Your child missed learning the alphabet today: A randomized trial of sending teacher-written postcards home to reduce absences

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Abstract

As a growing base of evidence links student success with attendance, many states have chosen to include measures of student absenteeism in their accountability systems under ESSA. Consequently, school districts face strong incentives to improve attendance. We report on a randomized field experiment in which parents of early elementary students received personalized information about the academic content their child missed while absent. Following an absence, school staff sent postcards to parents detailing how many days of school their child had missed alongside a handwritten note from their teacher summarizing the academic material covered during the absence. We randomized the intervention across schools and classrooms in two urban school districts. The analysis sample included 5,552 students in preschool through second grade. Overall, the treatment reduced absences by 8.3% (95% CI, 2.6% to 14.0%). The results provide evidence that a postcard intervention designed and implemented by schools and teachers rather than 3rd parties can be effective in reducing absences. The teachers' annotations with personalized academic and absence information are a unique feature of the intervention that suggest areas for further research.

Introduction

Decades of research link poor attendance with worse academic performance, higher retention rates, and increased likelihood of high school dropout (Ginsburg, Jordan, & Chang, 2014; Ehrlich, Gwynne, & Allensworth, 2018; Balfanz & Byrnes, 2006; Chang & Romero, 2008; Utah Education Policy Center, 2012; Mac Iver, 2010; Schoeneberger, 2012). Chronic absenteeism, typically defined as missing 10% of the school year or more, also predicts undesirable non-academic outcomes such as slower socioemotional development, greater feelings of disengagement, and poorer health (Gottfried, 2014; Henderson, Hill, & Norton, 2014; Ehrlich, Gwynne, & Allensworth, 2018). In light of the negative correlates of chronic absenteeism, thirty-six states and the District of Columbia recently included student absenteeism in their school accountability ratings under the Every Student Succeeds Act (ESSA) (Jordan & Miller, 2017).

Although much of the concern regarding absenteeism has been directed at high school, attendance issues often begin in early elementary grades (Romero & Lee, 2007; Schoeneberger, 2012). In fact, chronic absence rates in kindergarten can be nearly as high as those in high school (Balfanz & Byrnes, 2012; Utah Education Policy Center, 2012). National estimates indicate that 11% of kindergarten students and nine percent of first graders are chronically absent (Chang & Romero, 2008). Studies in several cities reveal that chronic absence rates in pre-kindergarten are even higher than in kindergarten (Ehrlich et al., 2014; Ehrlich, Gwynne, & Allensworth, 2018; Dubay & Holla, 2015). High absenteeism in the early grades should concern policymakers and practitioners not only because of the lost instructional time but also because absences in early grades strongly predict absences in subsequent grades and can have lasting negative impacts on children’s academic and social development (Ehrlich, Gwynne, & Allensworth, 2018; Connolly & Olson, 2012). Outcomes are particularly worrisome for young students who are chronically absent two years in a row. Evidence from

California districts shows that students chronically absent in both kindergarten and first grade are less likely to be reading at grade-level in third grade than peers with better attendance (Applied Survey Research, 2019). Furthermore, in Baltimore City Schools, more than 25% of students who were chronically absent in pre-kindergarten and kindergarten were retained before third grade (Connolly & Olson, 2012). Although non-attendance is problematic for all students, there is suggestive evidence that the academic consequences of missing school are particularly serious for low-income and Hispanic students (Romero and Lee, 2007; Chang and Romero, 2008; Ready, 2010).

What accounts for such high absence rates in kindergarten and pre-kindergarten? Some school staff believe that students in early grades are absent because parents underestimate the importance of the academic content covered in kindergarten and pre-kindergarten (Duardo, 2013; Robinson et al., 2018). In recent decades, kindergarten has certainly become more academically rigorous. Bassok, Latham, and Rorem (2016) show that compared to kindergarten teachers in 1998, kindergarten teachers in 2010 devoted more time to advanced literacy and math content. If parents are relying on their own experiences in early grades, they may be less aware of the opportunity cost of missing school. Interviews with pre-kindergarten parents confirm such impressions (Ehrlich et al., 2014; Katz, Adams, & Johnson, 2015). Teachers and administrators from the two school districts in our study echoed these sentiments.

State policies can reinforce parents' under-appreciation of the importance of early education. While schools must offer free education beginning at age 5 in all but two states, forty states do not legally require school attendance for 5-year-olds (National Center for Education Statistics, 2018, Table 5.1). The lack of compulsory schooling at this age may signal to parents that kindergarten is not important (National Center for Education Statistics, 2012, Table 136).

