CESifo
AREA CONFERENCES 2018

Economics of Education

Munich, 31 August & 1 September 2018

Better Together? Social Networks in Truancy and the Targeting of Treatment

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This draft: August 22, 2018

Abstract

Truancy is a risky behavior that predicts many adverse outcomes. We develop a strategy to use administrative data to construct social networks based on students who are truant together. We simulate these networks to document that certain students systematically coordinate their absences in the observed data. We show these networks are meaningful by leveraging a parent-information intervention on student absences in which we find spillover effects from treated students onto peers in their network. Excluding these effects understates the intervention's cost effectiveness by 43%. We develop an algorithm treatment-effect heterogeneity identified by machine-learning techniques to show there is potential to use these networks to target interventions more efficiently given a budget constraint.

[*]We thank Claudia Allende, Andrew Barr, Alex Bowers, Eric Chan, Francois Gerard, Laura Giuliano, Caroline Hoxby, Matthew Jackson, Adam Kapor, Bruce Sacerdote and participants in the NBER Education Program meeting for their comments. All errors are our own.

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1 Introduction

There is concern that the risky behaviors of teenage children negatively influence the behaviors of other children through their social networks. This influence could occur if, for instance, children learn behaviors from other children, signal their behaviors to peers, or derive utility from undertaking behaviors jointly (Akerlof and Kranton 2000; Austen-Smith and Fryer 2005; Bénabou and Tirole 2011; Bursztyn et al. 2014). Such mechanisms may be particularly relevant to school truancy, which predicts a number of adverse outcomes including high school dropout, substance abuse, and criminality (Kearney 2008; Goodman 2014; Aucejo and Romano 2016; Rogers and Feller 2016; Cook et al. 2017; Gershenson et al. 2017; Billings et al. 2016). Attendance is also an important metric for schools because it is frequently tied to state funding and many state proposals use chronic absenteeism as an indicator for accountability under the new Every Student Succeeds Act.[1]

Assessing the influence of social networks on risky behaviors such as absenteeism has important implications. Many interventions that aim to attenuate these behaviors can be expensive for school districts to implement. For instance, Check and Connect, which uses student mentors to significantly reduce student absences, costs $1,700 per child per year (Guryan et al. 2016).[2] Though difficult to assess, the benefits of this and other interventions may be understated if there are spillover effects. Moreover, if these spillovers occur along a measurable network, it may be possible to target the intervention more cost effectively by incorporating the potential for spillovers. Nonetheless, this possibility is muted if the networks are expensive to estimate or imperfectly measured, for instance via labor-intensive surveys or proxying a student's social network using students in the same grade.

In this paper, we show how administrative data can be used to cheaply construct social networks around a particular behavior of interest and how treatment effects spill over along these networks. Specifically, we use student-by-class-by-day attendance data to construct networks of who misses class with whom. The strength of each tie (or edge) between students is given by the number of times they miss the same class together. We assess the features of this network and test whether students systematically miss class with other students. We then leverage the random assignment from an automated-text message alert experiment, which includes alerts to parents for each time a student misses a class, to test if the effects of the alerts spill over to other students in the network and how these spillovers interact with the characteristics of the network. Lastly, we examine to what extent we can use the network information to target potentially costly but effective attendance interventions, subject to a budget constraint, to increase their cost effectiveness.

[1] cf. This article describing proposed state policies.

[2] There have been several experiments studying Check and Connect, including studies by Sinclair et al. (1998, 2005) and Maynard et al. (2014).

We find that students are more likely to miss an individual class than a full day of school, and that students systematically miss classes together. In fact, students are 4.7 times more likely to skip class with a specific student (whom we identify as their "closest peer") than with another of their peers. The networks also exhibit strong homophily: students tend to miss class with other students who have GPAs, behaviors, and racial characteristics that are predictive of their own characteristics. However, the latter could be due to correlated shocks or omitted contextual factors that induce apparent homophily. We show this explanation cannot fully account for the observed homophily in the network in certain characteristics by comparing simulated moments of the data to their observed counterparts. These tests provide evidence that the observed homophily is not entirely due to contextual factors. To demonstrate the relevance of these networks, we show that the text-message alert intervention exhibits spillovers onto individuals with whom treated students have strong network ties. In contrast, Bergman and Chan (2017) find that a common alternative measure of a student's network—students in the same grade as other students—exhibits weak, statistically insignificant spillovers. Lastly, we show evidence that joint absences are, in part, due to utility derived from missing class jointly with a specific student rather than deriving general utility missing class.

We also provide a targeting approach designed to maximize the total effect of an intervention considering heterogeneous spillovers. Given a budget restriction (the overall number of students that can be treated), this algorithm shows the potential to target other, more-expensive attendance interventions cost effectively. By identifying different types of students and their connections within the network, we show that it would be more efficient to first allocate the treatment to students who have many connections as other students' closest peer, and at the same time, have lower absenteeism rates. We find that using the social networks to target the treatment doubles the total effect on attendance in comparison to targeting the effect only based on baseline absenteeism rates. We also provide the framework to conduct the same exercise under other objective functions, such as the reduction of chronically absent students.

This paper contributes to a large literature on the interaction between social networks and risky youth behaviors.[3] The influence of peers on individuals' behaviors is difficult to estimate, in part, due to the reflection problem (Manski 1993). In the context of social influence on risky behaviors, a number of papers overcome this difficulty by structurally estimating peer interactions, as in Card and Giuliano (2013) and Richards-Shubik (2015), or by using quasi-random or random variation in the assignment of peers to individuals as in Imberman et al. (2012) and Carrell and Hoekstra (2010), and Feld and Zölitz (2017), Duncan et al. (2005) and Kremer and Levy (2008) respectively. The latter two examples use the random assignment of roommates to identify peer effects and find significant effects of peer drug and alcohol consumption on own

[3] While we focus on social-networks effects on risky behaviors, Sacerdote (2011) reviews the broader literature on peer effects in educational contexts, such as Hoxby (2000), Sacerdote (2001), and Angrist and Lang (2004).

use in college, while Feld and Zölitz (2017) use the random assigment of students to different sections at a university level, analyzing the effect of "peer quality" (identified by past GPA) on student performance. The authors find that even though there is a small but significant effect associated with higher performing peers, low-achieving students are actually worse off when sharing a section with high performance peers. Imberman et al. (2012) and Carrell and Hoekstra (2010) find that exposure to students who exhibit behavior problems leads to increased behavioral issues for their peers. Card and Giuliano (2013) and Richards-Shubik (2015) structurally estimate models of peer interactions around sexual initiation using self-reported networks.

Card and Giuliano (2013) write that one limitation of studies focusing on the random assignment of peers to individuals is that, because these peer relationships are formed primarily due to exogenous factors, any resulting peer effects on risky behaviors might not reflect those found in friendships that form more organically. Paluck et al. (2016) overcome this by surveying the entire student bodies of 56 schools to assess students' social networks and randomize an anti-bullying intervention. They find that highly-connected students had outsize effects on changing social norms in schools.

We develop an alternative network measure that sits between these research designs and has several advantages and disadvantages. First, in terms of the former, using administrative data on truancy has the advantage of reflecting networks based on the exhibited behavior of interest, which may be more pertinent to risky behaviors than general friendship networks, randomly assigned peers, or measures based on self-reported behaviors. Second, collecting secondary administrative data is typically lower cost than primary-data collection. Finally, Marmaros and Sacerdote (2006) show that proximity and repeated interactions, which is likely to occur for students who share the same classes, are strong predictors of long-term friendships. The disadvantages are that we place restrictions on how we define social networks by using class schedules, and we cannot be certain students are actually coordinating their absences. To test the latter, we simulate random networks under the null hypothesis that students do not coordinate their absences. We find that our observed measures of joint absences occur more frequently than what would be expected by chance under our chosen data-generating process. The latter may still have limitations, so we show our networks our meaningful by demonstrating that a randomly-assigned intervention exhibits spillovers along the observed networks. In this way, our paper relates to the study of peer influence in the context of a randomized intervention, as in the adoption of health and agricultural technologies (Foster and Rosenzweig 1995; Kremer and Miguel 2007; Conley and Udry 2010; Foster and Rosenzweig 2010; Duflo et al. 2011; Oster and Thornton 2012; Dupas 2014; Kim et al. 2015), the role of social interactions in retirement plan decisions (Duflo and Saez 2003), the adoption of microfinance Banerjee et al. (2013), and education technology adoption (Bergman 2016).

