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The Effect of Mentoring on School Attendance and Academic Outcomes: A Randomized Evaluation of the Check & Connect Program

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Abstract

The researchers present the results of a four-year randomized controlled trial evaluation of a structured student monitoring and mentoring program that aimed to increase student attendance. The program, called Check & Connect (C&C), was delivered to 765 students in grades 1 through 8 in 23 neighborhood schools in the Chicago Public Schools (CPS). C&C mentors were full-time employees and had caseloads of between 30 and 35 students. Each student was assigned a mentor for two school years, and the program was delivered to two cohorts of students over the 2011-12 to 2014-15 academic years. Mentors tracked data to monitor the attendance and academic progress of the 30 to 35 students on their caseload. Mentors also met regularly with students and delivered personalized interventions designed to increase students' attendance and engagement with school. Based on estimates of treatment on the treated (TOT), they find that participation decreased student absences among students who began the program in grades 5-7 by a statistically significant 3.4 days, or 20.2 percent relative to the control complier mean. The researchers do not find statistically significant effects of participating in C&C among students who began the program in grades 1-4. For both cohorts, the effect of participating in C&C was larger in the second year of the intervention than the first, though that difference was not statistically significant, which is at least suggestive of the possibility that the development of relationships between the mentor and student may be an important mechanism through which the mentoring program is effective. The researchers did not find significant effects on grade point average, but did find a statistically significant decline in courses failed. There were mixed results for test scores, but no evidence that test scores increased significantly.

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1. Introduction

One of the most important social policy priorities in the United States is to improve high school graduation rates for disadvantaged urban youth. During the 2009-10 school year, each of the nation's 10 largest public school districts had a graduation rate below 75 percent (Common Core of Data, 2010). In Chicago, the site of our project, the four-year graduation rate is 70 percent overall (Common Core of Data, 2010) and about 57 percent for African American males (Healey, Nagaoka, & Michelman, 2014). Despite a modest increase in graduation rates in recent years, the high school graduation rate today in the U.S, is not that much different from what it was 40 years ago, even though the returns to education have grown substantially (Heckman and LaFontaine, 2007; Goldin & Katz, 2010; Murnane, 2013). Given the strong relationship between graduation and a wide range of life outcomes such as crime involvement, health, and earnings (Card, 1999; Lochner & Moretti, 2004; Cutler & Lleras-Muney, 2006), inequality in graduation rates contributes to inequality in many other domains of American life as well.

While the decision to drop out of high school has received a great deal of attention, the problems that lead to dropout almost always start much earlier: with chronic school absences, or truancy. In Chicago, the site of the project we report on here, fully 39 percent of 9th graders and 50-55 percent of 10th-12th graders missed at least 10 percent of school days, a threshold considered chronically absent, during the 2011-12 school year (Allensworth et al., 2013). This translates to at least 17 days of school, or more than three weeks. In addition, 12.9 percent of elementary students—and 20.4 percent of African American elementary students—missed over four weeks of school (Jackson, Marx, & Richards, 2012). Similar truancy patterns can be found in almost every major urban school system. Chronic absenteeism in early grades has been found to be predictive of high school dropout (Schoeneberger, 2011; Cook, Crowley, Dodge, & Gearing, 2016).[1]

Despite the central role of truancy in contributing to the educational problems of disadvantaged urban students, very little is known about the modifiable risk and protective factors that contribute to truancy, and even less is known about effective remedies. While school districts have developed a wide range of policies and administrative systems to enforce truancy laws since schooling became compulsory in the late 19th century, almost none of these efforts

[1] Truancy is also associated with drug and alcohol use, early initiation of sexual activity, teenage pregnancy, and crime (Allensworth and Easton, 2007; Hallfors et al., 2002; Tait, 2004; Dryfoos, 1990; Huizinga & Jacob-Chien, 1998). For example data from Miami suggest truants are responsible for 71 percent of all youth crime (U.S. Department of Education, 1996).

have ever been subject to rigorous evaluation. Almost all of the research in this area is observational and may confound the causal effects of truancy prevention programs with those of hard-to-measure attributes associated with either program implementation decisions (for school- or district-level analyses) or selection into program participation (for student-level analyses).

Perhaps partly in response to this lack of understanding of the value of truancy prevention, such efforts often receive low priority in education policy decisions. For example, in 1972 Chicago employed 290 truancy officers to serve the city's roughly 600 elementary, middle and high schools. Responding to budget pressures in 1991, in order to save $4 million the Chicago school board reduced the number of truancy officers to precisely zero.[2]

The contribution of this paper is to carry out one of the few large-scale randomized controlled trials (RCTs) of a promising intervention to reduce truancy, which focuses on one important risk / protective factor identified by previous observational studies: social capital. Dating back at least to Coleman (1988), social scientists have been aware that the level of support children have from adults is strongly correlated with a wide range of different schooling outcomes. In many of our nation's most distressed urban areas it is challenging for adults to invest as much time and attention in children's outcomes as might be required, because poverty, irregular (or long) work schedules, crime, transportation problems, child care challenges, and untreated mental or physical health problems make doing so difficult. All of these problems may be exacerbated when households have just a single adult that must handle all responsibilities as both parent and provider.

The specific intervention we test seeks to supplement and support the social capital that parents can provide, by randomly assigning some children but not others within the Chicago Public Schools (CPS) to receive a structured mentoring program focused specifically on reducing truancy and improving student engagement in school. The program we study, Check & Connect (C&C), is a school-based structured mentoring program that is designed to promote student engagement through relationship building, problem solving, and persistence for marginalized students. C&C has four components: (1) a mentor who works with individual students and their families, (2) regular checks by the mentor, (3) timely personalized interventions to reestablish student connection to school and learning, and (4) engagement with parents.

[2] Similar financial restrictions have led to reduced numbers of truancy officers in Santa Rosa, CA and Las Vegas (Los Angeles Times, 2003; Las Vegas Review-Journal, 2009).

Two cohorts of students were assigned to the program to determine its impact on student attendance and achievement. The first cohort consisted of 487 students in 23 randomly selected elementary schools on the south and west sides of Chicago, who received C&C services during the 2011-12 and 2012-13 school years. The second cohort included 348 students in 9 of the original 23 schools who received C&C services during the 2013-14 and 2014-15 school years; 70 of the participating students in the second cohort had been randomly selected from among the treatment students from the first cohort. In general, students were eligible to receive C&C services if they had between 10 and 35 absences in the previous school year. A more detailed description of the sample selection and eligibility criteria is included later in the paper. Randomization occurred at the student, grade, and school level, and we used student-level, longitudinal administrative data collected by CPS to measure the program's impact on attendance, grades, and standardized tests scores.

Our findings suggest that C&C significantly reduced absences for middle school-aged students, but not for elementary school-aged students. Based on estimates of the effect of the treatment on the treated (TOT), we find that participation in C&C decreased student absences among students who began the program in grades 5-7 by a statistically significant 3.4 days, or 20.2 percent relative to the control complier mean. We do not find statistically significant effects of participating in C&C among students who began the program in grades 1-4. For both cohorts, the effect of participating in C&C was larger in the second year of the intervention than the first. While this difference was not statistically significant, this provides at least a suggestion that the development of relationships between the mentor and student may be an important mechanism through which the mentoring program is effective. We did not find significant effects of C&C on grade point average, or any consistent effects on achievement test scores. However, we did find that for the group of students who were in grades 5-7 in the first year of the program C&C caused a reduction in course failures of 0.17 courses, which is a 20.2 percent reduction relative to the control complier mean.

While C&C was effective in that it improved key outcomes for a target population of students, it is useful to compare C&C to other interventions that have an impact on student engagement outcomes, as interventions vary in cost, complexity of implementation, and the mechanisms through which they affect change in student behavior and outcomes. As implemented in this project, the C&C program cost about $1700 per student per year. This translates to a cost of approximately $500 per incremental day of attendance brought about by the intervention for the 5th-7th grade students.

By way of comparison, recent interventions in social policy have attempted to leverage insights from behavioral science through providing information, or "nudges", to affect individuals' behavior. This sort of intervention has the benefit of being automatized, low-cost, and scalable. For example, Rogers and Feller (2016) find that a mail-based intervention providing parents of frequently-absent students with information about their students' attendance record decreases absences by about one day at a cost of $5 per additional attendance-day. It is clear this type of intervention operates through a different set of mechanisms than a more "heavy-touch", person-centered intervention like C&C, which suggests that a broader set of outcomes besides attendance may be required for evaluating relative effectiveness. Other intensive interventions with costs similar to C&C have been shown to be effective at improving student engagement even when school attendance was not the primary focus of the intervention (Heller et al., 2015) and to simultaneously produce large gains in other outcomes like school engagement, high school graduation and delinquency. It may ultimately turn out that for very disadvantaged student populations of the sort we study in our Chicago context there may be decreasing but then increasing returns to program intensity for the problems of attendance and school disengagement. This is an important hypothesis for future research to examine.

The next section reviews previous studies and relevant literature on the causes of absenteeism and attempts to combat it. Section three provides a detailed description of the C&C model. Section four discusses our experimental design including a description of how randomization was carried out for each cohort. Section five reviews our data for this study, as well as descriptive statistics and balance tests. Section six describes the analysis plan. The results are discussed in section seven, and we conclude in section eight.

2. Prior studies

In order to understand what policies and programs might most effectively reduce student absenteeism, it is helpful to first understand what causes it. Balfanz and Byrnes (2012) categorize absent students by their decisions and agency to attend school, distinguishing between those who cannot attend (due to illness or housing instability), those who refuse to attend (to avoid bullying or unsafe conditions), and those who choose not to attend (because they are uninterested in school). The risk and protective factors that contribute to students falling into different categories can reside within the school or within the family or community (see for example Chang and Romero, 2008).

It must surely be true that in many cases the different factors that contribute to student absenteeism are complex and interact. For example a child with a sick younger sibling may stay home to provide care if the parent is unable to get off work to provide care themselves. Many affluent parents who have the advantage of working in more accommodating jobs, or being able to afford paid child care, would be able to send the child to school in the same case of sibling illness. Should we attribute the absence then to illness, or workplace problems, or unaffordable child care, or what exactly?

Recognizing these complexities, much of the research that has been done around student absenteeism argues that illness or health issues are the primary barrier (Ehrlich, Gwynne, Pareja, & Allensworth, 2014; Kearney, 2008). This conclusion is often drawn from information collected through school administrative data, which may not be designed to detect more nuanced factors driving student absenteeism, especially those that occur outside of the school setting. However, further analyses reveal the significant role that out-of-school economic and family circumstances may play in absenteeism. For example, a 2012 national survey found that children in single-mother families are twice as likely as children in two-parent families to report missing at least 11 days of school in the previous school year for health-related reasons (Bloom, Jones, & Freeman, 2013) and an analysis of administrative data from six states found that students living in poverty are also more likely to be chronically absent (Balfanz & Byrnes, 2012).

Similarly, absences that stem from school refusal behavior are hard to quantify, as such behavior may be indicated by tardiness or incomplete absences, the definitions of which vary by district (Kearney, 2008). One study found that truancy rates jump for students who are transitioning school levels (elementary school to middle school and middle school to high school), which may indicate absences that are driven by student reluctance or anxiety about adjusting to a new environment, peers, and schedule (Garrison, 2006).

Students attending urban schools are also more likely to miss school. In a national survey of eighth grade students, those attending urban or city schools were more likely to report being absent three or more times in the past month than eighth grade students attending rural or suburban schools (Child Trends Databank, 2015). One study found that schools in high-poverty urban areas have up to one-third of their students chronically absent (Balfanz & Byrnes, 2012).

