Experimental Evidence of the Effects of the Communities In Schools of Chicago Partnership Program on Student Achievement

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I. Introduction

Like many other organizations with the goal of providing services to children from disadvantaged backgrounds, Communities In Schools (CIS) of Chicago has sought to systematically identify the effects of its partnership program on students' quantifiable outcomes. However, it is typically extremely difficult to carefully study the effects of program participation because those receiving services are not randomly drawn from the population. CIS of Chicago selects schools to participate in the program based on several criteria including both student and community need as well as the school's institutional capacity, and it is clear that simply comparing students served by CIS of Chicago to Chicago Public Schools (CPS) students not served by CIS of Chicago would yield biased estimates of the true effects of program participation: Since CIS of Chicago selects schools with needier student bodies than the general population of schools, outcomes in CIS of Chicago -served schools may be expected to be lower than typical outcomes in CPS. But since CIS of Chicago selects schools with relatively high institutional capacity and leadership motivation, it may also be the case that outcomes in CIS of Chicago-served schools could be better than those in observationally comparable schools. It is therefore typically impossible to carry out an apples-to-apples comparison of CIS of Chicago participants to non-participants.

In the selection process for the 2012-2013 academic year, CIS of Chicago decided to randomly admit qualified schools into the program, therefore creating the opportunity to experimentally study the effects of CIS of Chicago program participation on student academic outcomes. During the application process for schools conducted in winter and spring 2012, CIS of Chicago identified 47 applicant schools – all K-8 elementary schools -- that would meet the institutional capacity, leadership motivation, and student and community need criteria for inclusion into the program, but could only bring 20 new schools into the program. Rather than rank-ordering schools as had been done in the past, for 2012-2013 CIS of Chicago instead entered these 47 schools into a lottery for inclusion. It is therefore possible to directly compare the student outcomes in the 20 schools randomly selected to participate in the program to those in the 27 schools not selected to participate. This randomization allows us to solve the selection bias mentioned in the preceding paragraph, because in this case selection on the margin between inclusion and exclusion from the program was based not on need or capacity, but rather on random factors.

This paper presents evidence about the effects of CIS of Chicago program participation on student outcomes. To carry out this work, CPS made available de-identified data on student demographic characteristics, attendance, and test scores for all students who began the 2012-2013 academic year in one of the 47 applicant schools. These data permit me to follow students forward through the 2012-2013 and 2013-2014 academic years, as well as to control for demographic attributes and outcomes in prior years (2010-2011 and 2011-2012). I estimate intent-to-treat effects of program assignment based on the school the students attended on the 20th day of the 2012-2013 academic year.

I find that school participation in CIS of Chicago partnership program led to increases in student reading and mathematics proficiency rates. The estimated effects are particularly strong for African-American students and those who were new to their school in the first year of the experiment. These results indicate that CIS of Chicago's partnership program, which involves a light-touch approach, yields an increase in student academic outcomes at very low cost.

II. Communities In Schools of Chicago's partnership program

Through its partnership program, CIS of Chicago makes connections, builds organizational and programmatic capacity and develops relations between its CPS partner

schools and a vast network of service providers. It does this in two ways, through school connections and through partner connections.

After working with key school leaders to identify major student needs, CIS of Chicago staff provide each school partner with referrals for programs and services that address identified priority needs. These services are offered at no cost to students or schools through CIS of Chicago's community partner network of non-profit organizations, arts and cultural institutions, healthcare and prevention education organizations, and caring adults. CIS of Chicago staff work with principals, teachers and other leaders in each school to understand which specific programs support the core academic and social goals of students across all grades. CIS of Chicago also works to ensure that sufficient planning is undertaken by schools to allow connected programs to be integrated into students' and teachers' academic day.

On the community partnership side, CIS of Chicago connects its network of Chicago Public Schools to no-cost services and programs offered by more than 160 community partners each year. This programming addresses fundamental school needs by attempting to enhance students' physical and emotional wellbeing through the connection of health education, mental health promotion and physical health services; providing schools access to arts, college and career and other enrichment opportunities that are often not typically provided due to limited resources; and emphasizing knowledge and skills for students to make smart decisions inside and outside of school through prevention education and life-skills programming. CIS of Chicago also offers trainings, workshops and networking opportunities to help these organizations build their organizational capacity and improve program quality.