Another literature considers the optimal allocation of treatment assignments (cf. Bhattacharya and Dupas 2012) and the assignment of peers to individuals in the presence of potential peer effects (Bhattacharya 2009;

Carrell et al. 2013; Graham et al. 2014). Carrell et al. (2013) use insights from Bhattacharya (2009) and Graham et al. (2014) to optimally assign peer groups in the United States Air Force Academy. Their findings suggestion caution when optimally assigning peers; their intervention actually reduced performance, which was likely due to subsequent, endogenous peer-group formation. Our paper is related, but rather than optimally assigning peer groups, we consider the targeting of an intervention across existing peer groups. Nonetheless, caution is warranted as individuals could substitute joint absences with one friend with coordinated absences among their other friends.

Finally, our paper also contributes to an emerging literature on partial-day absenteeism by estimating direct and spillover effects of an intervention on class attendance in contrast to full-day attendance. Similar to Whitney and Liu (2017), we show that partial-day absences are more common than full-day absences. This makes estimating the effects of interventions on attendance at the class-level of particular relevance; relatedly, Liu and Loeb (2017) show that teachers can impact class attendance as well.

The rest of the paper proceeds as follows. Section 2 details the background of the original experiment and the data used for constructing the spillover analysis. Section 3 describes the social networks in each school and its measurements, whereas Section 4 shows the results for the spillover analysis. Section 5 refers to the allocation algorithm for optimal targeting, and finally, Section 6 concludes.

2 Background and Data

The experiment, which refers to the original study of the direct effects of parent alerts, took place in 22 middle and high schools during the 2015-2016 school year in Kanawha County Schools (KCS), West Virginia.[4] West Virginia ranks last in bachelor degree attainment and 49th in median household income among US states and the District of Columbia.[5] KCS is the largest school district in West Virginia with over 28,000 enrolled students in 2016. The district's four-year graduation rate is 71% and standardized test scores are similar to statewide proficiency rates in 2016. In the school year previous to the study, 2014-2015, 44% of students received proficient-or-better scores in reading and 29% received proficient-or-better scores in math. At the state level, 45% of students were proficient or better in reading and 27% were proficient in math. 83% of district students are identified as white and 12% are identified as Black. 79% of students receive free or reduced-priced lunch compared to 71% statewide.[6]

The district uses a single gradebook system for teachers. Schools record by-class attendance and teachers mark missed assignments and grades using the same web-based platform. The Bergman and Chan (2017) study used data from this platform to create and test a text-message alert system to inform parents about

[4]This description closely follows that of Bergman and Chan (2017).
[5]American Community Survey one-year estimates and rankings by state can be found here.
[6]These summary statistics come from the state education website, which can be found here.

their child's academic progress. That study tested three types of parent alerts: Low-grade alerts, missed assignment alerts, and by-class attendance alerts. On Mondays parents received a text-message alert on the number of assignments their child was missing (if any) for each course during the past week. These assignments included homework, classwork, projects, essays, missing exams, tests, and quizzes. On Wednesdays parents received an alert for any class their child had missed the previous week. Lastly, and normally on the last Friday of each month, parents received an alert if their child had a cumulative average below 70% in any course during the current marking period. Each alert was sent at 4:00 P.M. local time and the text of each alert is provided in Table 1. The text messages also included a link to the website domain of the parent portal, where the parent could obtain specific information on class assignments and absences if necessary.

2.1 Original Experimental Design

The sample for the original experiment began with approximately 11,000 households with roughly 14,000 students who were enrolled in grades five through eleven during the end of the 2014-2015 school year. The parent or guardian of 1,137 students consented to participate in the experiment studying the effects of the alerts during the following school year, 2015-2016.

Among consenting families, random assignment was clustered at the school-by-grade level. The data were collapsed at the grade-by-school level and randomization was subsequently stratified by indicators for below-median grade point average (GPA) and middle versus high school grades. The intervention began in late October 2015 and continued through the remainder of the school year.

Parents in the control group received the default level of information that the schools and teachers provided. This included report cards that are sent home after each marking period every six to nine weeks along with parent-teacher conferences and any phone calls home from teachers. As discussed above, all parents had access to the online gradebook. Figure 1 shows a diagram of the experiment randomization. Note that we have administrative data on *all* students—those who consented and those who did not—which allows us to identify networks and spill over effects from consenting, treated students, onto all other students.

The parent alerts caused significant (40%) reductions in course failures and increases (17%) in by-class attendance. For further details on the experiment and the direct effects of the intervention, see Bergman and Chan (2017).

2.2 Data

Data for this study come from the electronic gradebook described above and baseline administrative data for students enrolled in grades 6 through 12 during the 2015-2016 school year. The administrative data record students' race and gender as well as their suspensions and English language status from the previous school year. We code baseline suspensions as an indicator for any suspension in the previous school year.

Importantly, we have de-identified data for *all* students in the district, whether they participated in the original experiment or not. The gradebook data were available at baseline and endline, and record students' grades and class-level attendance by date. We use these data to construct measures of how many classes students attended after the intervention began as well as the number of courses they failed in the second semester of the year and their GPA. Lastly, we define retention as an indicator taking any courses post treatment.

3 Network Measurement and Descriptive Statistics

In this study, we define the pertinent social network as the ties between students in the same school who miss the same class on the same day.[7] The strength of the tie (or edge) between students is given by the number of times they have missed the same class together. We can formulate this network as follows. Consider a table of students' class attendance in one school over the course of the year in which students' attendance by class, by day, is indicated by a '1' or '0' as follows

Student	Class 1 day 1	Class 2 day 1	Class 1 day 2	Class 2 day 2	…
Student A	1	1	0	1	⋯
Student B	1	0	0	1	⋯
Student C	1	1	0	0	⋯
Student D	1	1	1	1	⋯
⋮	⋮	⋮	⋮	⋮	⋯

We use these data to create a matrix of student attendance:

$$A_{N\times C} = \begin{bmatrix} 1 & 1 & 0 & 1 & \cdots \\ 1 & 0 & 0 & 1 & \cdots \\ 1 & 1 & 0 & 0 & \cdots \\ 1 & 1 & 1 & 1 & \cdots \\ \vdots & \vdots & \vdots & \vdots & \ldots \end{bmatrix}$$

Here, N is the number of students and C is the total number of classes times days in a year. We can then formulate a matrix of who skips class with whom by multiplying A by A':

[7] We do not have information whether these students missed class together coordinately or randomly.

$$S = AA'_{N\times N} = \begin{bmatrix} 1 & 1 & 0 & 1 & \cdots \\ 1 & 0 & 0 & 1 & \cdots \\ 1 & 1 & 0 & 0 & \cdots \\ 1 & 1 & 1 & 1 & \cdots \\ \vdots & \vdots & \vdots & \vdots & \cdots \end{bmatrix} \begin{bmatrix} 1 & 1 & 0 & 1 & \cdots \\ 1 & 0 & 0 & 1 & \cdots \\ 1 & 1 & 0 & 0 & \cdots \\ 1 & 1 & 1 & 1 & \cdots \\ \vdots & \vdots & \vdots & \vdots & \cdots \end{bmatrix}'$$

S is an $N \times N$ matrix where each cell s_{ij} represents how many times student i skipped class with student j. This number represents the strength of the tie or edge between students.

Figure 2 shows an example of this network for one of the schools during the pre-intervention period. In this figure each node (or vertex) represents a student, and the edges between vertexes represent the bond between students: The thicker the edge the stronger the bond, which means students have missed more classes together. In this network we can also observe a certain level of clustering, which indicates a group of students primarily skipping classes with other students in the group.