High levels of truancy clustered around certain grades reflect a pattern of absences that seem to evolve with the age of the student. A meta-analysis of administrative data from Oregon, Nebraska, Florida, and West Virginia found that chronic absenteeism goes down in third and fourth grades before sharply increasing in middle school, especially for students in 6^{th} – 8^{th} grades (Balfanz & Byrnes, 2012). This pattern highlights that rather than being a static condition, absenteeism is often caused by multiple and shifting barriers to attendance.

Interventions to address student absenteeism often target one or only a few specific barriers to attendance. Clinical or medically-based interventions are sometimes deployed to target youth with anxiety-based problems through pharmacotherapy or cognitive-behavioral strategies. Others work to influence the out-of-school environment of the student—by providing earlier family-school engagement, alternative or after-school programs—and others have focused on providing additional professional development to teachers working with at-risk youth (Kearney, 2008). Some of these programs have been successful: a review of absenteeism-prevention programs found that alternative education programs and behavioral programs may have positive impacts on attendance, academic performance, and graduation (Klima, Miller, & Nunlist, 2009). However, many may have unintentionally adverse effects. A review of research on alternative schools found that they often have no—and sometimes negative—impacts on student engagement outcomes (Klima et al., 2009). Zero tolerance policies, put in place by many schools as way to crack down on rising absence rates, have been found to be disproportionately enforced on at-risk students, causing them to only miss more school and likely exacerbating the problems such policies were designed to address (Gage, Sugai, Lunde, & DeLoreto, 2013). Conditional cash transfers to incentivize school attendance in Colombia have been shown to be effective in improving student outcomes (Angrist, Bettinger, Bloom, King, & Kremer, 2002; Angrist, Bettinger, & Kremer 2006; Barrera-Osorio, Bertrand, Linden, & Perez-Calle, 2008). However, providing financial incentives for improved attendance seems to be less common in the United States than outside of it. The results of the Opportunity NYC conditional cash transfer program, including the limited impacts on children's attendance and other schooling outcomes, may have contributed to dampened enthusiasm for this approach in the U.S. (Riccio et al., 2013).

Many of these strategies fail to support the personal relationships that are often vital for success within a socialized system, referred to as social capital (Coleman, 1988). Social capital exists in the relations between actors and, much in the same way that physical and financial capital do, facilitates productive activity. Coleman (1988) divides social capital into three forms: obligations and expectations, information channels, and social norms. Each of these forms provides a structure that promotes action and may be targeted to support a specific behavior, like attending school. It is possible that mentoring programs designed to engage at-risk students may form and utilize social capital in a way that helps a student to change his or her behavior. Mentoring programs may develop trusted relationships that create perceived obligations by the student. Mentors may also provide information and form social norms.

There is some evidence of the effectiveness of mentoring programs. Randomized controlled trials of Big Brothers Big Sisters, a national community-based and school-based mentoring program, has found significant impacts on improving academic achievement (Grossman, Chan, Schwartz, & Rhodes, 2012; Bayer, Grossman, & DuBois, 2015; Herrera et al., 2007; Schwartz, Rhodes, Chan, & Herrera, 2011) and decreasing unexcused absences (Grossman et al., 2012; Herrera et al., 2007; Schwartz et al., 2011). Other mentoring programs have also been found to improve socioemotional outcomes, including self-reported measures of depression (Herrera, DuBois, & Grossman, 2013), peer connectedness and self-esteem (Karcher, 2008), and pro-social behavior (Schwartz et al., 2011). One challenge with these studies is that they typically rely on self-reported outcomes. This may confound the effect of the intervention on actual behavior and outcomes with the possibility that youth assigned to mentors may be less willing to report socially undesirable outcomes for fear of disappointing their mentor (known in the survey research literature as "social desirability bias").

3. The Check and Connect (C&C) program

In response to Chicago Public School concerns about truancy, and the decision several decades earlier to phase out all truancy officers for budget reasons, we visited the U.S. Department of Education's What Works Clearinghouse (WWC) for what the available evidence suggests is best practice for improving school attendance. WWC suggested one intervention that seemed particularly promising: Check & Connect, developed at the University of Minnesota by

Sandra Christenson, who is a co-author on the present paper, and other colleagues from the University of Minnesota, as well as various school personnel.

C&C is a structured mentoring program that aims to reduce the number of days that students miss school, and to increase students' engagement with academic activities when they are in school. C&C has been implemented in several school districts around the United States, beginning in the Minneapolis Public Schools. C&C typically targets students who are at risk of disengagement or dropping out of school—often measured by high rates of absenteeism or poor academic performance—and assigns them to a mentor, who is typically an in-school staff member. Mentors are asked to monitor the attendance and school performance of the students on their caseload; serve as case managers, connecting students to social service and school-based resources that the mentors think might help the student to overcome barriers to school attendance; and develop relationships with the students.

C&C is standardized in the sense that there is a manual and a set of training materials that can be used to implement the program, but it is also adaptive in the sense that mentors are encouraged to assess why different students are not coming to school and tailor the ways they intervene with students to match what they think students need.

The C&C manual and training directs mentors to support student engagement through two primary channels. The "Check" component centers on monitoring student performance—tracking attendance, grades, and referrals—for signs of disengagement. Mentors then deliver personalized interventions to students designed to boost engagement as part of the "Connect" piece. These interventions are supposed to be based on information the mentor has about the student's school engagement level and family circumstances and to be shaped around available school and community resources. Mentors are also encouraged to connect with families of students, to partner with parents to increase student engagement, and to function as liaisons between home and school. In this study, mentors met with students, one-on-one or in small groups, an average of five times a month. On average, they connected with guardians through home visits or phone twice a month, although the level of family engagement varied substantially by mentor.

Since its development, C&C has been implemented by several cities beyond Minneapolis, including Tulsa and San Diego, to support students at risk of discontinuing school or to serve youth with disabilities. Florida, Missouri, and Utah have each developed state-wide initiatives that implement C&C in schools that have a high rate of students at risk of disengagement. The program has also been delivered to juvenile offenders and post-secondary students, although these contexts have been less common.

There have been several previous studies of C&C. Specifically, randomized controlled trials of the program's effect on middle school and high school students found it improved attendance outcomes and decreased disciplinary referrals for youth who are frequently absent (Maynard, Kjellstrand, & Thompson, 2014) and supported staying in school (through higher persistence and completion rates and lower drop-out rates) for students receiving special education services (Sinclair, Christenson, & Thurlow, 2005; Sinclair, Christenson, Evelo, & Hurley, 1998). However, a separate randomized controlled trial of the program's impact on students with absence histories that were representative of the full population of high school students, implemented concurrently with part of the intervention we study but in the San Diego Unified School District, found no significant effects on student performance or engagement, including attendance and total credits earned (Heppen, Zeiser, O'Cummings, Holtzman, Christenson, & Pohl, under review). The authors of this study attribute this to the age of the students, who were in 10th grade when the intervention began; they advocate for C&C as a more effective intervention for students showing early warning signs of disengagement, rather than those who may already be disengaged. While C&C's effect on elementary school students has been evaluated in the past (Anderson, Christenson, Sinclair, Lehr, 2004; Lehr, Sinclair, & Christenson, 2004), these studies were not experimental and have relied on comparing student outcomes to baseline measures, rather than a randomized control group.

In the present study, C&C was implemented in the Chicago Public Schools (CPS) by a social service agency called SGA Youth and Family Services (SGA). Mentors were selected and hired by SGA to work as full-time C&C mentors. SGA initially hired 15 mentors to work in 23 CPS schools, and when mentors quit or were fired SGA hired replacements. SGA also employed a full-time project manager who served as the supervisor of the C&C mentors. The SGA project manager oversaw the work of the mentors, organized and led weekly meetings of the mentors, and provided guidance and feedback to mentors about how to work most effectively with the students. In addition, a project manager within CPS oversaw the implementation of the C&C program, oversaw the SGA project manager, and helped to collect data on participation and implementation. Once or twice each year, consultants from the research team at the University of

Minnesota conducted training sessions with the C&C mentors to provide professional development and guidance on how to implement the C&C program with fidelity.

Based on this previous work and conversations with Chicago Public Schools about the context and characteristics of the students to be targeted for intervention, the current study involved intervention for two years. Two cohorts of students received Check & Connect services for two years each, and a small subset of students received the program for all four years. This research design enables further exploration of how duration of intervention affects outcomes.

4. Experimental design

The study was carried out in the Chicago Public Schools (CPS) over four school years. Two cohorts of students were offered the chance to participate in the program, each cohort lasting two school years. The cohort 1 intervention took place during the 2011-12 and 2012-13 school years, and the cohort 2 intervention took place during the 2013-14 and 2014-15 school years. Students who participated were assigned a C&C mentor and remained with that mentor for the full two years unless the mentor quit or was fired, or if the student moved too far away for it to be feasible for the mentor to continue providing services to the student.

A. Cohort 1 random site selection and random assignment

For cohort 1, random assignment took place in three steps. First, in collaboration with CPS, we went through a process of choosing 69 schools serving grades K-8 to be a part of the study. We tried to choose schools that were broadly representative of the district in terms of the demographic and socioeconomic characteristics of the students, and we wanted to ensure that the schools had a large enough group of students in each grade with absences in the range of 10-35 in the prior year. We also took geographic location into consideration because in cohort 1, the C&C mentors were to be initially assigned to two schools each. We wanted to ensure that these schools were geographically close enough together to enable mentors to travel back and forth between them regularly. The 69 selected schools are mostly on the south and west sides of Chicago, in neighborhoods that range from some of the very poorest in the city and the country, to some with moderate poverty levels. The free or reduced price lunch rates, which is commonly used as a proxy for school-level poverty rates, in 2010-11 for the schools that were selected for the cohort 1 study ranged from 71.2 to 99.8 percent. All but two of the study schools had free or reduced price lunch rates above 90 percent in 2010-11, and the median school-level free or reduced price lunch rate was 97.2 percent.

We placed those 69 schools into groups of three, matching on geographic location, student race and ethnic demographics, and school-level absence rates, and randomly selected one school from within each group of three to be in the cohort 1 study. We conducted this first round of school-level random selection to allow for estimates of spillover effects on control students within the schools where C&C was implemented.

Within those 69 schools, students in grades 1-7 who had between 10 and 35 absences in the prior year were eligible to be selected for the offer to participate in the C&C program. We chose not to offer the program to students who would be in 8th grade in the first year of the study because the program was planned to last for two school years, and we thought it would be logistically difficult for mentors to follow students from their elementary school to a high school.[3]

Within each of the 23 cohort 1 study schools, we first randomly selected five of the seven grades to offer the program. The remaining two grades would be in the control group, and were intended to help identify spillover effects under the assumption that spillover effects might be more pronounced within grades than across grades. Among the five selected grades we then placed students into groups of three, matched based on baseline absences, and randomly selected one of the three students to be offered treatment. In the main analyses reported below, we ignore the grouping of students into triples because within each student triple the probability of being selected for treatment was constant at 1/3. The triple fixed effects are therefore uncorrelated with treatment assignment, and omitting them should not cause bias. We report results including triple fixed effects in an appendix table.

The students selected for treatment were sorted within school and grade in descending order based on baseline absences, and were approached and offered the chance to participate in C&C in that order. In both the intent to treat (ITT) and treatment on the treated (TOT) analyses, we include all students randomly selected for treatment regardless of whether they were approached and offered the chance to participate. Thus, the ordering of students for the offer of treatment does not bias the ITT or TOT. The ordering did induce students with higher baseline

[3] In CPS, the K-8 schools are called elementary schools even though they include students in grades that would commonly be in middle schools or junior high schools. Most elementary schools in CPS include grades K-8, and most high schools include grades 9-12. There are very few middle schools in CPS.

absence rates to be more likely to be compliers, which means that if there are heterogeneous treatment effects the TOT estimates are for students with baseline absence rates somewhat towards the higher end of the 10 to 35 range.

