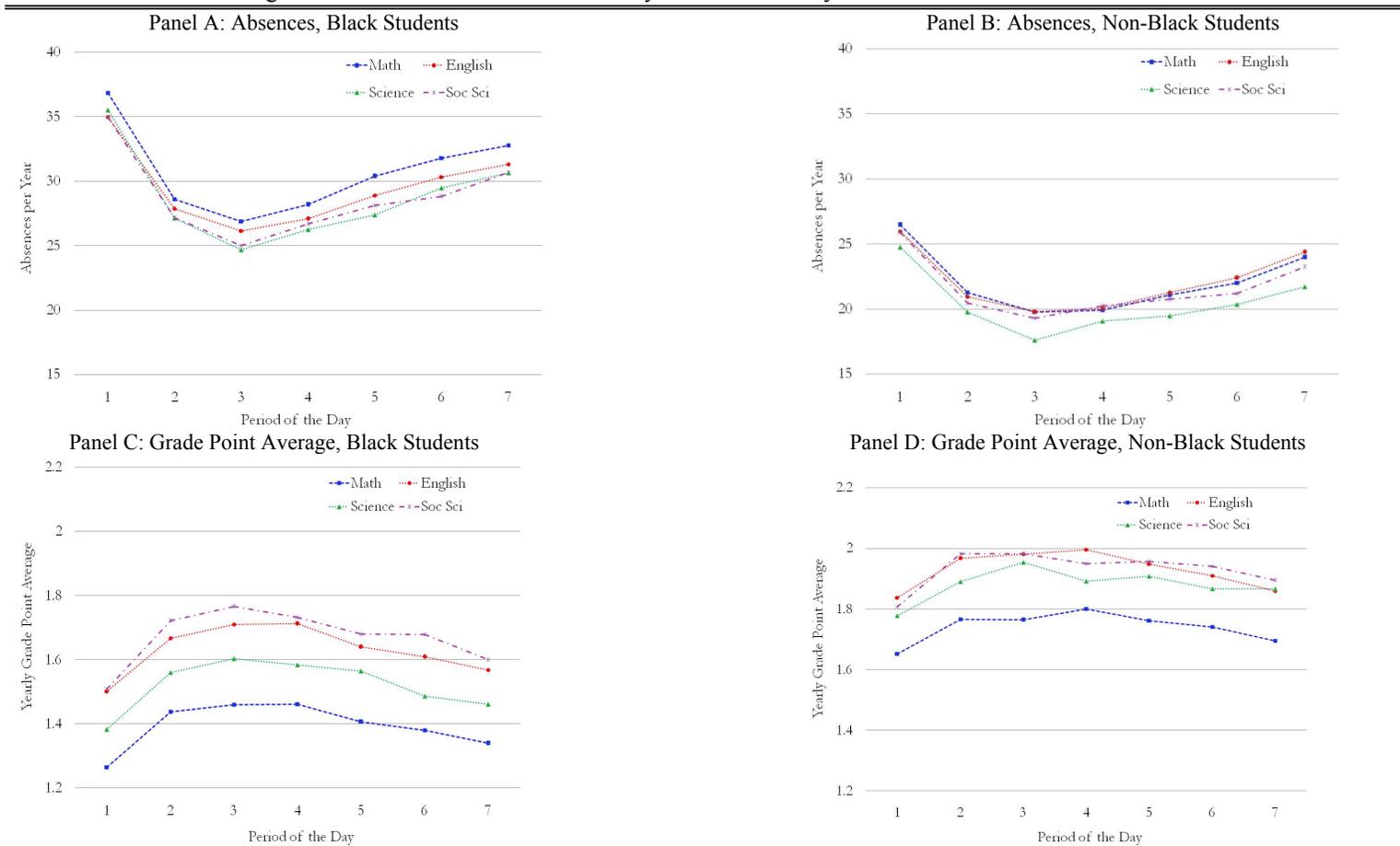
Next, in Figures 2 and 3, we examine graphically the reduced-form effects of having math and English in first period on absences in math and English, course

grades, and test scores. Panels A and B of Figure 2 show year-long absences (out of 180 school days) for black and non-black students in math, English, science, and social studies classes as functions of the period of the day in which each class meets. Panels C and D show the averages of fall and spring course grades in those subjects as functions of class period.

As panels A and B show, absences in all four of the subjects shown are at their highest levels in first period, they decline until reaching their lowest levels in third and fourth period, and then they gradually rise over the remainder of the day. This pattern is most pronounced among black students, for whom yearly absences vary across periods and subjects from 25 to 36. Nevertheless, the pattern is also detectable among non-black students, for whom yearly absences vary across periods and subjects from 19 to 26. As panels C and D show, the reverse pattern can be seen in course grades in all four subjects for both black and non-black students, with grades up to 0.3 points higher in classes meeting in periods three and four than in classes meeting in first period.