The main advantage of this network approach is that we have complete administrative attendance data at a disaggregated level, which allows us to construct all the connections between students with respect to attendance. However, we do not have information on the reason for the students' absences, which makes connections occurring from a random shock a concern. We describe how we address this concern below.

For simplicity we focus attention on the peer in students' networks with whom they skip the most class (if there exists such a peer) and their associated characteristics and spillover effects. Given that their strongest peer is the one with whom a student skipped the most classes simultaneously, we expect that spillovers would be larger through this connection than through other weaker ties. All results that follow generally become much weaker and more imprecise when we explore weaker ties (results available upon request).

3.1 Descriptive Statistics and Testing for Coordination

To analyze the social networks in each school in the absence of the intervention, we use baseline data from the beginning of August until the treatment began at the end of October to construct the networks in each school. Table 2 shows baseline summary statistics of the sample. Most students in KCS identify as white and 13% of students identify as Black. Additionally, 50% of students identify as female. Reflecting the student population, few students (2%) are classified as English-Language Learners, and 19% of the sample had at least one suspension in the past year. 4% of the sample was treated and 3% has a peer who was treated.

We also present several network-level measures that we can compare to our simulated measures: size of the network, clustering coefficients and degrees of centrality (Table 3). The size of the network relates to the number of nodes that are in the network, on average, which in this case corresponds to the average number of students by school that skip class with another student. The average clustering coefficient refers to the number of closed triplets over the total number of triplets in a network (Jackson 2008).[8] Due to the fact that edges have different weights in our network, which represent the number of absences between students, we use a weighted average of the clustering coefficient using both an arithmetic and geometric mean to consider the weights of a triplet (Opsahl and Panzarasa 2009).

Degrees of centrality refer to the number of edges between nodes or vertexes. In our case, the degree of centrality of a student is the number of connections with other students, while the eigenvector centrality is the measure of centrality proportional to the centrality of their neighbors (Jackson 2008). Table 3 shows estimates of clustering and centrality in the observed school-level networks.

To assess the extent of homophily within the networks—whether students tend to skip class with other students who have similar characteristics to themselves—we regress students' own characteristics on the characteristics of the peer with whom they skip the most class. Specifically, we estimate the following:

$$\text{characteristic}_i = \beta_0 + \beta_1 \text{characteristic}_{ij} + \varepsilon_i$$

In which j is a peer of i, and j indexes the rank of this peer in terms of joint absences. For instance, j equal to 1 indicates the student with whom i has missed the most class. We focus on j equal to 1 for this analysis.

Table 4 shows the results of this analysis. Across measures, the characteristics of students strongly correlate with the characteristics of their peers. GPA, gender, race, and suspensions all strongly predict these characteristics in their peers[9].

We benchmark these results, including the aggregate network measures, by constructing placebo networks or randomly generated networks. These networks are constructed for each school by randomly generating absences for each student i according to their probability p_{ijd} of absence during the baseline period for class j and day of the week d; each student's absent rate by class and day of the week is independent of each other and uniformly distributed according to their predicted baseline probability of absence p_{ijd}.[10] We ran 100 simulations per school to create random networks for the pre-intervention period where students randomly skipped classes based on their predicted probability from the observed baseline attendance data. With these data, we generate a distribution of the measures of the network.

Table 5 shows the average size of the network by school and measures of clustering, in addition to average

[8]A triplet is defined by three nodes connected by two (open) or three (closed) edges. A closed triplet refers to three nodes that are directly connected by three edges.

[9]Correlation characteristics between students and his/her closest peer at the middle school and high school level are very similar, with the exception of baseline GPA, which is 0.17 vs 0.28 for middle school and high school, respectively. Detail results are available upon request

[10]Predicted probabilities p_{ijt} are obtained using a linear probability model including fixed effects by student, class id, and day of the week.

degree of centrality for the nodes in the random networks. From Table 5 we can observe that the randomly generated networks are not only similar in terms of the size of the networks, but also the level of clustering is very similar between the observed network and the simulated random networks.

Estimating the same measures on the simulated data, we observe that the level of homophily in the random network is smaller compared to that in the observed networks for baseline GPA and gender, and somewhat larger for race and ever suspended status. This indicates that contextual factors, such as tracking students by previous performance or clustering by behavior and race in specific classes, could drive a share of the observed homophily we found in the previous regressions. However, this is less true for student gender. Students in the observed networks are more likely to skip class with another student of the same gender, but the simulated networks show otherwise (Table 4 and 6).

In terms of discerning whether these networks are meaningful or are simply artifacts of district tracking policies and correlated shocks within networks, the results found here are somewhat mixed. Aggregate network characteristics actually match those from our randomly generated network well, but certain measures of homophily substantially differ. We test the significance of the correlations derived from the regressions more formally by using the 100 simulations to compute placebo regression coefficients. This parametric bootstrap approach allow us to discern whether the empirically observed coefficient is significantly different from that found in the distribution from the simulated data, by comparing the distribution of the simulated coefficients to the observed one. We find that we can reject the null hypothesis for two of the four characteristics, with a p-value < 0.01. The observed coefficients for GPA and gender characteristics are larger than the maximum value obtained from the simulations, however we find no significant difference in terms of race or ever-suspended status. Figure 4 shows the distributions for the simulated coefficients, as well as the coefficient obtained from the observed data.

Additionally, we test whether students systematically coordinate their absences in a similar fashion. We examine the number of times students miss class with their strongest peer and compare this to the distribution of absences for this student pair in the simulated data. For each simulated network, we constructed the joint absences for each pair of students, which gives us a distribution under the null hypothesis of uncoordinated absences holding each students' individual absence rate by class and day of the week constant. We then calculate the p-value for the test that absences are uncoordinated based on the observed number of classes that student pairs skip together compared to that found under the null distribution. If students skip class randomly and do not coordinate their absences, we should not be able to reject the null hypothesis. However, if student i coordinates their absences with student j, then the observed joint absences would be on the right tail of the distribution, allowing us to reject the null hypothesis for that particular student pair.

Table 7 shows the total number of students who have a strongest peer, and the number of those students who coordinate their absences according to our parametric bootstrap approach using different thresholds. Almost 50% of the students who have a strongest peer coordinate their absences (at a 90% threshold level) compared to our simulated networks. This share is well above what we would expect by chance, given our data generating process.

Lastly, to get a sense of the magnitude of coordination, we compare the share of a student's absences with their closest peer to the average share of absences across their other peers. Students are absent 4.7 times more often with their the closest relative to the average across their other peers.

Overall, we find significant evidence that students coordinate their absences as well as evidence of homophily within the networks. However, several limitations of our network remain. There are certain contextual aspects we cannot observe, such as the time of the day students skip class, or whether other events might account for coordinated absences (e.g. sport events or class activities). Thus, to further assess the importance of networks and attendance, in the following section we analyze whether treatment effects from the alert intervention spill over onto peers within a student's baseline attendance network.

4 Network Spillovers

4.1 Treatment effect spillovers from strongest peer on attendance

To assess spillovers, we use the baseline attendance data—data collected prior to any intervention implementation or random assignment—to construct school-level networks. Note, we have data on all students—both those who participated in the experiment and those who did not. The key characteristic that we use from these networks is an indicator for whether a student's strongest peer, which we define as the peer with the strongest tie to an individual, referred to as *Peer 1*, was treated or not. This helps answer the question: if the person you skipped the most with is treated, does this affect your attendance as well?