From the list of students randomly selected for treatment, students were offered the chance to participate until the caseload for the mentor serving each school was filled. Schools were put into two categories based on the school's enrollment. A total of 15 mentors were either assigned to one larger school – large enough to support a caseload of 30 plus a comparison group – or to two smaller schools. Mentors assigned to a larger school began the school year with a caseload of 30 students all in a single school, while mentors assigned to two schools began with caseloads of 15 in each of the two schools for a total of 30 students.

B. Cohort 2 random site selection and random assignment

In the summer between the end of cohort 1 and the beginning of cohort 2, we conducted a second round of random assignment. There was enough funding to support nine mentors, and based on feedback from the mentors and from CPS, we decided to assign all mentors to begin at a single school for cohort 2. Nine of the 15 mentors were invited to continue. Of the original 23 schools in the cohort 1 study, several were closed as a part of school closings that occurred at the end of the 2012-13 school year. The nine schools where the nine returning mentors primarily worked were selected to continue the program for cohort 2.

Within the cohort 2 study schools, we placed students into five randomization blocks. Two of the randomization blocks were set aside for students who had been in the cohort 1 study, one block for students who had been assigned to treatment in cohort 1, and one for students who had been assigned to control in cohort 1. This will allow us to experimentally test whether getting four years of participation generates larger effects than two years of participation. Since the program was offered to students who were in grades 1-7 in the first year of cohort 1, students in both cohort 1 and cohort 2 studies were in grades 1-5 in the first year of cohort 1, and in grades 3-7 in the first year of cohort 2. To fill in the two earlier grades, we created a randomization block of students who were in 1st and 2nd grade in the first year of cohort 2. We also created a block of students who were new to the cohort 2 schools since the randomization for cohort 1. Since the probability of selection into treatment was not equal across all randomization blocks in cohort 2, we include randomization block fixed effects in all cohort 2 models. Finally, we offered principals at the schools the opportunity to nominate students to be in the study. Students nominated by principals were placed in their own randomization block within each school and subject to random assignment.[4]

5. Data, descriptive statistics, and tests of baseline balance

In this section we describe the student-level school records we use to measure baseline characteristics and outcomes, document the level of disadvantage among the students in our study sample, and confirm that random assignment appears to have been carried out correctly.

A. Data

The data for this study are drawn from longitudinal student-level records from the Chicago Public Schools (CPS) for the 2010-2011 through the 2014-2015 school years, and program participation data collected by C&C mentors and a CPS project manager. The CPS data include demographics, attendance, enrollment, misconduct, and achievement outcomes. The demographic data include each student's birth date, race / ethnicity, eligibility for free and reduced price lunch, and an indicator for having a learning disability (indicated by having an Individualized Education Plan (IEP)).

Because the primary focus of C&C is to reduce student absences, measures of attendance and absences are the primary outcome variables in the analyses. The data include measures of attendance and absences: days present, meaning the number of enrolled days a student attended school over the school year; days absent, meaning the number of enrolled days a student was absent over the school year; percent present, meaning the percentage of enrolled days a student was present over the school year; and membership days, meaning the total number of days the student was officially enrolled in a CPS school. Membership days are the sum of days present and days absent, but do not necessarily equal the total number of school days in the CPS school year. Students can move in the middle of the school year and leave CPS, and it is also possible that students might not accumulate membership days for a short period when they transfer from one school to another within CPS. For this reason, we present estimates on days present, days absent, and membership days separately.

[4] In a few cases, principals submitted lists of nominated students after randomization had already taken place for their school. In these cases, we followed the initial random assignment for nominated students who were already subject to random assignment in one of the other randomization blocks, and then created a randomization block consisting of the nominated students who were not in one of the other randomization blocks.

Our analyses of achievement outcomes use annual grade point average (GPA) and math and reading test scores. The test score data come from two different sources because CPS administered multiple tests for elementary school students over the study period. The first test for which we have reading and math test score data is the Illinois Standards Achievement Test (ISAT), which was state-mandated during the 2011-12, 2012-13, and 2013-14 school years. The ISAT was administered in math and reading among 3rd through 8th graders annually in the spring during each of these school years, but was discontinued after the 2013-14 school year. The ISAT measures individual student achievement relative to the Common Core State Standards. During the first three academic years of the study, ISAT scores were used to determine Adequate Yearly Progress for schools as part of the district and state school accountability systems.

The second standardized test we analyze is the Measures of Academic Progress (MAP), published by the Northwest Evaluation Association (NWEA). The MAP was administered in each of the four years of the study in math and reading to students in grades 3 through 8, and additionally to students in grade 2 in the final two years of the study. The MAP assessment is a computerized, adaptive test that changes which questions to ask students based on an estimate of ability level as indicated by responses to previous questions. For all four study years MAP scores were used as part of the growth component of teachers' evaluations, and for the 2014-15 school year the MAP scores replaced ISAT scores in the state school accountability system.

The C&C program manager at CPS maintained records of which students were approached and offered the chance to participate in C&C and which of those students participated in the program. We use this information to measure the participation rates used to estimate the TOT models. We also descriptively examined dosage through data collected by C&C mentors throughout the intervention. C&C mentors completed biweekly reporting forms documenting the number of interactions they had with students and their families, indicating whether these interactions were in-person or over the phone.

B. Descriptive statistics and balance tests

Tables 1 and 2 present pre-randomization descriptive statistics and randomization balance tests for the students in the schools where C&C services were provided in cohorts 1 and 2, respectively. As was described in section 4, there was a first round of school-level randomization to choose the schools where C&C would be implemented, and then within those schools students were selected for treatment and control groups. The schools that were randomly selected to be comparison schools in the first school-level round of randomization will be used to estimate whether there was a spillover of the program on non-participants, but are not included in the main analysis of program effects for participants. Focusing on within-school comparisons helps to align the cohort 1 and cohort 2 analysis since the cohort 2 sample was selected from within nine of the cohort 1 C&C schools.

Table 1 presents means measured during the 2010-11 school year, the year prior to the implementation of the C&C program, and prior to randomization for cohort 1. Table 2 presents means measured during the 2012-13 school year, the year prior to implementation and randomization for cohort 2. One of twenty pairwise t-tests conducted across the two cohorts was statistically significant (students assigned to treatment in cohort 1 had 0.7 more days present in the baseline year than students assigned to control), and joint F-tests in both cohorts indicate that baseline measures were balanced across students assigned to treatment and control.

In Table 1, the first column shows means for students assigned to control, because they either were in a school that was randomly selected to be a control school, were in a grade randomly selected to be a control grade within a treatment school, or were a student randomly selected to be control within a treatment grade in a treatment school. The second column shows means for the students assigned to treatment in cohort 1. There were 933 students assigned to treatment and 2,958 students assigned to control. The disproportionate size of the control group results from placing students into groups of three and selecting one for the treatment group and two for the control group and from randomly selecting two of seven grades within each school to be controls. In the year before random assignment, on average control and treatment students attended 150.3 and 151.0 out of a possible 170 days of school. This difference was statistically significant based on a pairwise t-test; this was the only statistically significant difference found in either cohort for any of the baseline variables. On average, in the year prior to random assignment, control students were absent 16.1 days and treatment students were absent 16.3 days, a difference that was not statistically significant. Total days present and total days absent do not sum to the length of the school year because some students were not enrolled as a CPS student for the entire school year, possibly because they moved out of the district. For this reason, in the analyses below, we report effects on days present and days absent separately, and analyze the treatment effect on the sum of days present and days absent, which is called membership days.

The students in the cohort 1 study were nearly 60 percent African-American and just under 40 percent Latino. On average, students failed 0.68 courses in the year prior to the study in both the control and treatment groups, and the average grade point average was 2.22 in the control group and 2.24 in the treatment group. About 10-11 percent of the students had documented learning disabilities, and between 15-18 percent of the students were old for grade.

There was one statistically significant difference between students assigned to treatment and control out of the ten variables shown in Table 1, and an F-test of the joint significance from a regression of a treatment assignment dummy on all of the variables yields a p-value of 0.599, indicating balance.

Table 2 presents means and balance tests for the students in cohort 2. As a reminder, randomization for cohort 2 took place in the summer of 2013. The treatment schools in cohort 2 were a subset of 9 of the cohort 1 treatment schools. Within those schools, students who were in the treatment and control groups of cohort 1 were re-randomized into treatment and control groups for cohort 2. In addition, there were randomization blocks for cohort 2 made up of students who were not in the cohort 1 study: students who were entering 1st and 2nd grade in the first year of cohort 2 and were therefore too young at the beginning of cohort 1 to be in the study, students who were new to the cohort 2 treatment schools, students whose school absences in the cohort 1 baseline year (2010-11) made them ineligible but who had absences within the eligibility range during the baseline year for cohort 2, and students who were nominated by their principal to be subject to random assignment.

As Table 2 shows, there were 1,111 students assigned to the control group, and 1,039 students assigned to the treatment group for cohort 2. There were no significant differences on any of the ten baseline variables for cohort 2. In the year before random assignment, on average control and treatment students attended 159.2 and 159.9 out of a possible 181 days of school.[5] On average, control students were absent 14.2 days and treatment students were absent 14.1 days in the year prior to random assignment, a slight overall decline in the percent of total days absent relative to the beginning of the cohort 1 study. Relative to the cohort 1 study sample, there were fewer African-American (47 versus 58 percent) and more Hispanic students (48 versus 39 percent) in the cohort 2 study schools. On average, control students in cohort 2 failed 0.41

[5] The CPS school year was extended from 170 to 181 days in 2012-13, the second year of the cohort 1 intervention and the baseline year for cohort 2. In 2013-14 and 2014-15, years one and two of the cohort 2 intervention there were 178 and 180 total days of school in CPS.

courses and students in the treatment group failed 0.44 courses in the year prior to the study, and the average grade point average was 2.29 in the control group and 2.35 in the treatment group. About 5-6 percent of the students had documented learning disabilities, between 10-12 percent of the students were old for grade.

For cohort 2, there were no statistically significant differences between students assigned to treatment and control on any of the variables shown in Table 2, and an F-test of the joint significance from a regression of a treatment assignment dummy on all of the variables yields a p-value of 0.446.

Table 3 shows participation rates for students assigned to treatment and control in each cohort. The table shows two types of participation rates for treatment students and one type of participation rate for control students. The column marked 'treatment' reports the share of students assigned to treatment who participated. This is the participation rate used to scale up the intent to treat estimate to obtain the estimate of the treatment on the treated. As described above, because we were unsure of how many students would take up the program when offered, we assigned more students to treatment than was necessary to fill the program slots. As a result, many of the students assigned to treatment were not offered the chance to participate in the program. The column marked 'approached' reports the share of treatment students who were approached and offered the chance to participate who chose to participate.

In cohort 1, approximately half of students assigned to treatment participated in the program, and in cohort 2 approximately one-third of students assigned to treatment participated in the program. Among students who were approached and invited to be in the C&C program, participation rates were higher, ranging from 72 to 84 percent.