III. Data and method

CPS provided me with data for all students beginning the 2012-2013 academic year in one of the 47 schools that were part of CIS of Chicago's experiment. For each of these students, I have information about the student's race and ethnicity, date of birth, free lunch status, English as a second language, special education participation, and schools attended, grade in school, test scores and attendance in 2010-2011, 2011-2012, 2012-2013, and 2013-2014, regardless of where the student attended school so long as they attended school in CPS.

I estimate models in which the dependent variable is one of three outcomes – mathematics scores on the ISAT, reading scores on the ISAT, and school attendance – and the key explanatory variable is an indicator for whether the student began the 2012-2013 school year (as measured by 20th day attendance) at a school randomly selected to participate in the CIS of Chicago program.[2] I conduct all analyses considering 2012-13 academic outcomes, and then again considering 2013-14 academic outcomes. I control for student pre-treatment characteristics in the analysis, and all standard errors are adjusted for clustering at the initial school assignment level. I estimate both intent-to-treat (ITT) effects of CIS of Chicago assignment at the start of the 2012-2013 school year, as well as treatment-on-the-treated (TOT) effects in which "treatment" is defined as spending the majority of the 2012-2013 school year in a treatment school. All told, I have data for 10,360 students who took the ISAT mathematics exam in 2012-2013, 10,343 who took the ISAT reading exam in 2012-2013, and 17,432 students who had attendance information.[3] While the unit of observation is the student, because I am assigning student treatment to the school where they were attending on the 20th day of school, regardless of how long students remain in the school, the effective unit of analysis is the school level, and standard errors are adjusted to account for this fact.

The most interpretable way to compare across students in this context is to construct a dichotomous variable reflecting whether the student's ISAT exam "meets or exceeds" standards for the subject and grade; this was true for around 45 percent of the comparison group in mathematics in 2012-2013 and around 41 percent of the

[2] It would be ideal if I had access to school assignment before classes began in the school year, or on the first day of class, but day 20 school assignment is the first official measure of school assignment provided to me by CPS. As a check, if I assign treatment based on the school that students attended the prior year, in 2011-2012, the results are somewhat less precise but still follow the same patterns. I therefore am convinced that the results presented herein are not driven by school choices that may have taken place between initial school assignment and the 20th day of school in 2012-2013.

[3] The ISAT is administered in grades three through eight only.

comparison group in reading in 2012-2013. Because CPS provided me with the raw scale scores for students beginning the 2012-2013 academic year in the 47 schools in this study, it is also possible to estimate the models with the raw scale score or with a z-scale constructed from standardizing the scores of all students within the sample, but neither of these approaches are as easily interpretable as the dichotomous dependent variable approach taken. That said, the estimates are qualitatively similar regardless of the approach taken[4]

One complicating factor in the analysis is that during the 2013-2014 school year, CPS announced that it would close 50 schools, an event that led to considerable student, family and school disruption. Two of the treatment schools in the experiment, and one control school in the experiment were announced as closing schools[5] Because the school closing announcements occurred midway through the school year and well before the timing of the state's high-stakes standardized test, the ISAT, my preferred model specification is to exclude students initially assigned to closing schools from the analysis, and the comparisons of treatments in Table 1 (page 8) exclude these students. However, in the results tables that follow, I show the estimated effects of starting in a school initially assigned to the CIS of Chicago treatment from specifications that in turn exclude and include the schools that would be identified as closing.

IV. Evidence of randomization and differential program takeup

I begin by exploring the differences in ultimate treatment probabilities between students who were attending treatment schools on the 20th day of the 2012-2013 academic year and those who were attending comparison schools at the same time. Table 1 (page 8) presents evidence along these lines. One observes that while 75.4 percent of students in schools assigned to the CIS of Chicago treatment spent the majority of the 2012-2013 academic year in a treatment school, not a single student assigned to the comparison group spent the majority of the 2012-2013 academic year in a treatment school. Even the next year the differences remain at about the same level: 74.8 percent of students initially attending schools assigned to the CIS of Chicago treatment began the 2013-2014 academic year in a treatment school, while only 0.5 percent of students initially attending schools assigned to the comparison group began the 2013-2014 academic year in a treatment school.