We estimate the following equation to examine peer effects:

$$\mathrm{y}_i = \beta_0 + \beta_1 \mathrm{P1Treat}_i + \beta_2 \mathrm{Peers}_i + \beta_3 \mathrm{D1}_i + \gamma_i X_i + \varepsilon_i \tag{1}$$

In this equation, the key outcome of interest, y_i, is the number of classes attended after the intervention began, though we also check for effects on other gradebook outcomes such as course failures and GPA. $\mathrm{P1Treat}_i$ is an indicator for whether the strongest peer is treated. All regressions control for the variable Peers_i, which is the number of peers with whom student i has skipped class, as well as an indicator $\mathrm{D1}_i$ which accounts for whether the student has skipped class with at least one other student. Additionally, we include indicator variables for whether the student and their closest peer were in the original experimental sample. These variables are important as they determine the probability of treatment.[11] All regressions also include the original strata from the treatment assignment. The X_i include additional controls specified in the original experiment's pre-registered analysis plan for the student and his or her closest peer. These variables are indicators for race, gender, suspension in the past year and IEP status, as well as baseline attendance and GPA. Our preferred specification shows results controlling by covariates as these greatly improve precision. P-values considered the cluster design at the school-by-grade level, which is the original unit of treatment assignment.[12]

To test for heterogeneous peer effects, we interact the P1Treat_{i1} with baseline academic and demographic covariates for student i. Similarly, we also examine heterogeneity by measures of centrality of the strongest peer as well, such as eigenvector centrality and the number of students for whom Peer1_i is the closest peer to. This last measure would give us an indication of how many students are connected to Peer1_i.

One important challenge in our analyses is taking into account the small number of network clusters (i.e. 22 schools), particularly given the small share of treated students within each cluster, which may bias cluster-robust standard errors. The latter rely on the number of clusters going to infinity for accurate inference. For that reason, we use a randomization inference approach to obtain accurate p-values for our spillover effects under sharp null hypothesis. Following Athey et al. (2018) (AEI), we randomly choose half of the clusters to be our "focal" units, and then randomly assign the non-focal clusters to either treatment or control maintaining the observed number of clusters in each group according to the observed experiment.[13] Then, as in Athey et al. (2018), we construct the observed test statistic as the covariance between the P1Treat variable for our focal group and the residual of our preferred specification minus that variable. The idea is that if there is little spillover effect, then this test statistic would be close to 0. [14] Finally, we compare our observed test statistic with the distribution of test statistics under the null of no spillovers using 2,000 draws.[15].

Lastly, if this research design is valid, we should see that P1Treat_{i1} is uncorrelated with baseline characteristics of students conditional on the Peers_i variable. Table 8 shows the result of estimating equation (1) with baseline covariates as the dependent variable. The magnitudes are all small and statistically insignifi-

[11] As a robustness check, we also incorporated flexible interactions of Peers_i variable with the P1Treat_i treatment variable, and our results are extremely similar (available upon request).

[12] Moreover, students' own grade level is a near one-to-one predictor of their strongest peer's grade level; the coefficient on a regression of own grade level on their strongest peer's grade level is 0.96.

[13] The allocation of clusters to the focal group was random, based on the number of network edges between clusters in an adaptation of a *2-net* approach. See Athey et al. (2018) for more details.

[14] While Athey et al. (2018) find this test statistic is more powerful for detecting treatment effects, our results hold if we simply use the regression coefficients as the test statistic; we find the power gains are quite small in our context (results available upon request).

[15] For estimating the p-values for the direct effect under sharp nulls, we followed the same AEI procedure, but keeping the allocation of Peer1 fixed for the focal group and randomizing the treatment allocation for the students to obtain the distribution under the null hypothesis.

cant, particularly around baseline absence measures, which provides reassurance that peer treatment status is randomly assigned.

4.2 Results

Attendance

Given the networks are constructed based on class absences, we focus on whether there are spillover effects of the treatment on students' by-class attendance and the robustness of these effects to different measures of peers. To test whether there are spillover effects on attendance for students whose strongest peer was treated, we estimate two models that build upon each other: (1) a simple regression between $\mathrm{P1Treat}_1$ and attendance controlling for the size of the network and whether the students and his closest peer was in the original experiment sample and (2) we then add controls for the set of predefined covariates described above.[16]

Table 9 shows the results, using randomization inference for estimating p-values. Both models yield stable and positive spillover effect of treated students onto their strongest peer. The estimated spillover effect is 24 more attended classes (note that, as described previously, classes are not the same as days of school). We can reject the sharp null hypothesis of no spillover effect with a p-value < 0.01 (see also Figure 3a and 3b). This is 72% of the direct treatment effect (ITT) of the intervention on classes attended found in Bergman and Chan (2017), and 8% of the mean for control students for those who were not treated and did not have their closest peer treated. For comparison, Avvisati et al. (2013) examined the spillover effects of a parent-engagement intervention in which a share of students in a classroom were treated. In that setting, students remained with the same group of students throughout the day. They found spillover effects on attendance roughly 50% of the size of the direct (ITT) effect of the intervention.

We also analyze how the joint attendance between a student and their strongest peer changes during the post-intervention period. Table 10 shows the results of these analyses. If students derive utility from a joint absence or a joint attendance with a particular peer, we could observe that joint *attendance* with their strongest peer increases if that peer is treated. Students may also reallocate their attendance to be spend more time with their closest peer and less time with other, less-connected peers. Panel A shows the effect of having a student's strongest peer treated on the number of classes attended with that peer. Panel B shows the effect of having a student's strongest peer treated on the number of classes attended with all other students, excluding the strongest peer. The results show that there is an increase in attendance with the closest peer, which is consistent with the idea that students derive utility from jointly attending (or skipping) class with a particular peer; however, we do not have enough precision to rule out a zero effect.

[16] The size of the network is defined as the number of peers with whom a student skipped classes.

We do find a small but significant increase in attendance with other peers other than their closest one, which could indicate that overall students increase their attendance, and they do not just substitute their absences with other peers.

4.2.1 GPA, Course Failures and Retention

Finally, we also analyze whether spillovers from treated peers extend beyond attendance to other measures of academic performance: GPA, number of failed courses, and dropout. Table 12 shows the results for these outcomes.

For inference, we again used the randomization inference approach of Athey et al. (2018) as we did for the attendance outcomes. We do not find evidence of a significant effect on GPA, failed courses or retention, though we may be under-powered to detect effects on academic outcomes other than attendance.

Heterogeneity

We examine how these treatment effects vary by network and demographic characteristics. We explore heterogeneous effects by gender, race, academic performance, suspension status, baseline absences,[17] centrality in the network of their peer,[18] and whether their closest peer is the closest peer to more than 2 students. Table 11 shows the results for heterogeneous effects. We find that spillovers seem to be larger for Black students and also for those students who have a closest peer who is central in the network. Students who were ever suspended and who have a higher number of baseline absences seem to experience smaller spillovers.

Lastly, we follow Athey and Imbens (2016) to identify potential heterogeneity in the spillover effects while mitigating the threat of "data mining." Athey and Imbens (2016) use machine learning techniques to identify groups of students within the data who experience differential spillovers. Table A.3 shows the results for the spillover effects under heterogeneity. We find that one subgroup, those with a low baseline absence rate, have a higher spillover effect than others in the sample (significant at the 10% level).

In results not shown, we also analyzed whether there are second-degree spillovers. If peer 1 is a student's strongest peer, we define second-degree spillovers as spillovers stemming from whether or not the strongest peer to peer 1 is treated or not. We find no significant effects.

Robustness

In the appendix, we consider the robustness of our spillover measure to a stricter definition of coordinated absences. We generate an indicator that equals one if the strongest peer measure is significant at the 10% level according to our parametric bootstrap.[19] While more than 40% of the sample have joint absences that

[17] A student is considered to have a high percentage of baseline absences if it is higher than the median for all students with at least one absence.

[18] We define a student as "central" if, according to their eigenvector centrality, they are on the top 50% of the distribution.

[19] We also constructed the same variable for a 50%, 40%, 30%, and 20% threshold.

pass this test, very few of these students are treated, so the estimates are much less precise than before. Table A.1 shows the results of spillovers for peers that are treated and coordinate their absences according to our bootstrap sampling. Despite larger standard errors, we find that the point estimates are slightly larger than before, though the results are not statistically significant at conventional levels.