During each cohort, treatment was provided for two years. In the first year of cohort 1, 50 percent of students assigned to treatment participated in the Check & Connect program. Some noncompliance occurred because we randomly selected more students for treatment than there were available program slots, and some treatment students were not offered treatment. In the analysis, we first compare all students assigned to treatment with all those assigned to control, which is our intent to treat (ITT) analysis, and then we report treatment on the treated (TOT) estimates that scale up the ITT by the participation rate among the full set of randomly assigned treatment students (we discuss our analytic methods further below). In year 2 of cohort 1, the participation rate was 46 percent; the decline was partly the result of some students switching schools and other students leaving the CPS district, though C&C mentors continued to work with students who transferred schools as long as students did not move too far away within the district.[6] Across both years of cohort 1, 52 percent of students assigned to treatment participated in either or both years. The participation rate in cohort 2 was lower than in cohort 1. During the first year of cohort 2, 31 percent of students assigned to treatment participated, and during the second year 32 percent did. Across both years of cohort 2, 33 percent of students assigned to treatment participated in either or both years.

As Table 3 also shows, there was no crossover from control to treatment. No students assigned to control were assigned a Check & Connect mentor or were offered a chance to formally participate in the program. Since the mentors worked in the schools every day, it is possible that they interacted with control students in a way that we are not able to measure. One nice feature of the experimental design is that because randomization occurred both at the school and student level, we are able to experimentally test whether there were spillovers effects on control students in treatment schools. In future work we will analyze whether there were spillover effects on students who did not participate directly in C&C.

Table 4 presents a descriptive analysis of which students chose to participate from among those offered the chance. The table shows results from linear probability regressions of an indicator for participation on baseline variables and a full set of randomization block fixed effects. The regressions only include students assigned to treatment, and there is a separate regression for each cohort. In cohort 1, students with more baseline absences and lower baseline GPA were more likely to participate conditional on being selected for treatment. In cohort 2, none of the baseline variables were significantly related to participation. The pattern of participation in cohort 1 was most likely the result of the way students were ordered to be offered treatment conditional on being randomly selected. Among the randomly selected treatment students, those with more baseline absences were approached first during cohort 1. While this affects who the compliers were, since we analyze the data based on randomly assigned treatment status – all students randomly assigned to treatment are considered treatment in both the ITT and

[6] In cohort 1, each mentor began working in either one or two schools. By the end of the second year of the program, the median number of schools a mentor was working in was 6, and two mentors were working in more than 10 schools. Informed by the degree of mobility experienced during cohort 1, we instituted a rule in cohort 2 where mentors would follow transferring students only if they moved less than 5 miles, and mentors could work at a maximum of 6 schools. Once mentors hit their school maximum during cohort 2, they did not follow students who subsequently transferred.

TOT analyses, regardless of whether they were offered treatment – the ITT and TOT estimates should not be biased by the order in which the offer of treatment occurred. In cohort 2, students were randomly ordered conditional on being selected for treatment and were offered treatment in that random order.

6. Analysis plan

A. ITT and TOT

Our goal is to estimate the causal effect of participating in the C&C program on student attendance and academic outcomes. We rely on the random assignment of the offer to participate in the program to identify the causal effect. We present two types of estimates: intent to treat (ITT) and treatment on the treated (TOT). The ITT estimate comes from estimating equation (1):

(1) $$Y_{it} = \pi_0 + \pi_1 Z_{i0} + X_{i0}\pi_2 + B_i\pi_3 + \varepsilon_{it}$$

where Y_{it} is an outcome for student *i* measured after random assignment in year $t \in (1,2)$ of the program, Z_{i0} is an indicator for having been randomly assigned to receive an offer to participate in C&C, B_i is a set of school and grade fixed effects for cohort 1 and a set of school and grade-by-randomization block fixed effects for cohort 2, ε_{i1} is a random error term, and X_{i0} is a set of baseline controls measured prior to random assignment that include days present, days absent, GPA, course failures, indicators for gender, race / ethnicity, age, old for grade, and presence of a learning disability. The ITT is an estimate of $\hat{\pi}_1$ and should be an unbiased estimate of the effect of being assigned to treatment because Z_{i0} is randomly assigned conditional on B.

Since the offer to participate was not extended to all students who were randomly assigned to treatment, and because some students offered the chance to participate declined, the ITT is likely to understate the magnitude of the effect of participating in C&C. To estimate the effect of having a C&C mentor, we use random assignment as an instrument for participation (Angrist, Imbens and Rubin, 1996; Bloom, 1984). This instrumental variables estimate recovers the effect of the treatment on the treated (TOT) because no students assigned to the control group were assigned to a C&C mentor. Though C&C mentors may have interacted with students in the control group, and treatment students whose behavior was influenced by their C&C mentor may have had follow-on effects on their peers in the control group, we distinguish these kinds of spillover effects from treatment-control crossover. We describe below the way the research design allows for experimental estimates of these spillovers.

The first stage of the TOT estimation is described in equation (2):

(2) $$D_{it} = \gamma_0 + \gamma_1 Z_{i0} + X_{i0}\gamma_2 + B_i\gamma_3 + \mu_{it}$$

where D is an indicator for having participated in C&C, the $\gamma's$ are parameters to be estimated, μ is a random error term, and all other variables are defined as above. The second stage equation is described in equation (3):

(3) $$Y_{it} = \beta_0 + \beta_1 D_{it} + X_{i0}\beta_2 + B_i\beta_3 + \upsilon_{it}$$

where υ is a random error term and the $\beta's$ are parameters to be estimated. By using random assignment (Z) as an instrument for participation (D), the TOT estimate is identified by conditional random assignment. The TOT does not compare participants to non-participants – that comparison would be biased because participants are different on average than non-participants. Rather, the TOT compares students randomly assigned to treatment, regardless of whether they were invited to participate and regardless of whether they chose to participate, to control students (the ITT), and scales this comparison by the participation rate among the treatment group to recover the effect of receiving treatment on those who participated.

B. Analysis of spillover effects

It is possible that the presence of the C&C program within a school has effects on students who are not assigned to a mentor. For example, if C&C mentors help to reduce absences among chronically absent students, those students might be less likely to induce their friends to skip school. To the extent that there are positive spillover effects of C&C, the TOT estimates based on within-school comparisons will tend to understate the effect of participation on the C&C participants.

The research design allows us to measure spillover effects based on random assignment. As described above, the 23 study schools for cohort 1 were randomly selected from a group of 69 schools. And, within each of the 23 study schools, two of the seven eligible grades were randomly selected as comparison grades. If spillovers operate only within school, we can make comparisons between the control students in the 23 study schools and the comparable students in the 46 control schools to help identify spillover effects. And, if spillover effects operate only within grades, comparisons between the control students in treated grades and control students in the non-treated grades within the same schools will also measure spillover effects. In future work, we plan to use this feature of the experimental design to estimate spillover effects.

7. Results

A. Overall effects on school attendance

We report both ITT and TOT estimates of the effect of C&C in our exhibits. We begin in Table 5 with estimated effects on attendance that pool data across both years of the program, and that pool students across all of the grades included in the study. Subsequent tables show separate program effects for older and younger students, for year 1 and year 2 of the program separately, and for different outcomes.

The top two panels of Table 5 show estimates for cohort 1 and cohort 2 separately, and the bottom panel shows estimates for both cohorts pooled. The table presents the mean of the dependent variable for the control group alongside the ITT estimate, and the mean of the dependent variable among control compliers (the "control complier mean," or CCM; see Katz, Kling and Liebman, 2001) alongside the TOT estimate. Before turning to the estimated treatment effects, it is interesting to note a pattern in absences among the control group. Control group absences declined from the baseline year to the two program years. For cohort 1, absences among the control group declined from 16.1 in the baseline year to an average of 14.2 per year over the two program years. For cohort 2, absences among the control group declined from 14.2 in the baseline year to an average of 11.5 per year over the two program years. This decline is likely a reversion to the mean since the study sample was selected based on having absences within a high range in the baseline year. In the results presented below, the treatment effects are measured relative to the control group and therefore account for any reversion to the mean experienced between the baseline year and the program year.

Focusing first on days absent, the ITT estimate for cohort 1 pooled across all grades is a 0.55 day reduction, and the estimated effect of participation for the participants is a reduction of 1.10 days (both estimates significant at the 10 percent level). For cohort 2, the ITT estimate is a statistically significant 0.78 day reduction, and the TOT estimate of the effect of participation in C&C is a statistically significant 2.7 day reduction. Combining both cohorts and including all grades, the ITT estimate is a statistically significant reduction of 0.71 days, and the TOT estimate of the effect of participation in C&C is a statistically significant reduction of 1.71 days. We estimate that the average treatment effect of participation was a 1.71 reduction in absences, equal to 11.6 percent of the control complier mean of 14.7 days.

The estimated effect on days present largely mirrors the effects on days absent. In cohort 1, the ITT effect on days present is a 1.0 day increase, and the TOT estimate is a 2.0 day increase, both significant at the 10 percent level. For cohort 2, the ITT estimate is a statistically insignificant 0.39 increase in days present and the TOT estimate is a statistically insignificant 1.3 day increase in days present. Combining cohorts 1 and 2, we estimate that participation in C&C caused a statistically significant increase of 2.6 additional days present.

As mentioned above, days present and days absent do not sum to the total number of days in the school year because some students either leave the CPS district or have days when they are not officially enrolled at any CPS school while they are transitioning from one school to another. Each day a student is officially enrolled in a CPS school he or she accumulates what is called a membership day. A student's total membership days are equal to the sum of his days absent and days present. To check to make sure the estimated effects on school absences are not a result of reducing the number of days students are enrolled in school, the table also shows estimates of the effect of participation in C&C on membership days. We do not find statistically significant effects of assignment to treatment or participation in C&C on membership days in cohort 1 or cohort 2 separately, or in the model that pools both cohorts.

B. Attendance effects by age

It seems reasonable that the effect of a mentoring-based attendance program might change as students age. Though the C&C program encourages mentors to connect with parents, in practice the mentors primarily interacted with students. If parents and other adult caregivers are relatively more influential on student attendance when students are younger, and students begin to play more of an independent role as they age, we might expect a largely school-based intervention to have larger effects for middle school-aged than for elementary school-aged students. To test this hypothesis, Table 6 presents estimates of attendance effects separately by age. The table shows separate estimates for students who were in 1st-4th grade, and those who were in 5^{th}-7^{th} grade, in the first year of the program.[7] The left side of the table shows ITT estimates by age group, and the right side of the table shows TOT estimates for each age group.

The results show a clear pattern of larger treatment effects for the grade 5-7 group than for the grade 1-4 group. For cohort 1, we estimate that participation in C&C reduced absences by a statistically significant 3.4 days for students who began the program in grades 5-7. For cohort 2, the comparable estimate is also a statistically significant 3.4 day reduction in absences. Pooling both cohorts together, we estimate participation in C&C reduced absences by 3.4 days, also statistically significant. Relative to mean absences among the control compliers of 17.7 and 16.0, we estimate that participation in C&C reduced the number of absences by participants in 5^{th}-7^{th} grade by 19.4 and 21.4 percent in cohorts 1 and 2, respectively, and by 20.2 percent on average across the two cohorts. Treatment effects on days present are similar in magnitude though vary slightly across cohorts, with a statistically significant 5.2 additional days attended for cohort 1 and a statistically insignificant 2.5 additional days attended for cohort 2. Pooling cohorts 1 and 2, we estimate that participation in C&C caused a statistically significant increase of 4.3 days present. Though the point estimates for days absent and days present are different by 1-1.5 days, we do not estimate a statistically significant difference in membership days (the sum of days absent and days present) for the grade 5-7 students.

In contrast to the significant treatment effects for students in grades 5-7, the treatment effects for the younger elementary-aged students were statistically indistinguishable from zero in both cohorts. In cohorts 1 and 2, the TOT estimates are an increase of 0.67 days absent and a decrease of 2.0 days absent, neither of which is statistically significant. For days present, the TOT estimates are an increase of 0.46 days for cohort 1 and a decrease of 0.35 days for cohort 2, neither of which is statistically significant. In cohort 1 and in the models that pool both cohorts, the difference in treatment effects on days absent by age group is statistically significant, though it is not for cohort 2 separately.