The four panels of Figure 3 plot fall and spring test scores and fall to spring test score growth (measured as the change in z-score) as functions of period of the day. Panels A and B show math scores as functions of math period for black and non-black students. Panels C and D show reading scores as functions of English period for black and non-black students. In panel A, spring math scores are somewhat lower for black students taking math in first period than for those taking it later in the day. Other than this difference, no clear relationships are apparent in the other panels, and we do not observe strong reduced-form relationships between having math or English in first period and math or reading test scores. An additional point to note from Figure 3 is that the magnitudes for the fall to spring test growth variables do not match the observed differences between spring and fall scores. These discrepancies arise because there is a relatively small number of years during which students took both fall and spring tests, and the growth data are taken from this smaller sample.

Figure 2: Absences and Course Grades by Period of the Day for Black and Non-Black Students



Notes: Each mean on the graph is calculated using student-level data. Each panel shows average absences or GPA, presented separately by black and non-black students, subject, and period of the day in which the class meets. Year-long absences are measured as the sum of average fall and average spring absences in that subject. Year-long course grades are measured as the average of each student's fall and spring grades in that subject. Additional details are in the text.

Figure 3: Standardized Test Scores and Test Score Growth by Period for Black and Non-Black Students



Notes: Means for fall (October) and spring (April or May) test scores are calculated at the student-level; students who do not take the tests are not included. Test score growth is computed as the change from fall to spring for that student. Additional details are in the text.

IV. DISCUSSION OF RESULTS

A. OWN-SUBJECT EFFECTS OF FIRST PERIOD MATH AND ENGLISH

In Table 4, we use multivariate regressions to estimate the reduced-form effects of having math or English in first period while controlling for other determinants of student achievement. Within each panel of Table 4, each of the six columns shows results from a different regression in which the regressor of interest is a dummy for whether the class meets in first period. In columns (1) to (6), the sample is restricted to math classes, and in columns (7) to (12), the sample is restricted to English classes. As in Table 3, columns (1) to (3) and (7) to (9) show results for black students and columns (4) to (6) and (10) to (12) show results for non-black students. Panels A and B show first-stage OLS estimates of Equation (2), where the dependent variables are year-long absences in math and English, respectively. Panels C through J show reduced-form OLS results from a restricted form of Equation (3), where cross-subject effects are not included and the dependent variables are different measures of student achievement as listed in the panel headings. The spring and fall math and English grades shown here are the average grades across the math or English course taken that year. All of the regressions in panels A and C control for the average number of math classes taken by the student that year, all of the regressions in panels B and D control for the average number of English classes taken, and all of the regressions in panels E to J control for the fraction of students with non-missing test scores and the fractions taking each type of test. Each regression in columns (1), (4), (7), and (10) includes those controls plus teacher-by-year fixed effects and the full set of student-, classroom- and neighborhood-level characteristics except for ITBS scores (as listed in rows 3 to 28 of Table 2). Columns (2), (5), (8), and (11) replace the teacher-by-year fixed effects with teacher-by-year-by-grade fixed effects. Columns (3), (6), (9), and (12) include all of these controls and add 8th grade math and reading ITBS scores as controls. The ITBS scores are missing for some students; consequently, the sample sizes in columns (3), (6), (9), and (12) are slightly smaller than in the preceding columns.

As panels A through D of Table 4 show, adding controls and fixed effects has relatively little effect on our estimated effects of first period math on math absences and grades and of first period English on English absences and grades. We find that having math in first period increases year-long math absences by 7.1 to 7.5 days for black students and by 5.2 to 5.7 days for non-black students. Likewise, we find that having English in first period increases year-long English absences by 6.6 to 7.0 days for black students and 4.5 to 5.1 days for non-black students. All four effects are robust to the inclusion of controls and are significant

Table 4: Reduced-Form OLS Estimates of Effects of First Period Math and English on Absences, Grades, and Test Scores