Lastly, if a handful of students miss many classes, this may generate some extreme baseline values for the Peers_i variable, which is the number of individuals with whom a student missed class. We examine the robustness of our effects by dropping observations whose baseline, pre-random assignment Peers_i value is more than three standard deviations away from the mean. Table A.2 shows the effects are still significant and very similar in magnitude.

These results contrast to results using another measure commonly used as a proxy for networks in clustered-randomized controlled trials. This alternative strategy for measuring spillovers compares the untreated students in a treated cluster to another cluster that is completely untreated. This occurs, for instance, if fractions of a classroom are treated and some classrooms are untreated. This strategy can be effective in settings where students may change classrooms during the day but they do so with the same group of students.[20] In the United States, many high-school and middle-school students change classrooms and the student composition of the classrooms may change as well. Students do tend to remain within grades however, and Bergman and Chan (2017) analyze within-grade spillovers using this design, but find no evidence spillover effects.

These findings show that our network measures are meaningful not only in the sense that students coordinate absences, as shown in the previous section, but also because there are meaningful spillover effects that occur along this network as well.

5 Targeting of treatment and Cost Effectiveness

We show the implications of these spillovers for targeting the intervention and a basic accounting exercise to measure cost effectiveness. For the latter, the cost of the learning management software, training, and text messages is $7 per student. Without accounting for spillovers, the cost per additional class attended given the number of students treated and the intent to treat effects found in Bergman and Chan (2017) is $0.21. Incorporating the average spillover effect given the number of students with their closest peer treated, the cost per additional class attended falls by 43% to $0.12.

We next assess the extent which we can leverage the social network information to target the treatment more cost effectively. This is primarily a conceptual exercise, as this particular treatment has low marginal

[20] For instance, Avvisati et al. (2013) use this measure in an experiment aimed to involve parents in their education, and they find evidence of spillover effects.

cost, but other evidence-based absence interventions cited above (e.g. Check and Connect) cost thousands of dollars per treated student. We solve for a targeted allocation of the intervention given the direct effects and spillovers previously estimated, subject to a cost restriction. We represent the budget restriction as a maximum number of students that can be treated. Given that most policy-relevant interventions are subject to a budget constraint, we aim to find the maximum impact on class attendance subject to the number of possible students who can be treated. We could also consider objectives as well in which we aim minimize the number of chronically absent students, which could be an appealing objective for schools and policymakers.

We make the following assumptions to simplify this problem:

1. No general equilibrium effects. We assume that the direct and spillover effects do not change with the share of treated students. This assumption is plausible when small shares of students are treated, but could be violated when the proportion of treated students increases.

2. Homogeneous effects within types of students. To simplify our model and make it computationally feasible, we consider heterogeneous effects on a particular set of characteristics (specified below), and assume the effects and spillovers are constant with respect to other individual and school characteristics not considered in the optimization model. This assumption may not hold in all settings, and other relevant characteristics should be included in the analysis depending on the context, increasing the types' of students considered.

3. Spillover effects only occur through a student's closest peer. Empirically, we did not find significant spillover effects beyond a student's closest tie, so we assume that peers that have weaker ties to a student have a negligible spillover effect on that student's attendance.[21] Formally, let $\mathbf{PTreat_i}$ be a vector indicating the treatment statuses of student i's J peers, where J is ordered by the strength of ties to student i such that $j = 1$ indicates with whom student i skips the most class. We assume that

$$\mathbf{PTreat_i} = \text{PTreat}_{i1}$$

4. No school-boundary considerations. For simplicity, we present the optimization model for the total population of the 22 schools in our sample, without considering allocation restrictions within schools. Adding restrictions within schools for student allocations is straightforward and can be implemented by either solving the same model for each school, or adding a school variable interacted with the type of student characteristic.

[21] We previously defined ties between peers as the number of joint absences.

Maximizing the Effect of the Intervention

We begin by setting up the objective function as the maximization of the effect of the intervention on class attendance, irrespective of the distribution of this effect across students. Let $I = \{1, 2, .., n\}$ be the set of students that could potentially be treated, and, as defined by Sviatschi (2017), let A be a $n \times n$ matrix for effects and spillovers. In this case, each off-diagonal cell (i, j), where $i \neq j$, contains the spillover effect of student i on student j, and the elements on the diagonal, (i, i), represent the direct effect of the treatment of student i[22].

In order to set up our maximization problem, we need to define our closest peer matrix D as well. This network will contain an indicator $d_{ij} = 1$ if student i's closest peer is student j, and 0 otherwise[23]. The definition of this matrix can be easily obtained from the social network matrix S previously defined, by taking an indicator function to each row vector and applying it to the maximum number of joint absences.

In this case, the decision variables are z_i $(i \in I)$ and m_{ij} $(i, j \in I;\ i \neq j)$, where z_i is a binary allocation variable that indicates whether the treatment is assigned to student i, and m_{ij} is an indicator variable for treatment assigned to student j, and he/she is student's i closest peer $(m_{ij} = z_j \cdot d_{ij})$.

The objective function we maximize corresponds to the function that optimizes the allocation of the treatment given the direct effects and spillovers we estimated, subject to the budget restriction of treating at most b students as follows:

$$\begin{aligned}
\max_{\mathbf{z},\mathbf{m}} \quad & \sum_{i \in I} a_{ii} z_i + \sum_{i \in I} \sum_{j \in I; i \neq j} a_{ij} m_{ij} \\
\text{s.t.} \quad & m_{ij} = z_j \cdot d_{ij} && \forall i, j \in I; i \neq j \\
& \sum_{i \in I} z_i \leq b \\
& z_i \in \{0, 1\} && \forall i \in I \\
& m_{ij} \in \{0, 1\} && \forall i, j \in I; i \neq j
\end{aligned}$$

In order to estimate the effects matrix A and the potential heterogeneity in spillover effects, we used a machine learning approach proposed by Athey and Imbens (2016) to identify different "types" of students according to baseline characteristics. We define $t = 1, ..., T$ as the different types of students that are identified through this approach.

Then, the total effect exerted by student i (who is type t), would be:

$$E_i = a_{ii} z_i + \sum_{j \in I; i \neq j} a_{ij} m_{ij}$$

[22] One key difference with Sviatschi (2017) is that we do allow for spillovers onto students that are also directly treated

[23] Note that because we are only choosing one closest peer, $\sum_{i \in I} D_{i \cdot} = 1$

$$E_i = d_t z_i + \sum_{t \in T} s_t n_{it} z_i = z_i(d_t + \sum_{t \in T} s_t n_{it})$$

Where d_t is the direct effect for treated students of type t, s_t is the spillover effect that a student of type t would experience if his/her closest peer was treated, and n_{it} is the total number of students type t for whom student i is a closest peer, meaning $n_{it} = \sum_{j \in t, j \neq i} d_{ij}$.

Thus, it is easy to see that we can re-write the previous optimization problem as following:

$$\begin{aligned}
\max_{\mathbf{z}} \quad & \sum_{t \in T} \sum_{i \in I_t} z_i (d_t + \sum_{k \in T} s_k n_{ik}) \\
\text{s.t.} \quad & \\
& \sum_{t \in T} \sum_{i \in I_t} z_i \quad \leq \quad b \\
& z_i \quad \in \quad [0, 1] \qquad \forall i \in I
\end{aligned}$$

The budget restriction is given by b, which represents the maximum number of students that can be treated given our budget constraint. Vectors d and s represent the direct effects and spillovers for each type of student. Thus, the objective function sums the direct effect of the treatment in terms of the number of present days with the spillover effects that the treatment has on other students, considering the number of students who have the most connections.

To illustrate this algorithm, we consider the following groups of students: students who do not have a closest peer (i.e. have not missed class with another student), referred to as NP, and the subgroups identified by the machine-learning analysis proposed by Athey and Imbens (2016) described above: students with baseline absence rates less than 1.5% (Group 1) and students with baseline absence rates greater than 1.5% (Group 2). This analysis could easily be extended to more groups if they are relevant in other contexts. These characteristics define three different types of students, $T = \{NP, P1, P2\}$. Each of these types of students connect to other types of students. The types of students can be identified considering other relevant characteristics that affect the magnitude of the effects or spillovers, or students whom the district would otherwise like to target. The same logic above would apply as well.