It is possible that the difference in treatment effects by age may be due to differences in other characteristics that are correlated with age and related to variation in treatment effects. For example, older students have higher baseline absence rates, and it may be the case that treatment effects are larger for students with higher baseline absence rates. To assess whether something

[7] Because the intervention lasted for two school years, the former group received C&C services in grades 1-5 and the latter group received C&C services in grades 5-8. For ease of exposition, we will refer to the two groups as 1^{st}-4^{th} graders and 5^{th}-7^{th} graders, though the treatment occurred in 1^{st}-5^{th} and 5^{th}-8^{th} grades, respectively.

along these lines explains the difference in treatment effects by age, we reweight the models for both age groups to make them representative of the older group. We first run a regression of an indicator for being in the older group on a set of baseline variables, including baseline absences. We then calculate a predicted value for each student in the study. Finally, we estimate the standard treatment effect models, weighting by the predicted value from the first-step regression. The results of this exercise are presented in Table 7. As can be seen in the table, the results are almost identical to the unweighted results from Table 6. It does not appear that the difference in treatment effects by age are related to variation in treatment effects correlated with observables that differ between older and younger students. Rather, it appears that C&C is a more effective student attendance program for middle school-aged students than for students in the elementary grades. Perhaps this is the case because C&C primarily focuses on interactions between the mentor and the student, and middle school-aged students have more agency over school attendance than elementary school-aged students.

C. Effects on the distribution of days absent

Thus far, we have presented results on the average number of days absent or present, as well as the percent of days present. Additionally, the pattern of effects throughout the absences distribution may be informative of the mechanism by which C&C mentors help to reduce absenteeism. C&C mentors may be most effective at identifying and remedying situations in which students are at risk for extreme absenteeism, or they may be effective at reducing absences a small amount for all of their students.

To explore the distribution of treatment effects, in Tables 8 and 9 we present results from models in which the dependent variables are a series of indicators for having more than 5, 10, 15 and 20 absences. When we break the results up by age, we find that participation in C&C caused a reduction of about 5 to 13 percentage points in the likelihood of crossing each of the 5, 10, 15 and 20 day thresholds for the students who began the program in grades 5-7. Since the baseline rates are lower for higher thresholds – by construction, fewer students have more than 20 absences than have more than 5 absences – the treatment effects are proportionally larger for the higher absence thresholds. In other words, C&C leads to larger proportional reductions in the share of students who miss more than four weeks of school than in the share of students who miss more than one week of school.

D. Effects by intervention duration

Students in both cohorts of the intervention received C&C services for two consecutive school years. One motivation for designing the intervention to be for two years was the hypothesis that the effectiveness of mentoring depends on the strength of the relationship between the student and the mentor, and the idea that relationships take time to develop. Thus far, we have presented results that pool the two years within each cohort. In contrast, the results presented in Table 10 show the treatment effects on days absent separately for the first and second year of the intervention. The table shows both ITT and TOT estimates, and breaks the results out by grade grouping, and by cohort.

For the 5^{th}-7^{th} grade students, we find that within both cohorts C&C reduced absences by more in the second year of the program than in the first, though the differences in treatment effects across years was not consistently statistically significant. In the first year of cohort 1, we estimate that participation in C&C caused a 2.9 day reduction in absences for 5^{th}-7^{th} graders, and in the second year of cohort 1, we find that C&C caused a 4.2 day reduction in absences for this group, both statistically significant. In cohort 2, the results were similar: a 2.5 day reduction in absences (not statistically significant) in year 1, and a (statistically significant) 4.4 day reduction in absences in year 2. When both cohorts are pooled, we find that C&C caused a 2.8 day reduction in absences in the first year of the program, and a 4.1 day reduction in absences in the second year of the program, both statistically significant. Though the pattern of results is suggestive that the effects are larger in the second year of the program than the first, the differences in treatment effects between the first and second year of the program are not quite statistically significant (though this is somewhat sensitive to whether we allow the effects of the baseline covariates to be different across cohorts or constrain them to be the same).

Though the differences in estimates are not statistically significant, the pattern in the results are suggestive that it may take some time for mentors and students to develop relationships, and that the strength of this relationship may be an important mediator of the effectiveness of the mentoring program. A related possibility is that it takes time for the C&C mentors to learn what is causing each individual student to be missing school, and that as the mentors recognize how they can most effectively help the student or intervene, the program's effect on absences grows. These are important questions for future research.

E. Effects on academic outcomes

We turn now to the question of whether the C&C mentoring program generated improvements in students' academic outcomes. Among mentoring programs, C&C is notable in its focus on attempting to engage students in school. In addition to the primary goal of increasing student attendance, C&C mentors are tasked with improving communications between students and teachers, and helping students to overcome things that make it hard for students to succeed while in school. In practice the C&C mentors in our study varied in the strategies they used to help students. Some acted as tutors from time to time, while others drew on their backgrounds in social work and acted as case managers, advocates, and counselors.

One might hypothesize that the increase in attendance induced by the C&C program may have generated improved academic outcomes. At the same time, given the emphasis on school engagement beyond attendance, and the ways C&C mentors were asked to track grades and academic performance alongside attendance, the mentors may have had a direct effect on academic outcomes over and above any effect that operated through improved attendance. It therefore seems natural to ask whether participating in the C&C program caused an improvement in grades and test scores.

Tables 11 and 12 present the effect on academic outcomes, including grade point average (GPA), course failures, and math and reading standardized test scores. Table 11 presents the results pooling all grades, and Table 12 shows the results separated out by age group. Regardless of whether the estimates are separated by age, we do not find significant effects of C&C on students' GPA. Point estimates for GPA are close to zero, never statistically significant, and based on the 95-percent confidence intervals we can rule out increases in the range of 0.1 to 0.15. In contrast, we find that C&C caused students in grades 5-7 to fail 0.17 fewer courses. This estimate was statistically significant when cohort 1 and cohort 2 data were pooled, but estimates were insignificant when modeled separately for cohort 1 and 2. A 0.17 decline in course failures is 20.2 percent of the CCM of 0.84.

We report treatment effects in math and reading for two standardized tests. Over the course of the study, CPS changed which test it used for accountability purposes. From 2011-12 through 2013-14, CPS relied on the Illinois Standard Achievement Test (ISAT), and in 2014-15 CPS used the Measures of Academic Progress (MAP) test published by the Northwest Education Association (NWEA), an assessment that CPS administered in every year of the study, but only used for accountability in 2014-15 when the ISAT was phased out. The ISAT was given to all

3rd-8th graders from 2011-12 through 2013-14, and the MAP was given to all CPS 3rd-8th graders in the 2011-12 and 2012-13 school years, and all 2nd-8th grade students in the 2013-14 and 2014-15 school years. In one test score model presented in Tables 14 and 15, the dependent variable is the test that was used for accountability in each year (the ISAT in the first three years of the study, and the MAP in the final study year). In the other model, the dependent variable is the MAP for all four study years. In all models, we standardize by grade, year, and subject to have mean zero and standard deviation one in the control group.

In cohort 1 for the 5th-7th grade group, we find a statistically significant negative effect on MAP math test scores of 0.24 standard deviations. In contrast, we find no significant effect on MAP reading scores, or on the test used for accountability in either math or reading in cohort 1 for the 5th-7th grade group. In cohort 2 for the older group, we do not find statistically significant test score effects on either math or reading in the MAP or in the test used for accountability. Pooling both cohorts together for the older group, we find a statistically significant decline of 0.19 standard deviations in MAP math scores, but no statistically significant effect on MAP reading or in math or reading on the test used for accountability. For the 1st-4th grade students, we do not find statistically significant effects on test scores for math or reading, for either test, in either cohort.

While the negative estimated effect on MAP math scores is sizeable, it is possible that it is influenced somewhat by selection. The students randomly assigned to C&C were 3 percentage points more likely to have a non-missing MAP math score during cohort 1 than the control group. One possibility is that by increasing attendance for the older students, C&C made it more likely that they would be in school when the MAP was administered. If the students induced to attend were lower-scoring students, this may have reduced the average test score among those with non-missing scores. Though we cannot say for sure, it seems unlikely however that a 3 percentage point change in the sample could explain a treatment effect of 0.2 standard deviations.

Taken together, the results in Tables 11 and 12 indicate that C&C had a marginal effect on academic outcomes. There is a suggestive finding of a negative effect on math scores for older students in cohort 1, though the significant majority of the estimates for test scores point to effects close to zero. The estimates for GPA are for the most part close to zero and fairly precisely estimated, though we do find a statistically significant and fairly sizeable reduction in course failures.

8. Conclusion

In this paper we report the results of one of the few large-scale RCTs of a policy effort to improve student school attendance. The intervention, Check & Connect, seeks to supplement the social capital parents can provide children to support them in school by assigning them to an in-school mentor. The intervention relies on providing youth with a fair amount of one-on-one time with an adult whose full time job is to deliver the program; the cost per participant per year equals $1,700, or $3,400 total over two years.

The program seems to be effective in improving student attendance, particularly for slightly older children. While there were no distinguishable effects on attendance for students who began the program in grades 1-4, we find that participation in C&C caused a 3.4 day, or 20.2 percent, reduction in absences among students in grades 5-7. The program effects appear to have grown in the second year of the program for both cohorts, though the gain in treatment effects over time was not statistically significant. For academic outcomes, the results are more mixed. We find no effects on grade point average for either age group, and a statistically significant decline in test scores for 5^{th}-7^{th} grade students in some models. However, we also find that C&C reduced the number of courses that 5^{th}-7^{th} grade students failed.

Though the program leads to significant improvements in its primary target outcome – attendance—given the costs of the program, the size of the impacts beg the question of whether this intervention is the most cost-effective way to improve student attendance. One way to measure cost-effectiveness is by the cost per day of attendance increased. Our calculations suggest C&C cost approximately $500 per day of improved attendance. By way of comparison, another program implemented in CPS during our study called the Chicago Attendance Project—a mail-based, large scale intervention that informed CPS guardians of how many absences their student had accumulated that semester—though it only incrementally decreased absences, did so at about $5 per day.

One lesson from these results may be that lower-cost, less-intensive interventions may provide the most cost-effective way to improve school attendance, particularly for marginal improvements. However, higher-cost interventions may be necessary to reduce absences by more than small amounts. Our results also are suggestive that helping students to overcome the causes of truancy may not be enough to significantly improve learning. Further research should focus on the most effective and cost-effective ways to improve learning and academic achievement for students who are currently missing substantial amounts of school.

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Table 1: Cohort 1 student baseline characteristics

	Control	Treatment
Students (n)	2,958	933
Days Present in 2010-11 SY	150.3	151.0*
Days Absent in 2010-11 SY	16.1	16.3
% Male	53%	53%
Age	9.0	8.7
% Old for Grade	18%	15%
% Black	59%	57%
% Hispanic	38%	40%
% Learning Disability	11%	10%
# Course Failures in 2010-11 SY	0.68	0.68
GPA in 2010-11 SY	2.22	2.24
P-value on F-test		p=.599

Notes: The length of the CPS school year changed during the study. The number of days in the school year, by year, was: 2010-11, 170; 2011-12, 170; 2012-13, 181; 2013-14, 178; 2014-15, 180. In the study sample, days present and days absent do not sum to total days in the school year because some students were not enrolled as CPS students or the full school year.
* p<0.10, ** p<0.05, ***p<0.01.