	(1)	(2)	(3)	(4)	(5)	(6)
Effects of First Period Math in Columns (1) to (6)						
	Black Students			Non-Black Students		
Panel A: Yearly Absences in Math						
First Period	7.102 (0.395)**	7.313 (0.399)**	7.481 (0.404)**	5.154 (0.450)**	5.542 (0.479)**	5.727 (0.505)**
R ²	0.27	0.36	0.35	0.20	0.28	0.28
Obs (students)	233,082	233,082	185,020	154,123	154,123	101,722
Schl x year clusters	790	790	745	665	665	631
Panel C: Average (Yearly) Math Grade						
First Period	-0.147 (0.011)**	-0.149 (0.011)**	-0.145 (0.011)**	-0.110 (0.016)**	-0.120 (0.016)**	-0.122 (0.016)**
R ²	0.22	0.30	0.34	0.20	0.28	0.35
Obs (students)	233,082	233,082	185,020	154,123	154,123	101,722
Schl x year clusters	790	790	745	665	665	631
Panel E: Spring Math Exam						
First Period	-0.034 (0.012)**	-0.038 (0.012)**	-0.021 (0.008)**	-0.005 (0.020)	-0.007 (0.020)	0.005 (0.013)
R ²	0.38	0.43	0.61	0.42	0.46	0.67
Obs (students)	105,171	105,171	82,236	64,332	64,332	46,874
Schl x year clusters	638	638	608	523	523	483
Panel G: Fall Math Exam						
First Period	-0.017 (0.016)	-0.022 (0.018)	-0.014 (0.012)	-0.002 (0.027)	-0.007 (0.028)	-0.021 (0.019)
R ²	0.38	0.42	0.55	0.40	0.45	0.61
Obs (students)	51,151	51,151	45,973	30,421	30,421	25,510
Schl x year clusters	249	249	246	217	217	211
Panel I: Fall-to-Spring Difference in Math Exam						
First Period	0.009 (0.023)	0.012 (0.023)	0.005 (0.023)	0.017 (0.028)	0.016 (0.029)	0.016 (0.029)
R ²	0.14	0.16	0.17	0.13	0.14	0.15
Obs (students)	13,561	13,561	11,912	11,189	11,189	9,463
Schl x year clusters	790	790	745	665	665	631
Student controls	Yes	Yes	Yes	Yes	Yes	Yes
Neighbrhd ctrls	Yes	Yes	Yes	Yes	Yes	Yes
Teacher x year F.E.	Yes	No	No	Yes	No	No
Teacher x year x grade F.E.	No	Yes	Yes	No	Yes	Yes
8th grade test scores	No	No	Yes	No	No	Yes

Notes: Within each panel, each column shows results from a different ordinary least squares regression using student-level data. The samples are the same as in Tables 2 and 3, with students taking math in columns (1) to (6) and students taking English in columns (7) to (12).

Table 4: Continued

	(7)	(8)	(9)	(10)	(11)	(12)
Effects of First Period English in Columns (7) to (12)						
	Black Students			Non-Black Students		
Panel B: Yearly Absences in English						
First Period	6.579 (0.324)**	6.982 (0.352)**	7.030 (0.374)**	4.464 (0.322)**	4.548 (0.336)**	5.118 (0.421)**
R ²	0.28	0.37	0.36	0.20	0.30	0.30
Obs (students)	281,165	281,165	216,770	195,243	195,243	123,720
Schl x year clusters	805	805	759	672	672	634
Panel D: Average (Yearly) English Grade						
First Period	-0.152 (0.011)**	-0.158 (0.011)**	-0.168 (0.011)**	-0.114 (0.013)**	-0.116 (0.013)**	-0.140 (0.016)**
R ²	0.24	0.33	0.34	0.22	0.31	0.34
Obs (students)	281,165	281,165	216,770	195,243	195,243	123,720
Schl x year clusters	805	805	759	672	672	634
Panel F: Spring Reading Exam						
First Period	0.016 (0.011)	0.012 (0.011)	0.002 (0.009)	0.025 (0.017)	0.023 (0.016)	-0.003 (0.012)
R ²	0.39	0.44	0.60	0.42	0.46	0.63
Obs (students)	134,586	134,586	104,162	83,310	83,310	59,669
Schl x year clusters	682	682	652	569	569	523
Panel H: Fall Reading Exam						
First Period	0.024 (0.023)	0.014 (0.029)	0.014 (0.023)	0.023 (0.024)	0.013 (0.029)	0.013 (0.023)
R ²	0.36	0.40	0.52	0.36	0.40	0.52
Obs (students)	31,904	31,904	26,722	31,904	31,904	26,722
Schl x year clusters	220	220	216	220	220	216
Panel J: Fall-to-Spring Difference in Reading Exam						
First Period	-0.039 (0.024)	-0.039 (0.025)	-0.042 (0.026)	-0.046 (0.031)	-0.046 (0.031)	-0.057 (0.033)*
R ²	0.15	0.16	0.16	0.16	0.17	0.17
Obs (students)	13,694	13,694	12,037	11,595	11,595	9,813
Schl x year clusters	805	805	759	672	672	634
Student controls	Yes	Yes	Yes	Yes	Yes	Yes
Neighbrhd ctrls	Yes	Yes	Yes	Yes	Yes	Yes
Teacher x year F.E.	Yes	No	No	Yes	No	No
Teacher x year x grade F.E.	No	Yes	Yes	No	Yes	Yes
8th grade test scores	No	No	Yes	No	No	Yes

Notes (continued): Student, classroom, and neighborhood controls are those listed in Table 2. Student controls omit 8th grade test scores, which are included in columns (3), (6), (9), and (12). ** and * indicate 5% and 10% significance, respectively.

in all three specifications. We also find that having math in first period reduces math grades by 0.145 to 0.149 grade points for black students and by 0.110 to 0.122 for non-black students. Lastly, we find that having English in first period reduces English grades by 0.152 to 0.168 grade points for black students and by 0.114 to 0.140 for non-black students. These effects are also stable and significant in all specifications.