For our particular example, we define our objective function as the maximization of the overall effect on our population. Given that all types of students have the same direct effect and that students with lower baseline absenteeism rates have higher spillover effects, we first treat students in Group 1. From this specific group, though, we also want to target first those students who have the most connections, meaning that they are other students' closest peer, and, thus, that would exert the largest overall effects.

Figure 6 shows the total effect of the treatment when targeted, as well as the total effect of the intervention (given our model) for the observed experiment. We compare this optimal targeting strategy to a commonly used rule of thumb, such as targeting students with the highest absenteeism rates[24]. We can see that when the intervention is targeted to 604 students (the number of students treated in our sample), targeting the intervention increases the overall effect by over 5 times. Compared to an alternative targeting mechanism, such as targeting by absenteeism rates, the optimal allocation by effects considering the network structure would double the total effect of the intervention.

This analysis can be further extended to different types of students following the same optimization algorithm and given other relevant characteristics that might affect the magnitude of the spillovers, direct effects, or otherwise prioritized students.

The same analysis can be easily adapted to other objectives, such as the reduction of chronically absent students. In that case, further assumptions should be made about the stability of the absenteeism rates in the post-intervention period, as well as considering how "far" from the chronically absent threshold the student is. For instance, some students that are considered chronically absent might change status if they are close enough to the threshold and their closest peer is treated, without treating them directly. Others, for instance, might need to be treated directly as well as their closest peer.

6 Conclusion

In this paper, we demonstrate a straightforward way to estimate meaningful social networks around a student's risky behavior, truancy. Our method is based on detailed, student-by-class-by-day attendance information for every student, which we use to construct a matrix of attendance for every student within a school.

Our study has several advantages and limitations compared to previous research studying social network effects on risky behaviors. First, one branch of the literature uses the exogenous assignment of peer groups to identify peer effects on risky behaviors, as opposed to naturally occurring friendships. In contrast, Card and Giuliano (2013) use self-reported friendship networks and estimate a structural model to discern peer effects on risky behaviors. Our paper sits somewhat in between these strategies. We use administrative data on the risky behavior, truancy, to construct social networks and combine that with a randomly assigned text-messaging intervention to identify social network effects. This strategy has the advantage of being low cost because it relies on existing data and it is less subject to bias arising from self-reporting. However, the disadvantage is that we cannot be certain students are actually coordinating their absences. We overcome the latter by simulating random networks under the null hypothesis that students do not coordinate their absences. We find that our observed measures of joint absences occur more frequently than what would be

[24]This targeting scheme performs better than random allocation, particularly because students with high absenteeism rates also tend to have more connections to other students, thus exerting more spillovers.

expected by chance under our chosen data-generating process. Lastly, we show that a randomly-assigned attendance intervention exhibits meaningful spillovers along our estimated networks.

We show that these spillover effects are meaningful in terms of their implications for measuring cost effectiveness and targeting to improve efficiency. By accounting for the spillovers, the intervention is 19% more cost effective than not accounting for these effects. Moreover, we show that leveraging baseline network information could help target the intervention and increase its overall effectiveness under different objective functions. Future research could test this targeting via a randomized controlled trial.

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7 Figures

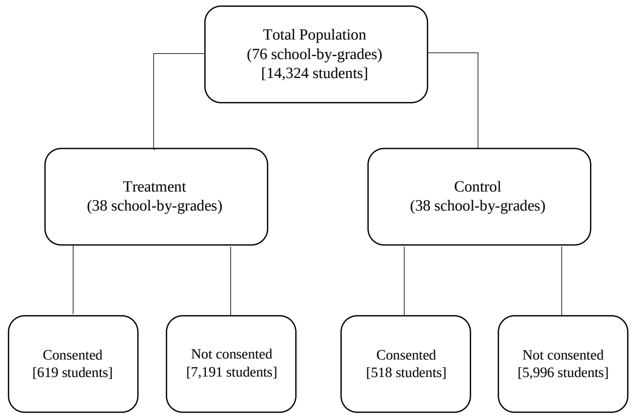
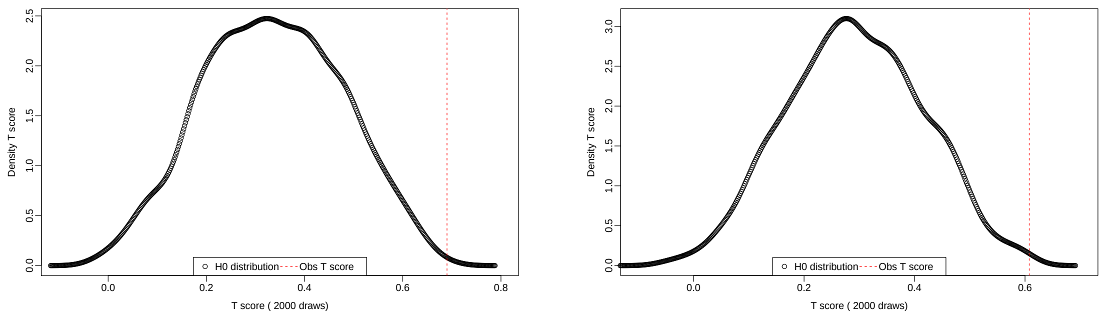


Figure 1: Randomization diagram for experiment

Figure 2: Social Network for School 1 (pre-intervention period)

Notes: Each circle corresponds to a student, and each interconnecting line or *edge* corresponds to the number of absences between two students.



(a) Observed T score and null distribution (2,000 draws) for model with no controls

(b) Observed T score and null distribution (2,000 draws) for preferred model with controls

Figure 3: Randomization Inference Approach for Sharp Null Hypothesis of No Spillover Effect

Notes: Both models include controls by strata, indicators for sample (for both the student and closest peer), number of peers, and an indicator for whether the student had a closest peer or not. Additional controls for preferred model include *GPA baseline, IEP, ELL, missed days (previous year), gender, black,* and *ever suspended.*

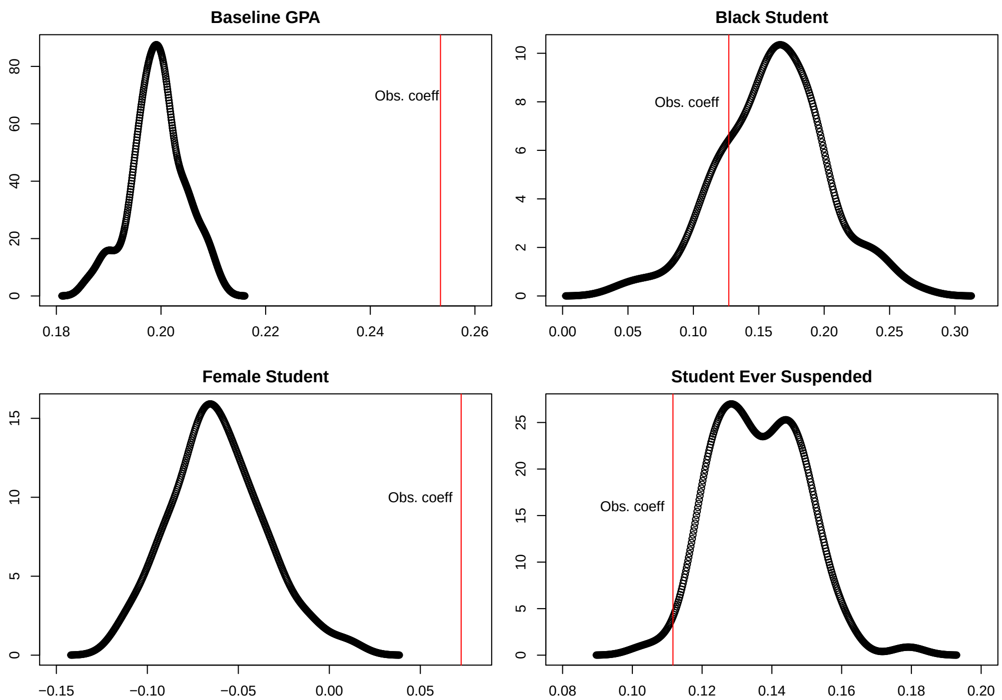


Figure 4: Distribution of simulated coefficients (100 simulations) and observed coefficient for homophily analysis

Notes: Distribution obtained from the coefficients of regressions using only strata fixed effects and clustered errors.