Table 2: Cohort 2 student baseline characteristics

	Control	Treatment
Students (n)	1,111	1,039
Days Present in 2012-13 SY	159.2	159.9
Days Absent in 2012-13 SY	14.2	14.1
% Male	53%	53%
Age	8.5	8.4
% Old for Grade	12%	10%
% Black	46%	47%
% Hispanic	49%	47%
% Learning Disability	5%	6%
# Course Failures in 2012-13 SY	0.41	0.44
GPA in 2012-13 SY	2.29	2.35
P-value on F-test		p=.446

Notes: The length of the CPS school year changed during the study. The number of days in the school year, by year, was: 2010-11, 170; 2011-12, 170; 2012-13, 181; 2013-14, 178; 2014-15, 180. In the study sample, days present and days absent do not sum to total days in the school year because some students were not enrolled as CPS students or the full school year.
* p<0.10, ** p<0.05, ***p<0.01.

Table 3: Participation rates

	Year 1		
	Treatment	Approached	Control
Cohort 1	50%	77%	0%
Cohort 2	31%	79%	0%
	Year 2		
	Treatment	Approached	Control
Cohort 1	46%	72%	0%
Cohort 2	32%	80%	0%
	Either Year		
	Treatment	Approached	Control
Cohort 1	52%	81%	0%
Cohort 2	33%	84%	0%

Table 4: Regression of participation dummy variable against baseline characteristics

Baseline Characteristics	Cohort 1	Cohort 2
Outcome	Participation (either year)	Participation (either year)
Baseline Days Present	0.000	0.000
Baseline Excused Absences	0.023***	0.003
Baseline Unexcused Absences	0.024***	-0.001
Baseline GPA	-0.088***	-0.009
Baseline Course Failures	-0.022	-0.008
Dummy for Male	-0.01	0.016
Age	-0.195	-0.013
Old for Grade	0.135	0.003
Dummy for Black	0.231 **	-0.071
Dummy for Hispanic	0.178 **	-0.073
Dummy for Learning Disability	0.061	0.032
Dummy for Missing Baseline Grade Data	-0.272 **	-0.042
Dummy for Missing Baseline Attendance Data	-	0.076
N	933	1039
R-squared	0.637	0.637
Notes: Both models include school fixed effects. The model for cohort 2 includes additional randomization block fixed effects to account for differential probability of selection for treatment based on whether the student participated in cohort 1 and which grade the student was in during cohort 2. * p<0.10, ** p<0.05, ***p<0.01.		

Table 5: Main impacts (pooled years)

	CM	ITT	TOT	CCM	Unadjusted p-value	FWER
	Cohort 1					
Days Absent	14.2	-0.550 *	-1.101 *	15.1	0.064	0.109
		(0.297)	(0.591)			
Days Present	156.6	1.024 *	2.049 *	156.7	0.069	0.109
		(0.563)	(1.118)			
% Present	91.6	0.373 **	0.747 **	91.1	0.038	0.070
		(0.180)	(0.358)			
Membership Days	170.8	0.474	0.948	171.8	0.337	0.313
		(0.493)	(0.981)			
	Cohort 2					
Days Absent	11.5	-0.784 **	-2.681 **	14.3	0.017	0.046
		(0.328)	(1.110)			
Days Present	164.0	0.393	1.342	164.0	0.540	0.635
		(0.640)	(2.165)			
% Present	93.3	0.491 **	1.679 **	91.7	0.011	0.036
		(0.194)	(0.654)			
Membership Days	175.4	-0.391	-1.339	178.3	0.502	0.635
		(0.583)	(1.982)			
	Pooled Cohorts					
Days Absent	13.4	-0.709***	-1.713***	14.7	0.001	0.007
		(0.217)	(0.520)			
Days Present	158.7	1.085***	2.620***	158.9	0.009	0.032
		(0.418)	(1.000)			
% Present	92.1	0.455***	1.100***	91.4	0.000	0.004
		(0.130)	(0.312)			
Membership Days	172.1	0.376	0.907	173.6	0.314	0.357
		(0.373)	(0.897)			
Notes: * p<0.10, ** p<0.05, *** p<.01.						

Table 6: Main impacts by age (pooled years)

	Grades 1 - 4		Grades 5 - 7		H0: Old = Young p-value	Grades 1 - 4		Grades 5 - 7		H0: Old = Young p-value
	CM	ITT	CM	ITT		CCM	TOT	CCM	TOT	
	Cohort 1									
Days Absent	12.9	0.315	16.1	-1.854***	0.001	13.1	0.665	17.7	-3.427***	0.001
		(0.353)		(0.544)			(0.743)		(0.999)	
Days Present	157.7	0.220	155.0	2.794***	0.032	158.2	0.463	153.7	5.164***	0.041
		(0.715)		(0.966)			(1.501)		(1.751)	
% Present	92.3	-0.133	90.4	1.203***	0.001	92.2	-0.281	89.6	2.224***	0.001
		(0.210)		(0.342)			(0.442)		(0.625)	
Membership Days	170.7	0.535	171.1	0.940	0.693	171.4	1.128	171.3	1.737	0.760
		(0.651)		(0.794)			(1.365)		(1.450)	
	Cohort 2									
Days Absent	10.4	-0.448	14.5	-1.651 **	0.150	12.9	-1.964	16.0	-3.417 **	0.497
		(0.339)		(0.771)			(1.471)		(1.552)	
Days Present	165.1	-0.080	161.0	1.222	0.373	165.9	-0.351	162.4	2.529	0.478
		(0.721)		(1.284)			(3.133)		(2.579)	
% Present	94.0	0.248	91.6	1.093 **	0.090	92.7	1.087	90.6	2.263 **	0.353
		(0.199)		(0.462)			(0.860)		(0.930)	
Membership Days	175.4	-0.528	175.5	-0.429	0.936	178.8	-2.314	178.4	-0.887	0.696
		(0.677)		(1.061)			(2.957)		(2.149)	
	Pooled Cohorts									
Days Absent	12.1	-0.191	15.8	-1.782***	0.002	13.1	-0.531	17.0	-3.430***	0.007
		(0.242)		(0.446)			(0.669)		(0.846)	
Days Present	160.0	0.641	156.2	2.242***	0.082	159.8	1.781	157.1	4.316***	0.207
		(0.500)		(0.773)			(1.381)		(1.455)	
% Present	92.9	0.133	90.6	1.161***	0.001	92.3	0.371	90.0	2.235***	0.004
		(0.143)		(0.276)			(0.395)		(0.523)	
Membership Days	172.2	0.450	172.0	0.460	0.989	172.9	1.250	174.1	0.886	0.836
		(0.464)		(0.635)			(1.282)		(1.208)	
Notes: *p<.10, **p<.05, ***p<.01.										

Table 7: Main impacts by age, weighted by probability of being in older grades

	Grades 1 - 4		Grades 5 - 7		H0: Old = Young p-value	Grades 1 - 4		Grades 5 - 7		H0: Old = Young p-value
	CM	ITT	CM	ITT		CCM	TOT	CCM	TOT	
	Cohort 1									
Days Absent	13.6	0.338	16.1	-1.854***	0.001	13.1	0.712	17.7	-3.427***	0.001
		(0.359)		(0.544)			(0.754)		(0.999)	
Days Present	157.7	0.182	155.0	2.794***	0.030	158.3	0.384	153.7	5.164***	0.038
		(0.715)		(0.966)			(1.498)		(1.751)	
% Present	92.0	-0.149	90.4	1.203***	0.001	92.2	-0.314	89.6	2.224***	0.001
		(0.213)		(0.342)			(0.447)		(0.625)	
Membership Days	171.3	0.520	171.1	0.940	0.683	171.4	1.096	171.3	1.737	0.747
		(0.650)		(0.794)			(1.362)		(1.450)	
	Cohort 2									
Days Absent	10.4	-0.451	14.5	-1.651 **	0.151	12.9	-1.971	16.0	-3.417 **	0.498
		(0.339)		(0.771)			(1.465)		(1.552)	
Days Present	165.1	-0.093	161.0	1.222	0.368	166.0	-0.408	162.4	2.529	0.468
		(0.719)		(1.284)			(3.115)		(2.579)	
% Present	94.0	0.250	91.6	1.093 **	0.091	92.7	1.093	90.6	2.263 **	0.355
		(0.199)		(0.462)			(0.857)		(0.930)	
Membership Days	175.5	-0.545	175.5	-0.429	0.926	178.9	-2.378	178.4	-0.887	0.682
		(0.675)		(1.061)			(2.938)		(2.149)	
	Pooled Cohorts									
Days Absent	12.8	-0.161	15.8	-1.782***	0.001	13.1	-0.450	17.0	-3.430***	0.006
		(0.242)		(0.446)			(0.672)		(0.846)	
Days Present	159.8	0.550	156.2	2.242***	0.066	160.0	1.533	157.1	4.316***	0.166
		(0.499)		(0.773)			(1.381)		(1.455)	
% Present	92.5	0.115	90.6	1.161***	0.001	92.4	0.320	90.0	2.235***	0.003
		(0.143)		(0.276)			(0.396)		(0.523)	
Membership Days	172.6	0.389	172.0	0.460	0.927	173.1	1.083	174.1	0.886	0.910
		(0.462)		(0.635)			(1.280)		(1.208)	
Notes: *p<.10, **p<.05, ***p<.01.										

Table 8: Absences by days absent threshold (pooled years)

Outcome	Control Mean	ITT	TOT	CCM	Unadjusted p-value	FWER
	Cohort 1					
Absent > 5days	0.870	-0.018 *	-0.036 *	0.901	0.083	0.205
		(0.010)	(0.021)			
Absent > 10 days	0.626	-0.031 **	-0.063 **	0.688	0.031	0.127
		(0.015)	(0.029)			
Absent > 15 days	0.369	-0.011	-0.022	0.400	0.434	0.588
		(0.014)	(0.028)			
Absent > 20 days	0.217	-0.008	-0.015	0.239	0.528	0.588
		(0.012)	(0.024)			
	Cohort 2					
Absent > 5 days	0.739	-0.012	-0.043	0.826	0.402	0.408
		(0.015)	(0.050)			
Absent > 10 days	0.465	-0.028 *	-0.097 *	0.569	0.079	0.148
		(0.016)	(0.055)			
Absent > 15days	0.264	-0.028 *	-0.094 **	0.355	0.050	0.137
		(0.014)	(0.048)			
Absent > 20 days	0.152	-0.028 **	-0.097 **	0.239	0.011	0.044
		(0.011)	(0.038)			
	Pooled Cohorts					
Absent > 5 days	0.834	-0.025***	-0.061***	0.892	0.004	0.116
		(0.008)	(0.018)			
Absent > 10 days	0.582	-0.041***	-0.099***	0.660	0.000	0.021
		(0.009)	(0.023)			
Absent > 15 days	0.341	-0.020 **	-0.048 **	0.376	0.048	0.116
		(0.009)	(0.021)			
Absent > 20 days	0.199	-0.015 **	-0.037 **	0.226	0.065	0.116
		(0.007)	(0.018)			
Notes: * p<0.10, ** p<0.05, *** p<.01 .						