While the negative effects of first period classes may arise due to a causal effect of absenteeism or grogginess on student learning, they may simply reflect a mechanical relationship in which absenteeism or apparent effort factors directly into course grades. To test this theory, we examine test scores as an alternative measure of achievement in panels E through J.

The results in columns (1) to (3) of panel E confirm the finding from Figure 3 that, for black students, having math in first period is associated with lower math scores. This effect is moderately-sized, ranging from -0.021 to -0.038 standard deviations and is significant in all three specifications. In panel G, we see that a smaller and insignificant effect of first period math, ranging from -0.014 to -0.022, was observable in the fall. Hence, the effect of first period became greater as the amount of exposure increased over the course of the academic year for black students. We observe very small, generally negative, and insignificant effects of first period math on non-blacks' math scores in both spring and fall. Turning our attention to first period English, we strangely find generally positive and moderately-sized (though insignificant) effects of first period English on reading scores in panels F and H. For both black and non-black students and both in spring and fall, the effects decline as controls are added to the regressions and are close to zero in the most complete specifications in columns (9) and (12). Hence, in the complete specifications that control for many possible student- and classroom-level correlates of first period, we find negative effects of first period math and essentially zero effect of first period English. This result is consistent with the view that math test scores are more sensitive to hours of math instruction than reading test scores are to hours of English instruction.

In panels I and J, fall to spring differences in test scores are used as dependent variables in the regressions. There were relatively few school year and grade combinations in which students were administered fall and spring tests in the same subject. For this reason, using test score growth as a dependent variable shrinks the sample considerably. The numbers of observations in the test score regressions range from 9,463 to 13,694, as compared to 25,510 to 53,996 for the fall test score regressions and 46,874 to 134,586 for the spring test score regressions. For math classes, we find moderate-sized, positive, and insignificant effects of first period on math test growth for both black and non-black students. In all six cases, these coefficients are fairly small relative to their standard errors, and the positive effects are probably the result of small sample sizes and sampling

variation. For English classes, we find larger, negative, and insignificant effects (with one marginally significant coefficient) of first period on reading test growth for black and non-black students. These effects, which are reasonably large relative to their standard errors, could indicate that using the first difference effectively controls for student and course-level variables that caused bias in the spring and fall test regressions. Nevertheless, these insignificant effects, like those in panel I, could simply result from small sample sizes and sampling error. We examine this issue further through the use of classroom-level data in the next subsection.

B. CROSS-SUBJECT EFFECTS OF FIRST-PERIOD CLASSES

Next, we examine the hypothesis that having a class in first period affects absences and student achievement in related subjects. The classroom-level dataset is used for Tables 5, 6, and 7. Table 5 presents the first-stage results from an expanded form of Equation (2), in which having first period in one subject can affect absences in another subject. The dependent variables in these regressions are average absences in different subjects. Some classrooms have missing values for absences in a specific subject because no student in the class takes that subject; consequently, the sample size varies across the different columns. The regressors of interest are the indicators for what course subject is taught in that class interacted with an indicator for whether the class meets in first period. Each regression includes teacher-by-year fixed effects and the full set of controls minus previous 8th grade test scores, as in columns (1), (4), (7), and (10) of Table 4, and these controls are interacted with the course subject indicators. The own-subject effects of having a class in first period on absences in that subject appear in bold along the diagonal.

Along the diagonal of Table 5, we see that having a given subject in first period significantly increases absences in that subject by 3.6 to 6.8 days per year, with effects of 3.6 to 4.9 for English, Art, Music, & Physical Education (P.E.), and Shop, Vocational, & Other, and effects of 6.0 to 6.8 for the remaining subjects. We observe moderately-sized negative cross-subject effects, with one insignificant coefficient of -0.44, two marginally significant coefficients of -0.62 and -1.08, and the remainder significant and ranging from -0.77 to -1.66. The cross-subject effects that are smallest are those for foreign language, business, art, music and P.E., and shop, vocational, and other courses, all of which tend to be offered less frequently than math, science, English, and social studies (as can be seen in Table 1). Hence, having a given subject in first period tends to reduce absences in every other subject by one to two days per year, probably because having one subject in first period precludes any other subject from being a first period class.