I next turn to an assessment of whether the students in schools that were assigned to the treatment and comparison conditions are balanced in terms of pretreatment characteristics. Table 2 (page 9) presents these comparisons in terms of the set of covariates provided by CPS: student race/ethnicity, free lunch participation, special education participation, English as a second language status, a dichotomous variable for whether the student is new to the school in 2012-2013, age in months on September 1, 2012, whether the student was born between September and February (and therefore in the older half of their "typical" grade cohort), and whether the student met the ISAT proficiency standards in reading and mathematics in 2011-2012. While there are some observable differences across the treatment categories (notably, treatment schools tended to serve more black students and fewer Hispanic students), along all ten dimensions there is no statistically significant difference in pretreatment characteristics between students assigned to treatment and students assigned to control.[6] These results therefore provide some evidence that the randomization resulted in equivalent groups for comparison.

[4] For example, the intent-to-treat point estimate for mathematics proficiency in 2012-2013 is 0.032, suggesting that assignment to CIS of Chicago participation increased the likelihood that a student would be proficient in math by 7.1 percent over the comparison group mean of 0.448, and the corresponding point estimate when the dependent variable is measured as a standardized score is 0.046, or one-twentieth of a standard deviation increase in mathematics scores.

[5] The two treatment schools that were closed represent 6.0 percent of the initial student body in the treatment condition, while the one control group school that closed represents just 0.2 percent of the initial student body in the comparison condition. An investigation of historical data indicates that the control school that closed rarely registered many students at the time of the 20th day of the academic year, and 2012-2013 was apparently no exception to this pattern. Therefore, the choice as to whether to include or exclude closed schools affects more students in the treatment condition than in the comparison condition.

[6] In all statistical comparisons in this paper, standard errors are adjusted for clustering at the level of the student's initial school, because that is the level of randomization. The rather large differences across racial and ethnic groups are related to the relative homogeneity of schools in the study population – only 7 schools in the study had between 10 and 90 percent black students and only 13 schools in the study had between 10 and 90 percent Hispanic students.

One might also be concerned that students initially assigned to treatment and control groups might differentially leave CPS following initial assignment. Table 3 (page 10) presents evidence regarding this possibility: We observe that even though it is common for students to change schools between the beginning of school and some later point in the school year, the overwhelming majority of students remain in the CPS system. Only around three percent of students are missing from the test score data in spring 2013, for example, and the difference between treatment and control students is not statistically detectable. One year later, fewer than seven percent of students are not observed in the test score data in spring 2014, and again, there is no difference between treatment and control students. I conclude from this analysis that it is unlikely that attrition from the analysis affects the findings reported in this paper.

One topic that is not related to internal validity per se involves the likelihood that a student would remain in CPS, and therefore stay in the study, but no longer be at the initially attended school. We observe in Table 3 (page 10) that there are modest differences across the treatment and control groups along this measure, with treatment students being somewhat more likely than control students (16.4 percent versus 12.0 percent, statistically significant at the 12.2 percent level) to change CPS schools between the first and second year of the analysis. In the analysis that follows, I treat all students as having their initial "treatment" in the second year, regardless of whether they remained in their initial school.

V. Results

Table 4 (page 11) presents estimates of the average effect of attending a school that participates in the CIS of Chicago partnership program on reading and mathematics proficiency and on attendance. The first row presents intent-to-treat findings. As can be seen, in the first year of the program, school participation in CIS of Chicago boosted the math proficiency rate by 3.2 percentage points (7.1 percent of the comparison mean) and the reading proficiency rate by 4.3 percentage points (10.4 percent of the comparison mean). By the second year of the program, school participation in CIS of Chicago boosted the math proficiency rate by 5.6 percentage points (11.9 percent of the comparison mean) and the reading proficiency rate by 4.3 percentage points (10.8 percent of the comparison mean).[7] The pattern is less consistent with regard to attendance rates: School participation in CIS of Chicago increased the likelihood that a student attends school at least 95 percent of the time (the district's definition of not being truant) by 3.0 percentage points (4.8 percent of the comparison mean) in 2012-2013, but the estimated effect is no longer statistically significant (and the point estimate is actually negative) in 2013-2014.[8]

The results are fundamentally the same when students assigned to closed schools are included in the analysis. As seen in the second row of Table 4 (page 11), the test score results are modestly smaller and the 2012-2013 attendance results are modestly larger than those excluding the closed schools. Because the closed schools did not offer the same treatment and those that did not close, and because of the considerable disruption associated with the school closing process, my preferred estimates are those that exclude the closed schools from the analysis, and the heterogeneity analyses reported below therefore exclude the closed schools.