Table 9: Absences by days absent threshold and age (pooled years)

	Grades 1 - 4		Grades 5 - 7		H0: Old = Young	Grades 1 - 4		Grades 5 - 7		H0: Old = Young
Outcome	CM	ITT	CM	ITT	p-value	CCM	TOT	CCM	TOT	p-value
	Cohort 1									
Absent > 5 days	0.856	-0.014	0.892	-0.028 *	0.501	0.882	-0.029	0.934	-0.052 *	0.583
		(0.014)		(0.016)			(0.029)		(0.030)	
Absent > 10 days	0.591	-0.004	0.679	-0.066***	0.043	0.639	-0.009	0.740	-0.122***	0.057
		(0.018)		(0.024)			(0.039)		(0.045)	
Absent > 15 days	0.325	0.029 *	0.437	-0.070***	0.001	0.329	0.061 *	0.489	-0.129***	0.001
		(0.017)		(0.024)			(0.036)		(0.045)	
Absent > 20 days	0.182	0.022	0.269	-0.046 **	0.009	0.184	0.046	0.299	-0.086 **	0.009
		(0.015)		(0.022)			(0.031)		(0.040)	
	Cohort 2									
Absent > 5 days	0.692	0.003	0.864	-0.063 **	0.034	0.745	0.012	0.953	-0.131 **	0.128
		(0.018)		(0.026)			(0.078)		(0.052)	
Absent > 10 days	0.422	-0.024	0.580	-0.045	0.572	0.519	-0.106	0.652	-0.094	0.909
		(0.018)		(0.033)			(0.079)		(0.067)	
Absent > 15 days	0.232	-0.021	0.352	-0.040	0.602	0.326	-0.093	0.384	-0.082	0.903
		(0.015)		(0.032)			(0.067)		(0.064)	
Absent > 20 days	0.128	-0.016	0.216	-0.057 **	0.146	0.208	-0.071	0.269	-0.118 **	0.524
		(0.012)		(0.026)			(0.052)		(0.052)	
	Pooled Cohorts									
Absent > 5 days	0.805	-0.017	0.887	-0.038***	0.234	0.860	-0.047	0.929	-0.073***	0.522
		(0.011)		(0.014)			(0.031)		(0.026)	
Absent > 10 days	0.538	-0.026 **	0.659	-0.059***	0.168	0.613	-0.074 **	0.706	-0.113***	0.445
		(0.013)		(0.020)			(0.035)		(0.037)	
Absent > 15 days	0.296	0.003	0.419	-0.061***	0.005	0.316	0.008	0.452	-0.117***	0.010
		(0.012)		(0.019)			(0.032)		(0.037)	
Absent > 20 days	0.165	0.002	0.258	-0.051***	0.007	0.186	0.005	0.284	-0.097***	0.013
		(0.009)		(0.017)			(0.026)		(0.032)	

Notes: * p<0.10, ** p<0.05, *** p<.01.

Table 10: Year effects (outcome = days absent)

	Grades 1 - 4				Grades 5 - 7			
	CM	ITT	TOT	CCM	CM	ITT	TOT	CCM
	Cohort 1							
Year 1	12.3	0.422	0.898	12.7	14.1	-1.574***	-2.861***	16.1
		(0.376)	(0.507)			(0.794)	(0.912)	
Year 2	13.6	0.193	0.403	13.6	18.2	-2.231***	-4.199***	19.5
		(0.475)	(0.772)			(0.985)	(1.430)	
H0:Y1=Y2; p-value		0.628	0.617			0.345	0.299	
	Cohort 2							
Year 1	10.6	-0.419	-1.911	12.8	13.3	-1.145	-2.478	14.7
		(0.396)	(0.827)			(1.773)	(1.705)	
Year 2	10.1	-0.467	-1.964	12.9	15.7	-2.245 **	-4.428 **	17.4
		(0.410)	(1.040)			(1.696)	(1.959)	
H0:Y1=Y2; p-value		0.912	0.977			0.279	0.328	
	Pooled Cohorts							
Year 1	11.8	-0.008	-0.023	12.5	14.0	-1.456***	-2.808***	15.6
		(0.268)	(0.434)			(0.752)	(0.821)	
Year 2	12.5	-0.386	-1.054	13.8	17.7	-2.161***	-4.149***	18.4
		(0.313)	(0.618)			(0.848)	(1.161)	
H0:Y1=Y2; p-value		0.239	0.242			0.215	0.213	
Notes: * p<0.10, ** p<0.05, *** p<.01.								

Table 11: Academic impacts (pooled years)

	CM	ITT	TOT	CCM	Unadjusted p-value	FWER
	Cohort 1					
GPA	2.26	-0.005	-0.009	2.20	0.853	0.889
		(0.025)	(0.049)			
Course Failures	0.57	-0.031	-0.062	0.67	0.364	0.819
		(0.034)	(0.068)			
Valid MAP Math Data	0.93	0.019 **	0.038 **	0.92	0.033	-
		(0.009)	(0.018)			
Standardize MAP Math Score*	0.00	-0.083 **	-0.170 **	0.05	0.018	0.168
		(0.035)	(0.072)			
Valid MAP Read Data	0.92	0.028***	0.056***	0.91	0.002	-
		(0.009)	(0.018)			
Standardize MAP Read Score*	0.00	-0.015	-0.031	-0.02	0.661	0.889
		(0.035)	(0.071)			
Valid Official Math Data	0.96	0.002	0.004	0.97	0.764	-
		(0.007)	(0.013)			
Standardize Official Test Math Score*	0.00	-0.038	-0.071	-0.02	0.262	0.819
		(0.033)	(0.063)			
Valid Official Read Data	0.96	0.004	0.007	0.97	0.561	-
		(0.006)	(0.013)			
Standardize Official Test Read Score*	0.00	-0.036	-0.068	-0.04	0.255	0.819
		(0.031)	(0.060)			
	Cohort 2					
GPA	2.67	-0.002	-0.005	2.45	0.952	0.998
		(0.026)	(0.087)			
Course Failures	0.36	-0.012	-0.039	0.44	0.734	0.990
		(0.034)	(0.115)			
Valid MAP Math Data	0.95	0.001	0.004	0.96	0.872	-
		(0.007)	(0.023)			
Standardize MAP Math Score*	0.00	0.006	0.019	-0.07	0.869	0.996
		(0.036)	(0.113)			
Valid MAP Read Data	0.94	0.004	0.012	0.94	0.626	-
		(0.008)	(0.025)			
Standardize MAP Read Score*	0.00	-0.023	-0.072	0.00	0.523	0.968
		(0.036)	(0.112)			
Valid Official Math Data	0.74	0.004	0.014	0.90	0.519	-

		(0.006)	(0.021)			
Standardize Official Test Math Score*	0.00	-0.002	-0.006	-0.08	0.952	0.998
		(0.037)	(0.101)			
Valid Official Read Data	0.73	0.008	0.029	0.88	0.220	-
		(0.007)	(0.023)			
Standardize Official Test Read Score*	0.00	-0.022	-0.059	-0.07	0.556	0.968
		(0.037)	(0.099)			
	Pooled Cohorts					
GPA	2.37	0.016	0.037	2.26	0.385	0.880
		(0.018)	(0.043)			
Course Failures	0.51	-0.032	-0.075	0.59	0.191	0.880
		(0.024)	(0.057)			
Valid MAP Math Data	0.94	0.008	0.021	0.94	0.141	-
		(0.006)	(0.014)			
Standardize MAP Math Score*	0.00	-0.043	-0.107 *	0.02	0.082	0.789
		(0.025)	(0.062)			
Valid MAP Read Data	0.93	0.013 *	0.032 **	0.93	0.037	-
		(0.006)	(0.015)			
Standardize MAP Read Score*	0.00	-0.035 **	-0.087	0.02	0.162	0.880
		(0.025)	(0.062)			
Valid Official Math Data	0.88	-0.013	-0.031 **	0.98	0.016	-
		(0.005)	(0.013)			
Standardize Official Test Math Score*	0.00	-0.036 **	-0.077	-0.01	0.142	0.880
		(0.024)	(0.052)			
Valid Official Read Data	0.88	-0.010	-0.025 *	0.97	0.053	-
		(0.005)	(0.013)			
Standardize Official Test Read Score*	0.00	-0.049 *	-0.105 **	-0.01	0.039	0.641
		(0.024)	(0.051)			
*The official test used for accountability purposes switched after year 1 of cohort 2, but the MAP was given all four years that cover both cohorts. Note: * p<0.10, ** p<0.05, *** p<.01.						

Table 12: Academic impacts by age (pooled years)

	Grades 1 - 4		Grades 5 - 7		H0: Old = Young	Grades 1 - 4		Grades 5 - 7		H0: Old = Young
	CM	ITT	CM	ITT	p-value	CCM	TOT	CCM	TOT	p-value
	Cohort 1									
GPA	2.38	0.009	2.08	-0.027	0.482	2.30	0.018	2.03	-0.049	0.493
		(0.033)		(0.037)			(0.070)		(0.068)	
Course Failures	0.44	-0.012	0.77	-0.063	0.488	0.51	-0.025	0.90	-0.115	0.521
		(0.041)		(0.060)			(0.086)		(0.111)	
Valid MAP Math Data	0.94	0.011	0.91	0.032 **	0.279	0.95	0.023	0.89	0.059 **	0.325
		(0.011)		(0.016)			(0.022)		(0.029)	
Standardize MAP Math Score*	0.00	-0.055	0.00	-0.129 **	0.304	0.03	-0.118	0.07	-0.246 **	0.382
		(0.045)		(0.057)			(0.096)		(0.109)	
Valid MAP Read Data	0.92	0.022 *	0.92	0.038 **	0.412	0.92	0.046 *	0.89	0.070 **	0.505
		(0.011)		(0.015)			(0.024)		(0.028)	
Standardize MAP Read Score*	0.00	-0.026	0.00	0.002	0.691	-0.01	-0.056	-0.04	0.004	0.669
		(0.045)		(0.054)			(0.097)		(0.102)	
Valid Official Math Data	0.96	0.000	0.95	0.004	0.769	0.98	0.000	0.97	0.007	0.780
		(0.010)		(0.009)			(0.020)		(0.017)	
Standardize Official Test Math Score*	0.00	-0.024	0.00	-0.050	0.706	0.03	-0.048	-0.06	-0.090	0.746
		(0.049)		(0.047)			(0.097)		(0.085)	
Valid Official Read Data	0.96	-0.005	0.96	0.012	0.213	0.99	-0.010	0.96	0.022	0.225
		(0.010)		(0.009)			(0.020)		(0.016)	
Standardize Official Test Read Score*	0.00	-0.045	0.00	-0.028	0.788	0.06	-0.090	-0.12	-0.050	0.745
		(0.044)		(0.045)			(0.089)		(0.081)	

	Cohort 2									
GPA	2.79	0.007	2.34	-0.028	0.565	2.49	0.030	2.39	-0.056	0.595
		(0.030)		(0.053)			(0.125)		(0.108)	
Course Failures	0.22	0.031	0.74	-0.139	0.084	0.20	0.131	0.79	-0.281	0.070
		(0.032)		(0.094)			(0.136)		(0.189)	
Valid MAP Math Data	0.95	-0.006	0.95	0.021	0.097	0.98	-0.025	0.93	0.043	0.126
		(0.009)		(0.014)			(0.034)		(0.028)	
Standardize MAP Math Score*	0.00	0.030	0.00	-0.057	0.291	-0.09	0.116	-0.05	-0.114	0.278
		(0.041)		(0.071)			(0.161)		(0.144)	
Valid MAP Read Data	0.94	-0.001	0.94	0.017	0.283	0.95	-0.005	0.94	0.036	0.395
		(0.010)		(0.014)			(0.038)		(0.029)	
Standardize MAP Read Score*	0.00	-0.017	0.00	-0.038	0.796	0.02	-0.068	-0.03	-0.078	0.962
		(0.042)		(0.069)			(0.166)		(0.137)	
Valid Official Math Data	0.67	-0.003	0.94	0.025 *	0.085	0.88	-0.012	0.93	0.051 *	0.128
		(0.007)		(0.015)			(0.030)		(0.030)	
Standardize Official Test Math Score*	0.00	0.005	0.00	-0.018	0.778	-0.07	0.017	-0.09	-0.035	0.789
		(0.045)		(0.068)			(0.143)		(0.136)	
Valid Official Read Data	0.65	0.003	0.95	0.024 *	0.203	0.84	0.014	0.93	0.050 *	0.417
		(0.008)		(0.014)			(0.033)		(0.029)	
Standardize Official Test Read Score*	0.00	0.002	0.00	-0.069	0.361	-0.09	0.006	-0.05	-0.140	0.441
		(0.045)		(0.064)			(0.143)		(0.127)	
	Pooled Cohorts									
GPA	2.51	0.036	2.13	-0.029	0.086	2.31	0.098	2.18	-0.055	0.068
		(0.022)		(0.031)			(0.060)		(0.059)	
Course Failures	0.37	-0.005	0.76	-0.090	0.135	0.43	-0.012	0.84	-0.170 *	0.183
		(0.026)		(0.051)			(0.070)		(0.097)	
Valid MAP Math Data	0.94	0.000	0.92	0.028	0.027	0.96	0.000	0.91	0.054***	0.055