Table 5: Reduced-Form Classroom-Level OLS Estimates of Cross-Subject Effects of First Period Classes on Absences

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Yearly Absences in . . .							
Regressor	Math	Science	English	Soc. Studies	Foreign Lang.	Business	Art/Music/P.E.	Shop/Vocational/Other
Math x First Period	6.535 (0.310)**	-1.264 (0.187)**	-1.566 (0.169)**	-1.342 (0.180)**	-1.189 (0.330)**	-1.481 (0.354)**	-1.026 (0.163)**	-0.617 (0.333)*
Science x First Period	-1.511 (0.184)**	6.790 (0.315)**	-1.655 (0.162)**	-1.461 (0.176)**	-1.288 (0.337)**	-1.515 (0.368)**	-1.215 (0.155)**	-1.253 (0.395)**
English x First Period	-1.534 (0.164)**	-1.486 (0.161)**	4.870 (0.214)**	-1.523 (0.147)**	-1.330 (0.247)**	-1.105 (0.236)**	-1.212 (0.122)**	-1.385 (0.244)**
Social Studies x First Period	-1.322 (0.180)**	-1.527 (0.187)**	-1.481 (0.159)**	6.319 (0.271)**	-0.899 (0.311)**	-1.259 (0.325)**	-1.298 (0.145)**	-1.109 (0.317)**
Foreign Language x First Period	-1.246 (0.234)**	-1.188 (0.241)**	-1.333 (0.204)**	-1.009 (0.219)**	6.595 (0.353)**	-1.320 (0.435)**	-0.961 (0.188)**	-1.144 (0.432)**
Business x First Period	-0.931 (0.280)**	-1.179 (0.311)**	-1.129 (0.241)**	-1.036 (0.258)**	-1.433 (0.534)**	5.994 (0.347)**	-0.774 (0.227)**	-1.276 (0.594)**
Art, Music, & P.E. x First Period	-1.358 (0.159)**	-1.377 (0.166)**	-1.465 (0.130)**	-1.366 (0.139)**	-1.414 (0.288)**	-1.271 (0.261)**	3.615 (0.180)**	-1.098 (0.273)**
Shop, Vocational, & Other x First Period	-1.575 (0.307)**	-1.243 (0.379)**	-1.205 (0.252)**	-1.130 (0.284)**	-0.437 (0.580)	-1.079 (0.561)*	-1.152 (0.262)**	4.938 (0.358)**
R ²	0.75	0.73	0.79	0.75	0.58	0.60	0.79	0.60
Obs (classrooms)	208,176	206,513	209,873	208,083	169,594	170,719	209,220	166,626
Schl x year clusters	754	755	755	755	729	728	754	718

Notes: Each column shows results from a separate regression where the outcome variable measures average absences in a given subject among students in that class. The regressors of interest are subject dummies interacted with “class is first period.” The level of observation is the classroom (teacher-by-period-by-semester). Each regression controls for student, classroom, and neighborhood controls (excluding 8th grade test scores), the average number of courses taken by subject, and teacher-by-year fixed effects. Own-subject effects are shown in bold. All controls are interacted with dummies for course subject. Standard errors are clustered by school-by-year. ** and * indicate 5% and 10% significance, respectively.

Table 6 presents reduced-form results from Equation (3), where the dependent variables are year-long course grades in different subjects. The specifications and format are the same as in Table 5 except for the change in the dependent variables. The results along the diagonal are similar to our findings from Table 4 and indicate that scheduling a class in first period significantly reduces average grades in that subject by 0.11 to 0.17 grade points. We also find cross-subject effects that are significant in 41 and marginally significant in three of the 56 coefficients, with generally positive coefficients ranging from -0.01 to 0.06, which suggests that having a subject in first period generally improves course grades in other subjects. Hence, like the cross-subject effects in Table 5, these effects probably occur because having first period in one subject prevents other subjects from being first period.

Table 7 presents additional results from Equation (3); however, the dependent variables in the four columns are the fall-to-spring changes in classroom average test scores for math, science, reading and English. The average numbers of courses taken in each subject are removed as control variables and replaced with the fractions of students with non-missing test scores and the fractions taking the different types of tests. The specifications and format are otherwise the same as in Tables 5 and 6. Because English and social studies courses may affect test score growth in both reading and English, the four pairwise combinations (English and reading, English and English, social studies and reading, and social studies and English) are all shown in bold. Because classroom-level data are used for the averages and blacks and non-blacks are combined, taking the fall to spring difference results in fewer lost observations than in the individual-level regressions panels I and J of Table 4.