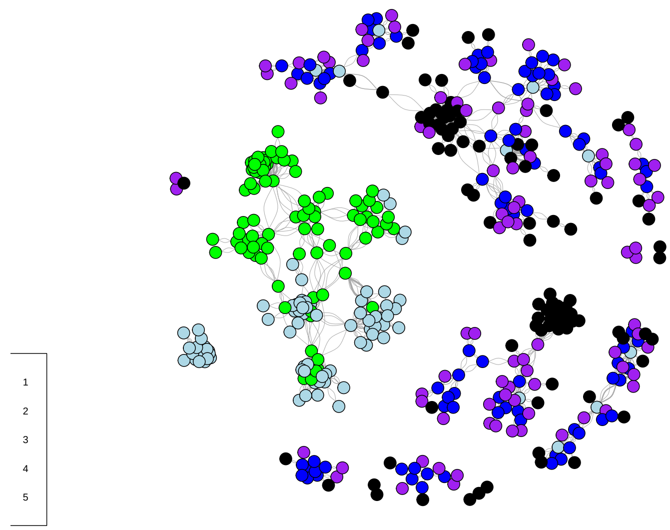


Figure 5: Social Network for School 1 (pre-intervention period) with quintiles of eigenvector degrees

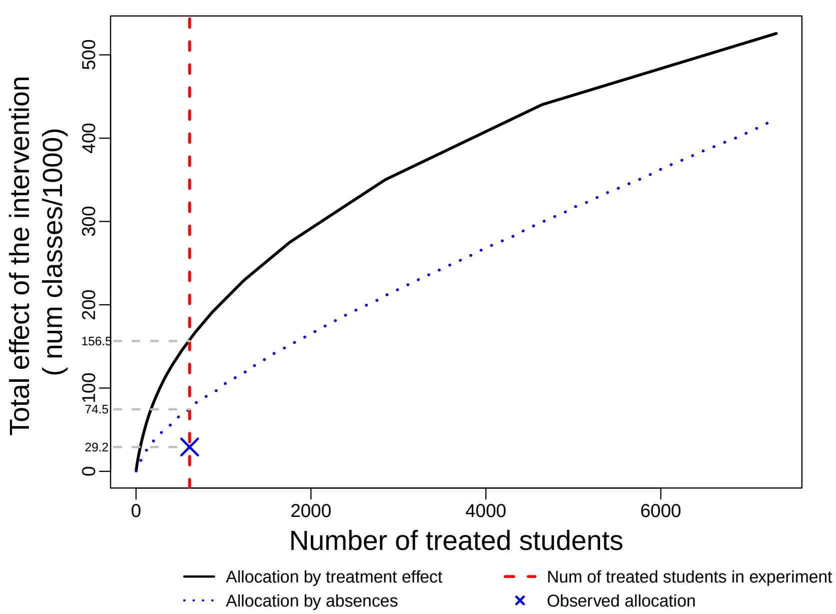


Figure 6: Overall intervention effect under optimal allocation by number of student treated

8 Tables

Table 1: Text messages sent to parents

Alert	Frequency	Message
Low Class Average Alert	monthly	"Parent Alert: [Student Name] has a [X]% average in [Class Name]. For more information, log in to [domain]"
Absence Alert	weekly	"Parent Alert: [Student Name] has [X] absence(s) in [Class Name]. For more information, log in to [domain]"
Missing Assignment Alert	weekly	"Parent Alert: [Student Name] has [X] missing assignment(s) in [Class Name]. For more information, log in to [domain]"

Notes: This figure shows the script for each of the three types of alerts sent via text messages: low class average, absence, and missing assignments.

Table 2: Baseline characteristics for the sample

Variable	Mean	Observations
Female	0.50	13,641
Black	0.13	13,641
KCS: Eng Lang Learner	0.02	12,955
IEP	0.15	12,955
Baseline GPA	2.54	14,653
ever suspended last year	0.01	13,723
Percent of days absent pre-intervention	0.10	14,621
Percent of classes absent pre-intervention	0.14	14,621
Percent of Days Missed	0.06	14,653

Notes: Mean characteristics consider only non-missing observations. Demographic information, IEP status, English Language Learner status and suspension data are from district administrative data. Attendance and GPA are from the gradebook data.
Observations consider school-students.

Table 3: Observed network features

Characteristic	Mean	1st Quartile	Median	3rd Quartile
Average size	606.6	363.0	509.0	759.0
Weighted clustering coefficient				
Arithmetic mean	0.41	0.30	0.43	0.48
Geometric mean	0.41	0.29	0.42	0.48
Centrality				
Average number of edges	39.38	13.55	21.56	65.12
Eigenvector centrality	0.14	0.07	0.11	0.22

Notes: These statistics are constructed using by class attendance data.

Table 4: Homophily in the observed network

Baseline Variable	GPA	Female	Black	Suspended
GPA peer	0.25***			
	(0.01)			
Female peer		0.07***		
		(0.01)		
Black peer			0.13***	
			(0.02)	
Suspended				0.10*
				(0.05)
Observations	11654	10474	11861	11657

Notes: Standard errors in parenthesis. Specification of the models include fixed effects by strata. Peer represents the person with whom a student missed the most class and is constructed using by class attendance data. GPA is from the gradebook data. Demographic information and suspensions are from school administrative data.

Results are similar for middle and high school levels.

* Significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Average simulated (random) network features

Characteristic	Mean	1st Quartile	Median	3rd Quartile
Average size	608.8	364.0	494.7	780.6
Weighted clustering coefficient				
Arithmetic mean	0.40	0.30	0.42	0.48
Geometric mean	0.40	0.29	0.42	0.48
Centrality				
Average number of edges	38.47	14.53	23.26	65.30
Eigenvector centrality	0.13	0.07	0.11	0.21

Notes: These statistics are constructed using simulated network data, with 100 simulations per school.

Table 6: Homophily in the simulated networks

Baseline Variable	GPA	Female	Black	Suspended
GPA peer	0.213***			
	(0.005)			
Female peer		-0.001		
		(0.021)		
Black peer			0.186***	
			(0.035)	
Suspended				0.133***
				(0.016)
Observations	1171055	1043438	1189596	1171233

Notes: Standard errors in parenthesis (clustered by grade-school and student). Specification of the models include fixed effects by strata and simulation. Regressions are estimated using the simulated network data, with 100 simulations per schools.
* Significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: Students with coordinated absences for different thresholds

Variable	Total	% of Sample
Observations in sample (Total)	14,653	100%
Observations with a closest peer	12,740	87%
Observations with coordinated absences		
90% threshold	5,777	39%
95% threshold	4,145	28%
99% threshold	1,821	12%

Notes: Results obtained from parametric bootstrap using 100 simulations of random networks. Data are from the by-class attendance data in the pre-intervention period.
Observations correspond to school-students.