		(0.007)		(0.010)			(0.020)		(0.020)	
Standardize MAP Math Score*	0.00	-0.019	0.00	-0.096	0.153	0.03	-0.054	0.02	-0.188 **	0.265
		(0.030)		(0.045)			(0.083)		(0.088)	
Valid MAP Read Data	0.93	0.005	0.93	0.029	0.067	0.94	0.015	0.91	0.057***	0.159
		(0.007)		(0.010)			(0.021)		(0.020)	
Standardize MAP Read Score*	0.00	-0.048	0.00	-0.008	0.448	0.08	-0.134	-0.06	-0.016	0.317
		(0.031)		(0.043)			(0.086)		(0.083)	
Valid Official Math Data	0.83	-0.029	0.95	0.015	0.000	1.00	- 0.085***	0.95	0.028 *	0.000
		(0.007)		(0.008)			(0.021)		(0.016)	
Standardize Official Test Math Score*	0.00	-0.034	0.00	-0.039	0.923	0.05	-0.081	-0.07	-0.073	0.937
		(0.032)		(0.038)			(0.076)		(0.072)	
Valid Official Read Data	0.83	-0.028	0.95	0.020	0.000	0.99	- 0.082***	0.95	0.037 **	0.000
		(0.007)		(0.008)			(0.022)		(0.015)	
Standardize Official Test Read Score*	0.00	-0.060	0.00	-0.034	0.583	0.09	-0.144 *	-0.12	-0.065	0.428
		(0.031)		(0.037)			(0.074)		(0.069)	

* The official test used for accountability purposes switched after year 1 of cohort 2, but the MAP was given all four years that cover both cohorts.
Note: * p<0.10, ** p<0.05, *** p<.01.

Appendix Table 2a: Cohort 1 students who stayed in study schools and were randomized again in cohort 2

	Control	**Treatment**
Students (n)	231	345
Days Present in 2012-13 SY	166.1	166.3
Days Absent in 2012-13 SY	13.6	13.8
% Male	57%	53%
Age	9.9	9.9
% Old for Grade	11%	11%
% Black	51%	51%
% Hispanic	46%	44%
% Learning Disability	8%	10%
# Course Failures in 2012-13 SY	0.55	0.56
GPA in 2012-13 SY	2.39	2.40
P-value on F-test		p=.902
Note: * p<0.10, ** p<0.05, *** p<0.01.		

Appendix Table 2b: Cohort 2 new entrants

	Control	**Treatment**
Students (n)	880	694
Days Present in 2012-13 SY	157.4	156.8
Days Absent in 2012-13 SY	14.4	14.3
% Male	52%	53%
Age	8.2	7.6
% Old for Grade	11%	7%
% Black	45%	44%
% Hispanic	50%	48%
% Learning Disability	5%	3%
# Course Failures in 2012-13 SY	0.37	0.38
GPA in 2012-13 SY	2.27	2.32
P-value on F-test		p=.205

Note: * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table 3: Participation Rates

	Year 1		**Year 2**		**Either Year**	
	Treatment	Control	Treatment	Control	Treatment	Control
Cohort 1	50%	0%	46%	0%	52%	0%
Cohort 2	31%	0%	32%	0%	33%	0%
	Cohort 1 kids randomized again in Cohort 2					
Cohort 1	52%	0%	52%	0%	54%	0%
Cohort 2	61%	0%	58%	0%	61%	0%

Appendix Table 5: Main impacts with triple fixed effects (pooled years)

	CM	ITT	TOT	CCM	p-value	FWER
	Cohort 1					
Days Absent	14.194	-0.501 *	-0.999 *	14.975	0.097	0.220
		(0.301)	(0.600)			
Days Present	156.648	1.105 *	2.204 *	156.543	0.073	0.196
		(0.615)	(1.223)			
% Present	91.568	0.366 *	0.730 *	91.120	0.051	0.163
		(0.187)	(0.372)			
Membership Days	170.842	0.604	1.205	171.518	0.273	0.297
		(0.551)	(1.097)			
	Cohort 2					
Days Absent	11.465	-0.768***	-2.632***	14.221	0.005	0.021
		(0.274)	(0.939)			
Days Present	163.980	0.222	0.763	164.560	0.718	0.732
		(0.615)	(2.105)			
% Present	93.345	0.458***	1.569***	91.828	0.006	0.021
		(0.166)	(0.567)			
Membership Days	175.445	-0.546	-1.870	178.781	0.346	0.472
		(0.579)	(1.992)			
	Pooled Cohorts					
Days Absent	13.444	-0.658***	-1.616***	14.586	0.002	0.009
		(0.208)	(0.509)			
Days Present	158.662	0.786 *	1.930 *	159.587	0.073	0.128
		(0.438)	(1.071)			
% Present	92.056	0.435***	1.069***	91.432	0.001	0.004
		(0.128)	(0.314)			
Membership Days	172.106	0.128	0.314	174.173	0.748	0.772
		(0.398)	(0.976)			
Note: * p<0.10, ** p<0.05, *** p<0.01.						

Appendix Table 6: Main impacts by age (pooled years)

	Grades 1 - 4		**Grades 5 - 7**		**H0: Old = Young p-value**	**Grades 1 - 4**		**Grades 5 - 7**		**H0: Old = Young p-value**
	CM	ITT	CM	ITT		CCM	TOT	CCM	TOT	
	Cohort 1									
Days Absent	12.944	0.445	16.098	-2.205***	0.000	12.878	0.936	18.258	-4.034***	0.000
		(0.335)		(0.579)			(0.707)		(1.058)	
Days Present	157.738	0.003	154.987	3.061***	0.018	158.695	0.006	153.216	5.601***	0.024
		(0.761)		(1.045)			(1.601)		(1.894)	
% Present	92.329	-0.208	90.407	1.405***	0.000	92.329	-0.436	89.213	2.571***	0.000
		(0.204)		(0.367)			(0.430)		(0.669)	
Membership Days	170.683	0.448	171.085	0.856	0.716	171.573	0.942	171.473	1.566	0.774
		(0.714)		(0.865)			(1.501)		(1.577)	
	Cohort 2									
Days Absent	10.352	-0.470 *	14.454	-1.473 **	0.176	12.964	-2.052 *	15.668	-3.092 **	0.585
		(0.274)		(0.647)			(1.194)		(1.357)	
Days Present	165.084	-0.116	161.013	0.704	0.574	166.092	-0.508	163.464	1.478	0.629
		(0.718)		(1.135)			(3.133)		(2.374)	
% Present	94.005	0.249	91.573	0.966 **	0.100	92.672	1.085	90.844	2.028 **	0.402
		(0.169)		(0.377)			(0.737)		(0.792)	
Membership Days	175.437	-0.587	175.467	-0.769	0.867	179.056	-2.560	179.132	-1.615	0.808
		(0.693)		(1.017)			(3.036)		(2.142)	
	Pooled Cohorts									
Days Absent	12.131	-0.030	15.759	-1.984***	0.000	12.688	-0.086	17.313	-3.793***	0.000
		(0.217)		(0.442)			(0.612)		(0.842)	
Days Present	160.042	0.017	156.228	2.302***	0.017	161.526	0.048	157.032	4.398***	0.039
		(0.521)		(0.795)			(1.468)		(1.503)	
% Present	92.855	0.037	90.647	1.272***	0.000	92.568	0.103	89.817	2.431***	0.000
		(0.133)		(0.276)			(0.374)		(0.526)	
Membership Days	172.174	-0.013	171.987	0.317	0.690	174.215	-0.038	174.344	0.606	0.733
		(0.494)		(0.665)			(1.392)		(1.268)	

Note: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Appendix Table 11a: Year effects (outcome = days present)

	Grades 1 - 4				Grades 5 - 7			
	CM	ITT	TOT	CCM	CM	ITT	TOT	CCM
	Cohort 1							
Year 1	152.8	0.145	0.309	152.7	152.1	2.309 **	4.197 **	151.17
		(0.903)	(1.124)			(1.903)	(1.993)	
Year 2	163.1	0.286	0.598	164.2	158.0	3.417 **	6.430***	156.16
		(0.904)	(1.341)			(1.873)	(2.478)	
H0:Y1=Y2; p-value		0.903	0.904			0.472	0.429	
	Cohort 2							
Year 1	163.7	-0.276	-1.256	165.4	162.0	-0.865	-1.873	165.58
		(0.947)	(1.526)			(4.255)	(3.175)	
Year 2	166.6	0.139	0.585	166.5	160.0	3.544 **	6.991 **	159.19
		(0.955)	(1.742)			(3.948)	(3.250)	
H0:Y1=Y2; p-value		0.740	0.732			0.028	0.026	
	Pooled Cohorts							
Year 1	156.2	0.807	2.283	155.3	154.1	1.213	2.338	156.51
		(0.656)	(0.908)			(1.838)	(1.708)	
Year 2	164.2	0.472	1.288	164.5	158.4	3.443***	6.610***	157.55
		(0.649)	(1.065)			(1.752)	(1.997)	
H0:Y1=Y2; p-value		0.695	0.674			0.070	0.066	
Note: * p<0.10, ** p<0.05, *** p<0.01.								

Appendix Table 11b: Year effects (outcome = days present / membership days)

	Grades 1 - 4				Grades 5 - 7			
	CM	ITT	TOT	CCM	CM	ITT	TOT	CCM
	Cohort 1							
Year 1	92.5	-0.161	-0.343	92.1	91.4	0.873 **	1.587***	90.5
		(0.229)	(0.340)			(0.484)	(0.605)	
Year 2	92.2	-0.103	-0.215	92.2	89.4	1.608***	3.026***	88.4
		(0.276)	(0.473)			(0.574)	(0.876)	
H0:Y1=Y2; p-value		0.834	0.827			0.096	0.078	
	Cohort 2							
Year 1	93.8	0.230	1.046	92.7	92.3	0.660	1.429	91.5
		(0.245)	(0.501)			(1.095)	(1.033)	
Year 2	94.2	0.265	1.115	92.7	90.8	1.579***	3.114***	89.7
		(0.234)	(0.609)			(0.965)	(1.147)	
H0:Y1=Y2; p-value		0.895	0.953			0.125	0.154	
	Pooled Cohorts							
Year 1	92.9	0.058	0.164	92.4	91.6	0.804***	1.550***	90.9
		(0.166)	(0.280)			(0.464)	(0.527)	
Year 2	92.8	0.213	0.582	92.2	89.7	1.571***	3.017***	89.0
		(0.181)	(0.375)			(0.488)	(0.706)	
H0:Y1=Y2; p-value		0.425	0.434			0.030	0.029	
Note: * p<0.10, ** p<0.05, *** p<0.01.								