The coefficients in Table 7 are considerably less significant than in Tables 5 and 6, with only one significant and four marginally significant coefficients out of the 32 estimates. Unlike with Tables 5 and 6, the own-subject effects are roughly the same signs and magnitudes as the cross-subject effects. As with Table 6, the cross-subject effects of first period may occur due to some combination of mechanisms including the effect of preventing other classes from being first period and true spillovers in learning. Strangely, we observe a large and stable negative effect of having shop, vocational, or other in first period on all test scores.

Table 6: Reduced-Form Classroom-Level OLS Estimates of Cross-Subject Effects of First Period Classes on Grades

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Yearly Average Course Grade in . . .							
Regressor	Math	Science	English	Soc. Studies	Foreign Lang.	Business	Art/Music/P.E.	Shop/Vocational/Other
Math x First Period	-0.129 (0.009)**	0.011 (0.009)**	0.026 (0.008)**	0.023 (0.009)**	0.021 (0.015)**	0.031 (0.016)**	0.033 (0.008)**	0.032 (0.017)**
Science x First Period	0.034 (0.009)**	-0.131 (0.009)**	0.045 (0.008)**	0.042 (0.009)**	0.028 (0.017)**	0.059 (0.018)**	0.050 (0.009)**	0.015 (0.019)
English x First Period	0.028 (0.007)**	0.037 (0.007)**	-0.105 (0.007)**	0.047 (0.007)**	0.034 (0.012)**	0.028 (0.013)**	0.047 (0.006)**	0.049 (0.013)**
Social Studies x First Period	0.021 (0.008)**	0.030 (0.008)**	0.031 (0.007)**	-0.147 (0.008)**	0.008 (0.016)*	0.031 (0.016)**	0.053 (0.008)**	0.035 (0.016)**
Foreign Language x First Period	0.025 (0.011)**	0.015 (0.012)	0.040 (0.010)**	0.029 (0.011)**	-0.169 (0.012)**	0.057 (0.024)**	0.028 (0.012)**	0.041 (0.025)*
Business x First Period	-0.005 (0.013)	0.014 (0.015)	0.015 (0.011)	0.017 (0.012)	0.017 (0.025)*	-0.153 (0.012)**	0.027 (0.012)**	-0.009 (0.032)
Art, Music, & P.E. x First Period	0.027 (0.006)**	0.025 (0.007)**	0.041 (0.006)**	0.038 (0.007)**	0.051 (0.014)**	0.037 (0.013)**	-0.123 (0.007)**	0.031 (0.014)**
Shop, Vocational, & Other x First Period	0.018 (0.013)**	0.015 (0.016)	0.016 (0.010)	0.030 (0.013)	-0.010 (0.027)	0.022 (0.029)	0.049 (0.013)**	-0.148 (0.012)**
R ²	0.68	0.67	0.73	0.71	0.59	0.61	0.77	0.58
Obs (classrooms)	208,176	206,513	209,873	208,083	169,594	170,719	209,220	166,626
Schl x year clusters	754	755	755	755	729	728	754	718

Notes: See notes to Table 5. Specifications are the same as in Table 5; however, the outcome variable is yearly average course grade rather than yearly absences. ** and * indicate 5% and 10% significance, respectively.

Table 7: Reduced-Form Classroom-Level OLS Estimates of Cross-Subject Effects of First Period Classes on Test Score Growth

Regressor	(1)	(2)	(3)	(4)
	Dependent Variable is Change in Classroom's Average Z-Score from Fall-to-Spring			
	Math (Difference)	Science (Difference)	Reading (Difference)	English (Difference)
Math	-0.046	-0.045	-0.073	-0.006
x First Period	(0.029)	(0.035)	(0.033)**	(0.031)
Science	-0.031	0.003	-0.013	-0.023
x First Period	(0.028)	(0.033)	(0.034)	(0.032)
English	-0.018	0.021	-0.022	-0.011
x First Period	(0.029)	(0.031)	(0.031)	(0.031)
Social Studies	-0.018	-0.007	0.025	-0.024
x First Period	(0.035)	(0.033)	(0.035)	(0.030)
Foreign Language	-0.019	0.006	-0.033	-0.032
x First Period	(0.030)	(0.032)	(0.028)	(0.029)
Business	0.053	-0.041	0.027	0.040
x First Period	(0.065)	(0.064)	(0.066)	(0.059)
Art, Music, & P.E.	0.054	0.024	0.058	0.048
x First Period	(0.033)	(0.033)	(0.032)*	(0.034)
Shop, Vocational, & Other	-0.085	-0.051	-0.096	-0.052
x First Period	(0.049)*	(0.055)	(0.057)*	(0.054)*
R ²	0.52	0.51	0.51	0.53
Obs (classrooms)	33,415	33,380	33,388	33,414
Schl x year clusters	229	229	229	229

Notes: The regression results reported estimate the effects of period x course subject on the difference between the spring and fall classroom-level average test scores for math, science, reading, and English tests. The own-subject are shown in bold (e.g., math classes on math scores). English and social studies classes could be viewed as primarily affecting both reading and English tests, so all four of those class-test combinations are bolded. The structure of the regressions is the same as in Tables 5 and 6. ** and * indicate 5% and 10% significance, respectively.