The third row of Table 4 (page 11) reports treatment-on-the-treated estimates of the effects of school participation in the CIS of Chicago partnership program. TOT estimates of the effect of CIS of Chicago participation on mathematics proficiency are 9.5 percent of the traditional mean in 2012-2013 and 15.7 percent of the traditional mean in 2013-2014. TOT estimates of the effect of CIS of Chicago participation on reading proficiency are 13.8 percent of the traditional mean in 2012-2013 and 14.4 percent of the traditional mean in 2013-2014.

Figure 1 (page 13) presents essentially the same relationships in graphical form for students who are

[7] There is no appreciable difference in the rate of missing test scores in 2012-2013 or 2013-2014 by treatment category. 3.7 percent of the treatment students for whom I expected a test were missing test scores in 2012-2013, as compared to 3.0 percent of control students (p=0.271). In 2013-2014, the figures are 6.8 percent and 6.9 percent, respectively (p=0.971).

[8] If one instead measures attendance by attendance rate instead of the 95 percent attendance threshold, one finds ITT estimates of increased rates of attendance of 0.36 to 0.40 percent of the school year (standard error of 0.22 in both cases), depending on the treatment of closed schools, and TOT estimates of 0.48 percent of the school year (standard error of 0.29).

observed with test scores in all four years between 2010-2011 (two years before treatment) through 2013-2014 (second year of program participation). The two lines represent differences in proficiency rates in math and reading, with no controls for background variables (note that the same students are present in all four years.)[9] As can be seen, proficiency rates were virtually identical in reading in the years before treatment, with treatment students having 0.2 percentage points lower proficiency rates than control students in 2010-2011 and 0.3 percentage points lower proficiency rates in 2011-2012. In math, treatment students performed slightly worse than control students before the treatment occurred – with 1.8 percentage points lower proficiency rates in 2010-2011 and 1.6 percentage points lower proficiency in 2011-2012. In the first year of treatment, however, a positive gap opened up between treatment and control students: Treatment students had 4.4 percentage points higher proficiency rates in reading and 3.2 percentage points higher proficiency rates in math in 2012-2013, and 3.5 and 4.8 percentage points higher proficiency rates, respectively, in reading.

While there are not statistically significant differences in background characteristics between treatment and control schools, some differences are meaningful in magnitude. Therefore, it is especially worthwhile to observe the degree to which there are subgroup differences in the estimates effects of CIS of Chicago participation. Table 5 (page 12) presents estimate effects of school participation in CIS of Chicago for a variety of subgroups – black students, Hispanic students, free lunch recipients, those new to the school in 2012-2013 at the beginning of the partnership program, those relatively old for their grade (born September to February) and those relatively young for their grade (born March to August). The results are generally relatively consistent across subgroups. The test score results are particularly strong for black students and those new to the school in 2012-2013, but these groups do not observe particularly strong estimated effects for attendance. Therefore, while the test score results appear to be particularly driven by some groups, the differences are rarely extremely large.

VI. External validity

While the evidence suggests that school participation in the CIS of Chicago partnership program was beneficial for the students in those schools, there is an open question regarding external validity. Specifically, in order to be part of the experiment, a school both had to apply for inclusion as well as to be selected as part of a process based in part on need. It is therefore unsurprising that schools participating in the experiment serve somewhat needier populations than the typical school in CPS. That said, the differences are not extremely stark: The average school in the experiment ranks at the 55th percentile districtwide in terms of percentage low-income, the 54th percentile districtwide in terms of student mobility rates, the 58th percentile districtwide in terms of Census poverty rates, and the 50th percentile districtwide in terms of the percentage of students with individualized education plans (IEPs). The pool of applicants overall, including those not chosen to participate in the treatment or control group, is even slightly more representative of the district as a whole, with the average school applying to be part of the program ranking at the 49th percentile districtwide in terms of percentage low-income, the 53rd percentile districtwide in terms of student mobility rates, the 58th percentile districtwide in terms of Census poverty rates, and the 50th percentile in terms of fraction of students with an IEP.

The fact that participating schools are not extremely dissimilar from the district as a whole speaks to the fact that most schools in CPS serve a relatively disadvantaged population from a broader perspective, and also that a disproportionate share of the neediest schools in CPS were already being served by CIS of Chicago, suggesting that these results might be particularly relevant for schools serving needy but not extremely needy populations. Of course, it's still very possible that these results would be different if the program were randomly allocated to all schools in CPS because only a set of particularly motivated schools applied to be part of the program, and CIS of Chicago further selected based in part on motivation and school capacity. It stands to reason that the program works better for schools that are relatively invested in participation, but we have no way of gauging this possibility empirically.