Table 8: Balance between students with a treated peer and without a treated peer

Variable	Control Mean	Treatment-Control Difference	P-value	Observations
Female	0.49	0.03	0.365	13,628
Black	0.10	-0.00	0.988	13,628
KCS: Eng Lang Learner	0.01	-0.00	0.532	12,945
IEP	0.19	-0.03	0.062*	12,945
Baseline GPA	3.02	0.05	0.425	13,628
Ever suspended last year	0.00	-0.00	0.999	12,766
Percent of days absent pre-intervention	0.10	-0.00	0.706	13,596
Percent of classes absent pre-intervention	0.10	-0.01	0.340	13,596
Percent of Days Missed	0.05	-0.00	0.446	13,628

Notes: Specification of the models include fixed effects by strata and clustered standard errors. In the regressions, demographic information, IEP status, English Language Learner status and suspension data are from district administrative data. Attendance and GPA are from the gradebook data.
Observations correspond to school-students.
* Significant at 10%; ** significant at 5%; *** significant at 1%

Table 9: Effect of treated peer on attendance

Variable	Classes present	Classes present
Treat	29.80	31.39*
	[0.12]	[0.06]
P1Treat	21.76***	24.16***
	[<0.01]	[<0.01]
Controls	Strata	All
Observations	12766	12764

Notes: P-values from randomization inference shown in squared parenthesis. The model from column (1) includes fixed effects for strata, an indicator for *treatment* and *sample* for the student and the closest peer, a control for number of peers and whether the student has a closest peer, with no additional controls. Column (2) includes all the variables from column (1) in addition to controls by *GPA baseline, IEP, ELL, missed days (previous year), gender, black,* and *ever suspended* (for both the student and his closest peer). In the regressions, demographic information, IEP status, English Language Learner status and suspension data are from district administrative data. Attendance and GPA are from the gradebook data.

* Significant at 10%; ** significant at 5%; *** significant at 1%

Table 10: Effect of treated peer on joint attendance (classes) post-intervention

Variable	Classes present with P1 (1)	(2)
P1Treat	15.80	16.95
	[0.802]	[0.749]
Controls	Strata	All
Observations	12766	12764

Variable	Classes present *not* with P1 (1)	(2)
P1Treat	5.96***	7.21***
	[<0.01]	[<0.01]
Controls	Strata	All
Observations	12766	12764

Notes: Top panel shows effect of having Peer 1 on joint attendance; bottom panel shows effect of having Peer 1 treated on attendance with other peers that are *not* Peer 1. P-values from randomization inference are shown in square parenthesis. The model from column (1) includes fixed effects for strata, an indicator for *treatment* and *sample* for the student and the closest peer, a control for number of peers and whether the student has a closest peer, with no additional controls. Column (2) includes all the variables from column (1) in addition to controls by *GPA baseline, IEP, ELL, missed days (previous year), gender, black,* and *ever suspended* (for both the student and his closest peer). In the regressions, demographic information, IEP status, English Language Learner status and suspension data are from district administrative data. Attendance and GPA are from the gradebook data.

* Significant at 10%; ** significant at 5%; *** significant at 1%

Table 11: Heterogeneity of effect of treated peer on attendance

Variable	Classes present	Classes present	Classes present	Classes present	Classes present	Classes present	Classes present
Peer Treat (CA)	29.08***	22.91***	38.04***	26.89**	39.57***	18.56*	35.99
	[0.007]	[<0.001]	[0.001]	[0.002]	[<0.001]	[0.077]	[0.176]
Peer Treat (CA) × female	-5.34						
	[0.137]						
Peer Treat (CA) × black		31.28***					
		[0.001]					
Peer Treat (CA) × below med GPA			-20.23				
			[0.380]				
Peer Treat (CA) × suspended				-23.20***			
				[<0.001]			
Peer Treat (CA) × above med absences					-31.28*		
					[0.061]		
Peer Treat (CA) × Peer1 is eigen-central						19.54*	
						[0.100]	
Peer Treat (CA) × Peer1 is closest peer to >2 students							-21.29
							[0.400]
Controls	All	All	All	All	All	All	All
Observations	12764	12764	12762	12734	12764	12764	12764

Notes: P-values from randomization inference in square parenthesis. All models include fixed effects by strata, indicator variables for whether the student and the closest peer belonged to the original experimental sample, and controls by *GPA baseline, IEP, ELL, missed days (previous year), gender, black,* and *ever suspended* for the students and his closest peer. In the regressions, demographic information, IEP status, English Language Learner status and suspension data are from district administrative data. Attendance and GPA are from the gradebook data.
* Significant at 10%; ** significant at 5%; *** significant at 1%

Table 12: Effect of treated peer on other outcomes

	GPA	
P1Treat	-0.01	-0.02
	[0.328]	[0.363]
Controls	Strata	All
Observations	12139	12139
	Courses failed	
P1Treat	-0.11	-0.07
	[0.581]	[0.481]
Controls	Strata	All
Observations	12139	12139
	Drop out	
P1Treat	-0.03	-0.00
	[0.633]	[0.583]
Controls	Strata	All
Observations	13596	13583

Notes: P-values from randomization inference in squared parenthesis. The model from column (1) includes fixed effects for strata, an indicator for *treatment* and *sample* for the student and the closest peer, a control for number of peers and whether the student has a closest peer, with no additional controls. Column (2) includes all the variables from column (1) in addition to controls by *GPA baseline, IEP, ELL, missed days (previous year), gender, black,* and *ever suspended* (for both the student and his closest peer). In the regressions, demographic information, IEP status, English Language Learner status and suspension data are from district administrative data. Attendance and GPA are from the gradebook data.
* Significant at 10%; ** significant at 5%; *** significant at 1%

Appendix A

Table A.1: Effects of treated peer for peers that coordinate absences according to parametric bootstrap

Variable	Classes present	Classes present
Treat	27.61	32.01*
	[0.12]	[0.05]
P1Treat (CA)	25.59	28.44
	[0.44]	[0.14]
Controls	Strata	All
Observations	13657	13655

Notes: P-values from randomization inference shown in squared parenthesis. The model from column (1) includes fixed effects for strata, an indicator for *treatment* and *sample* for the student and the closest peer (90% threshold for coordinated absences), a control for number of peers they coordinate absences with and whether the student has a closest peer with whom they coordinated absences, with no additional controls. Column (2) includes all the variables from column (1) in addition to controls by *GPA baseline, IEP, ELL, missed days (previous year), gender, black,* and *ever suspended* (for both the student and his closest peer). In the regressions, demographic information, IEP status, English Language Learner status and suspension data are from district administrative data. Attendance and GPA are from the gradebook data.

* Significant at 10%; ** significant at 5%; *** significant at 1%

Table A.2: Effect of treated peer on attendance removing outliers (>3 SD)

Variable	Classes present	Classes present
Treat	24.35	27.87*
	[0.14]	[0.08]
P1Treat	18.32*	22.92**
	[0.07]	[0.02]
Controls	Strata	All
Observations	12449	12447

Notes: P-values from randomization inference shown in squared parenthesis. Outliers with joint absences larger than 3 standard deviations from the mean are removed from the sample. The model from column (1) includes fixed effects for strata, an indicator for *treatment* and *sample* for the student and the closest peer, a control for number of peers and whether the student has a closest peer, with no additional controls. Column (2) includes all the variables from column (1) in addition to controls by *GPA baseline, IEP, ELL, missed days (previous year), gender, black,* and *ever suspended* (for both the student and his closest peer). In the regressions, demographic information, IEP status, English Language Learner status and suspension data are from district administrative data. Attendance and GPA are from the gradebook data.
* Significant at 10%; ** significant at 5%; *** significant at 1%

Table A.3: Direct and spillover effects for optimal allocation problem

Variable	Effect
Treatment	31.91*
P1Treat $\times$ Fraction of absences < 0.015	37.00**
P1Treat $\times$ Fraction of absences ≥ 0.015 $\times$ GPA baseline < 2.6	16.06
P1Treat $\times$ Fraction of absences ≥ 0.015 $\times$ GPA baseline $\in [2.6,3.4)$	55.04
P1Treat $\times$ Fraction of absences ≥ 0.015 $\times$ GPA baseline ≥ 3.4	4.91
Controls	All

Notes: Treatment coefficient is obtained from the prefered specification. Spillover coefficients are obtained using Athey and Imbens (2016) approach using the following covariates: *GPA baseline, IEP, ELL, missed days (previous year), gender, black,* and *ever suspended.*
* Significant at 10%; ** significant at 5%; *** significant at 1%