One possible reason for this negative effect is a selection bias that occurs because the shop and vocational courses taught in first period are different from those taught later in the day. For example, “food services,” and “personal development” are common courses in this category that are disproportionately taken in first period. Students taking shop, vocational, or other courses in first period have significantly higher 8th-grade test scores (but significantly lower growth over the year) than do students taking those courses later in the day.⁹

With the exception of this last category of classes, having math class in first period appears to be more harmful to test score growth than having any other subject in first period. The direct effects of first period math on math test score

⁹ The “shop, vocational, and other” category does include one-on-one math tutoring; however, these tutoring classes are sufficiently rare that they cannot explain the large negative effects observed in Table 7.

growth are negative but insignificant. We also observe moderately-large negative spillover effects of having math in first period on test score growth in science and reading. The mechanisms for these cross-subject effects are unclear; however, one possible explanation is that math classes include valuable instruction in how to take a standardized test. While imprecise, the remaining estimates suggest that having first period science, English, or foreign language is harmful to learning. We observe mixed results for first period social studies and business and positive effects of having art, music, or P.E. in first period.

If we suppose that the direct effects of first period (through the channel of grogginess) are known to be negative, then we can obtain a (negative) lower bound on the effect of missing a class in a given subject on test scores. This bound is constructed by dividing the effects that we estimate in Table 7 of first period on test scores by the effects that we estimate in Table 5 of first period on absences. Because the fall tests are taken in October and the spring tests are taken in late April, an adjustment is necessary to convert this ratio into a per-day measure of the effects of absenteeism. Roughly 120 days elapse between the October and April exams, whereas the school year lasts 180 days. Hence, some of the absences that are measured in the transcript data will have occurred before the fall exam and some will have occurred after the spring exam. If we assume that absences are uniformly distributed over the year, then we can adjust for this discrepancy by multiplying the ratios of coefficients by 1.5 (i.e., $120 \text{ days}/180 \text{ days}=1.5$).

Dividing the bold reduced-form coefficients in Table 7 by the corresponding first-stage effects on absences from Table 5 and multiplying by 1.5, we find that a single absence in math reduces math test score growth by 0.011 standard deviations (i.e., $(-0.046/6.535)*1.5=-0.0106$). We find a positive effect of a single absence in science on science test score growth of 0.0007 standard deviations. We find that an absence in English reduces reading and English test score growth by 0.007 and 0.003 standard deviations, respectively. Finally, we find mixed effects of +0.006 standard deviations in reading test score growth and -0.006 standard deviations in English test score growth per missed social studies class. The positive effect on reading score growth may reflect the effect of preventing another class from being first period, or it may simply reflect imprecision in the estimates.

If we suppose that missing the most useful class of the day reduces test score growth by roughly 0.011 standard deviations, and that many classes have roughly zero effect, our estimated effects of absences are comparable to Hansen's (under revision) estimate that a weather-induced missed day of school reduces test scores in Colorado, Maryland, and Minnesota by 0.003 to 0.039 standard

deviations, with an average estimate around 0.0127.¹⁰ Our estimates are somewhat large, however, relative to Jacob and Lefgren's (2004) finding that summer school has no detectable effect on Chicago sixth graders' test performance. One likely explanation for the differences in these various estimates is that the true effect of classroom attendance is heterogeneous. The students in Chicago Public Schools are slightly older and come from considerably poorer and less-educated families than do the students in the Colorado, Maryland, and Minnesota school districts examined by Hansen, and the returns to schooling could be negatively correlated with age and poverty. Additionally, by examining the effects of snow days, Hansen identifies the effect of the average day of school, whereas students may selectively skip classes that are less important, and unmotivated students skip at higher rates, so that the return to a typical day missed is probably lower than the return to the average day. Alternatively, Jacob and Lefgren (2004), by estimating the effects of being held back for summer school, identify the returns to attendance for a particularly unmotivated set of students. Moreover, as Jacob and Lefgren discuss, the negative psychological effect of holding students back may partially counteract the positive effects of classroom attendance in their study.

C. LONG-TERM EFFECTS OF FIRST PERIOD

Our final set of results examine the reduced-form effects of having a course in first period on performance in future classes in that same subject. To the extent that the own-subject effects of first period on course grades reflect actual learning, we should expect these effects to persist and to improve students' performance in the same subject in future years.