[9] As before, these are akin to "intention to treat" estimates, because I assign treatment to students enrolled in treatment schools on the 20th day of the 2012-2013 school year, rather to those who spent the full year or the majority of the year in a treatment school.

VII. Conclusion

This paper presents experimental evidence that school participation in the partnership program of Communities In Schools of Chicago led to marked and lasting improvements in reading and mathematics proficiency rates, and increases in attendance rates in the first year in the program that do not persist into the second year of the program. The results are present across a range of groups of students, and appear to be particularly pronounced for black students and those who are new to the school in the first year of the program. While it is impossible to measure long-term outcomes, such as high school graduation, and the data do not permit me to study some important short-term outcomes, like student behavior or classroom grades, the results indicate that this program, that was largely aimed at boosting students' social and emotional skills, also leads to improvements in academic outcomes in a population of students for whom fewer than half of the population are proficient. Given the very low cost of the program (below $50 per served student per year), the results suggest that the CIS of Chicago partnership program produces very large gains in student outcomes per dollar spent.

Table 1: ***Difference in treatment between assigned treatment and control groups***

Measure of treatment	Treatment group	Control group	p-value of difference
In treatment school on day 20 of 2012-2013	1.000	0.000	0.000
Spent majority of 2012-2013 in a treatment school	0.754	0.000	0,000
In treatment school on day 20 of 2013-2014	0.748	0.005	0.000

Notes: Schools announced for closure in winter during the 2012-2013 academic year are excluded from this table. Students are assigned to the school in which they were enrolled on day 20 of the 2012-2013 academic year. There were 18 treatment schools and 26 control schools that were not selected for closure by the Chicago Public Schools. The results table displays the difference in estimated effects of program participation depending on treatment of closed schools. Standard errors are clustered at the initial assigned school level.

Table 2: ***Pretreatment covariate balance between assigned treatment and control groups***

Pretreatment Characteristic	Treatment group	Control group	p-value of difference
Black	0.392	0.253	0.381
Hispanic	0.459	0.642	0.260
Free lunch recipient	0.837	0.853	0.715
Special education participant	0.120	0.125	0.588
English as a second language	0.256	0.289	0.628
New to school in 2012-2013	0.259	0.227	0.117
Age (months) on September 1, 2012	107.5	110.4	0.297
Born September to February	0.483	0.484	0.320
Met ISAT proficiency standard in reading in 2011-2012	0.645	0.628	0.682
Met ISAT proficiency standard in math in 2011-2012	0.779	0.765	0.659

Notes: Schools announced for closure in winter during the 2012-2013 academic year are excluded from this table. Students are assigned to the school in which they were enrolled on day 20 of the 2012-2013 academic year. There were 18 treatment schools and 26 control schools that were not selected for closure by the Chicago Public Schools. The results table displays the difference in estimated effects of program participation depending on treatment of closed schools. Standard errors are clustered at the initial assigned school level.

Table 3: *Differential attrition by assigned treatment status*

Measure of attrition	Treatment group	Control group	p-value of difference
No CPS test scores observed in spring 2013	0.034	0.028	0.376
No CPS test scores observed in spring 2014	0.066	0.067	0.915
Changed CPS school between 2012-13 and 2013-2014	0.164	0.120	0.122

Notes: Schools announced for closure in winter during the 2012-2013 academic year are excluded from this table. Students are assigned to the school in which they were enrolled on day 20 of the 2012-2013 academic year. There were 18 treatment schools and 26 control schools that were not selected for closure by the Chicago Public Schools. The results table displays the difference in estimated effects of program participation depending on treatment of closed schools. Standard errors are clustered at the initial assigned school level.