Table 8 presents our estimates of the long-term and short-term effects of first-period classes on math and foreign language course grades. Each of the nine columns shows results from a different regression. The student-level data are used, as in Table 4, and the full set of control variables (excluding 8th grade test scores) is included. In columns (1) to (3), the sample is restricted to students who took Algebra II and are observed in an earlier year of data having taken Algebra I. In columns (4) to (6), the sample is restricted to students who took both French I and II, and in columns (7) to (9), it is restricted to students who took both Spanish I and II. Panel A shows results for black students, and panel B shows results for non-black students. The dependent variable in columns (1) to (3) is the yearly average course grade in Algebra II, and the regressors of interest include an indicator variable for whether that class is first period as well as whether Algebra I was first period. In columns (4) to (6), the dependent variable is course grade in

¹⁰ Averaged across the 15 estimates in Hansen's Table 6 in which test scores are measured in standard deviations.

French II, and in columns (7) to (9), the dependent variable is course grade in Spanish II. In each case, the regressors of interest include an indicator for whether the second-level course meets in first period as well as an indicator variable for whether the introductory course met in first period. It should be noted that the effects of Algebra I, French I, and Spanish I are estimated for the subsample of students who went on to take the next level courses. If having the earlier course in first period reduces the likelihood of continuing in that subject, these estimates may understate the full long-term effect of having Algebra I, French I, and Spanish I on performance in that subject.

The results in columns (1) to (3) are consistent with a long-term learning effect of first period Algebra. As in Tables 4 and 6, we observe negative direct effects of having Algebra II in first period on Algebra II grades, in this case ranging from -0.120 to -0.176 and significant in all six specifications. In addition, we find that having Algebra I in first period is associated with a smaller negative effect on Algebra II grades, ranging from -0.035 to -0.059 and significant in two and marginally significant in two of the six specifications. Our estimates for French and Spanish classes are imprecise and vary widely in magnitude across specifications, probably due to the small sample sizes. Consequently, we are not able to draw clear inferences from the results in columns (4) to (9).

Table 8: Reduced-Form OLS Estimates of the Long-Term Effects of First Period on Later Course Grades

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Black Students									
Year-Long Course Grade in . . .									
	Algebra II			French II			Spanish II		
First Period	-0.157 (0.023)**	-0.179 (0.024)**	-0.176 (0.026)**	-0.308 (0.139)**	-0.304 (0.115)**	-0.339 (0.082)**	0.019 (0.145)	-0.127 (0.151)	-0.084 (0.173)
Previous Course in that Subject was First Period	-0.051 (0.023)**	-0.041 (0.019)**	-0.037 (0.020)*	0.139 (0.297)	0.362 (0.420)	0.364 (0.422)	0.092 (0.103)	0.065 (0.125)	0.137 (0.166)
R ²	0.04	0.26	0.33	0.16	0.39	0.47	0.04	0.23	0.31
Obs (students)	62,041	62,041	62,041	422	422	422	1,840	1,840	1,840
Schl x year clusters	662	662	662	32	32	32	57	57	57
Panel B: Non-Black Students									
First Period	-0.120 (0.028)**	-0.146 (0.030)**	-0.155 (0.031)**	-0.061 (0.118)	-0.162 (0.120)	-0.173 (0.126)	-0.279 (0.147)*	-0.154 (0.153)	-0.178 (0.185)
Previous Course in that Subject was First Period	-0.059 (0.032)*	-0.043 (0.031)	-0.035 (0.031)	-0.416 (0.156)**	-0.038 (0.111)	-0.064 (0.113)	0.194 (0.099)*	0.083 (0.085)	0.053 (0.094)
R ²	0.03	0.23	0.31	0.08	0.36	0.40	0.07	0.28	0.38
Obs (students)	51,713	51,713	51,713	1,425	1,425	1,425	2,183	2,183	2,183
Schl x year clusters	510	510	510	41	41	41	53	53	53
Teacher x year F.E.	No	Yes	No	No	Yes	No	No	Yes	No
Teacher x year x grade F.E.	No	No	Yes	No	No	Yes	No	No	Yes

Notes: The specifications are the same as in Table 4; however, the sample is restricted to those students who have taken specific course sequences involving at least one semester of an introductory course (Algebra I, French I, or Spanish I), and at least one semester of a later course that builds on that material (Algebra II, French II, or Spanish II). ** and * indicate 5% and 10% significance, respectively.

V. CONCLUSION

This study exploits quasi-random variation in the ordering of courses over the day to estimate the degree to which the returns to classroom attendance vary across course subject and the degree to which attending class in one subject has spillover effects onto learning in other subjects. We find that having a class in first period reduces grades in that course and has little effect on long-term grades or grades in related subjects. We also find suggestive evidence of moderate-sized own-subject and cross-subject effects on test scores, particularly for math classes. Our study also provides two policy implications: that math classes for at-risk students should be scheduled after first period and that math teachers' preparation time should be scheduled during first period.

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