Table 4: ***Estimated mean effects of participation in Communities In Schools of Chicago partnership program***

Specification	Outcome measure					
	Proficient in math 2012-2013	Proficient in reading 2012-2013	Proficient in math 2013-2014	Proficient in reading 2013-2014	Attend 95% in 2012-2013	Attend 95% in 2013-2014
Control mean	0.448	0.414	0.473	0.397	0.605	0.639
Intent to treat — exclude closed schools	0.32* (0.018)	0.043*** (0.012)	0.056* (0.030)	0.043** (0.019)	0.030* (0.018)	-0.008 (0.017)
Intent to treat — include closed schools	0.025 (0.018)	0.038*** (0.016)	0.051* (0.029)	0.038** (0.019)	0.033* (0.017)	-0.009 (0.016)
Treatment on treated — exclude closed schools	0.042* (0.024)	0.057*** (0.016)	0.074* (0.040)	0.057** (0.025)	0.040* (0.024)	-0.011 (0.023)
Number of students (excluding closed schools)	10,360	10,343	10,072	10,036	17,432	16,596

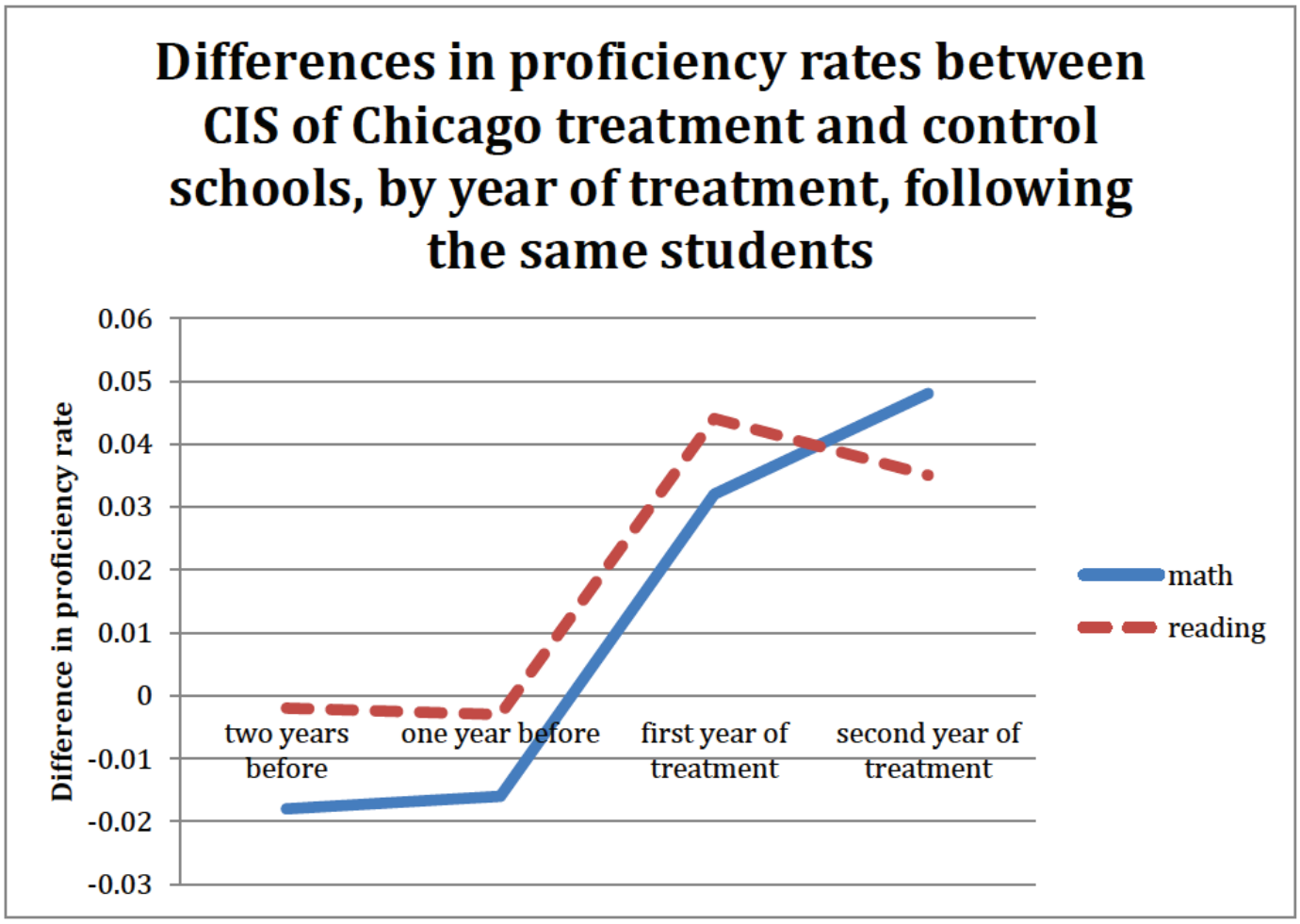
Notes: Schools announced for closure in winter during the 2012-2013 academic year are excluded from this table. Students are assigned to the school in which they were enrolled on day 20 of the 2012-2013 academic year. There were 18 treatment schools and 26 control schools that were not selected for closure by the Chicago Public Schools. Note that 6.0 percent of students initially assigned to treatment schools were in schools later slated to close, while only 0.2 percent of students initially assigned to control schools were in schools later slated to close. Models control for 2010-2011 and 2011-2012 reading and math scores and attendance rates, when available, and missing data flags when not, free lunch status, race/ethnicity, grade, and whether the student is new to the assigned school in 2012-2013. Standard errors are clustered at the initial assigned school level. Coefficients marked *, **, and *** are statistically significant at the 10, 5, and 1 percent levels, respectively.

Table 5: ***Heterogeneity in estimated effects of participation in Communities In Schools of Chicago partnership program: Intent-to-treat estimates***

Subgroup	Outcome measure					
	Proficient in math 2012-2013	**Proficient in reading 2012-2013**	**Proficient in math 2013-2014**	**Proficient in reading 2013-2014**	**Attend 95% in 2012-2013**	**Attend 95% in 2013-2014**
Full sample	0.032* (0.018) [10,360]	0.043*** (0.012) [10,343]	0.056* (0.030) [10,072]	0.043** (0.019) [10,036]	0.030* (0.018) [17,432]	-0.008 (0.017) [16,595]
Black	0.061* (0.030) [3,219]	0.062** (0.026) [3,225]	0.114** (0.046) [2,989]	0.063* (0.037) [2,971]	0.014 (0.036) [5,312]	0.011 (0.023) [4,938]
Hispanic	0.016 (0.027) [5,758]	0.029** (0.012) [5,756]	0.029 (0.039) [5,780]	0.043** (0.018) [5,765]	0.038* (0.019) [9,698]	-0.039* (0.023) [9,410]
Free lunch recipients	0.034* (0.019) [8,821]	0.046*** (0.013) [8,809]	0.060* (0.035) [1,501]	0.047** (0.019) [8,591]	0.032 (0.019) [14,813]	-0.008 (0.018) [14,095]
New to school in 2012-2013	0.090*** (0.033) [1,463]	0.107*** (0.027) [1,418]	0.076** (0.035) [1,501]	0.055* (0.030) [1,481]	0.026 (0.035) [14,813]	-0.008 (0.018) [14,095]
Born September to February	0.033* (0.019) [5,104]	0.047*** (0.027) [1,418]	0.076** (0.035) [1,501]	0.055* (0.030) [1,481]	0.026 (0.035) [4,106]	0.002 (0.025) [3,918]
Born March to August	0.031 (0.020) [5,256]	0.039** (0.016) [5,251]	0.056* (0.031) [5,147]	0.049** (0.021) [5,128]	0.025 (0.017) [8,937]	-0.008 (0.017) [8,533]

Notes: Schools announced for closure in winter during the 2012-2013 academic year are excluded from this table. Students are assigned to the school in which they were enrolled on day 20 of the 2012-2013 academic year. There were 18 treatment schools and 26 control schools that were not selected for closure by the Chicago Public Schools. Note that 6.0 percent of students initially assigned to treatment schools were in schools later slated to close, while only 0.2 percent of students initially assigned to control schools were in schools later slated to close. Models control for 2010-2011 and 2011-2012 reading and math scores and attendance rates, when available, and missing data flags when not, free lunch status, race/ethnicity, grade, and whether the student is new to the assigned school in 2012-2013. Standard errors are clustered at the initial assigned school level. Coefficients marked *, **, and *** are statistically significant at the 10, 5, and 1 percent levels, respectively.

Figure 1:



Notes: This figure presents the difference in proficiency rates between students the treatment and control schools for students who were observed with test scores in all four years of the analysis: 2010-2011 (two years before), 2011-2012 (one year before), 2012-2013 (first year of treatment), and 2013-2014 (second year of treatment). Schools announced for closure in winter during the 2012-2013 academic year are excluded from this table. Students are assigned to the school in which they were enrolled on day 20 of the 2012-2013 academic year. There were 18 treatment schools and 26 control schools that were not selected for closure by the Chicago Public Schools. Note that 6.0 percent of students initially assigned to treatment schools were in schools later slated to close, while only 0.2 percent of students initially assigned to control schools were in schools later slated to close.