Beliefs about the rigor of kindergarten are not the only aspect of parental knowledge that may impact attendance. Survey results show that parents of high-absence students underestimate how many times their child has missed school, even in the early grades when parents are more directly involved in getting their child to school (Rogers & Feller, 2018). Several randomized trials have found that providing information to parents about their child's attendance through repeated mailings can partially correct this misconception and improve attendance (Robinson et al., 2018; Rogers et al., 2017; Rogers & Feller, 2018). Randomized trials in Philadelphia found that sending parents of K-12 students postcards with information about their child's attendance reduced absences by up to 10%, depending on the specific content of the mailer (Rogers et al., 2017; Rogers & Feller, 2018). Additionally, the mailings partially corrected parents' underestimation of their child's total absences (Rogers & Feller, 2018). Robinson et al. (2018) tested a similar intervention aimed specifically at the parents of high-absence elementary school students. Over the course of a school year, parents received up to six mailings providing their child's attendance record and describing a consequence of high absences for students in their child's grade. This simple intervention reduced absences by nearly eight percent.

In these interventions, parents received information about their child from a research team, not through the school or the student's teacher. However, recent research shows that increasing teacher communication to parents can improve student outcomes (Kraft & Rogers, 2015; Kraft & Dougherty, 2013; Shirvani, 2007). In three small-scale randomized experiments, students whose parents received consistent and structured communication directly from their teacher had improved academic outcomes, including a higher probability of earning summer-school credits as well as increased homework and test grades. The authors reported improved outcomes for all modes of communication tested: emails, text messages, phone calls, and sheets sent home with the student. Frequent communication to parents from the school can also improve student attendance. Bergman and Chan (2019) found that weekly text messages to parents of middle and high school students about missed classes reduced class absences by 12%.

In this paper, we present the results of the first randomized trial of a personalized postcard intervention that combines information for parents about a child's absences and the content missed. The study was designed to answer the question: Do students whose teachers send postcards to parents about their children's absences have better attendance than those whose teachers do not send postcards? Following each absence, parents received a postcard from their child's teacher indicating the number of days their child had missed that school year and describing the academic content covered during their recent absence. The intervention differed in several ways from those previously studied: (1) it involves handwritten communication from teachers to parents; (2) it includes students in pre-Kindergarten; (3) it informs parents of the specific academic content covered during their child's absence in addition to flagging recent absences; (4) it provides a benchmark for how concerned parents should be about their child's total number of absences; (5) the participating school districts designed and implemented the intervention themselves.[1]

Giving parents a postcard written by the teacher may signal to parents that the teacher notices their child's absence and wants to have their child in class. As in previous studies, including the student's number of absences aimed to correct parental underestimation of their child's total absences. Furthermore, the teacher's detailing of what the student missed in class was designed to reveal to parents that important academic content is taught every day, even in early grades. While we hypothesized that the personalized academic and absence information contained in these teacher-written postcards would be more effective than similar prior

[1] The research team participated in the design process in a facilitation role; this is described in detail in the Methods section.

interventions, ultimately the design of our study did not allow us to test the marginal impact of these differences. As such, this study strengthens the existing body of literature on the effectiveness of sending parents personalized information about their children's absences to improve student attendance while introducing novel features that merit further research.

Methods

Sample

Our sample consisted of 5,874 students in two urban school districts in the United States who were enrolled at the start of the intervention. In both districts, we measured absences for all students in pre-kindergarten, kindergarten, and first grade. In one district, second graders were also included. We tested the intervention in two rounds, with some district-grades participating in the fall and others in the spring of the 2018-19 school year (see Table S1). Our analysis sample consists of 5,552 students after excluding 316 students who were no longer enrolled at the end of the analysis period and six students who were missing key covariate information. In District A, 18 schools participated and in District B, 107 classes from 10 schools participated (see Figure 2). Students in District A are 22% white, 48% black, 14% Hispanic and 7% Asian. Eighteen percent of students were English language learners (ELL) and 20% were students with individualized education plans (IEPs). District B is 41% white, 36% black, and 7% Hispanic. Three percent of students were ELL and 13% of students had IEPs. In both districts more than 20% of children under 18 lived below the poverty line (U.S. Census Bureau, 2019).

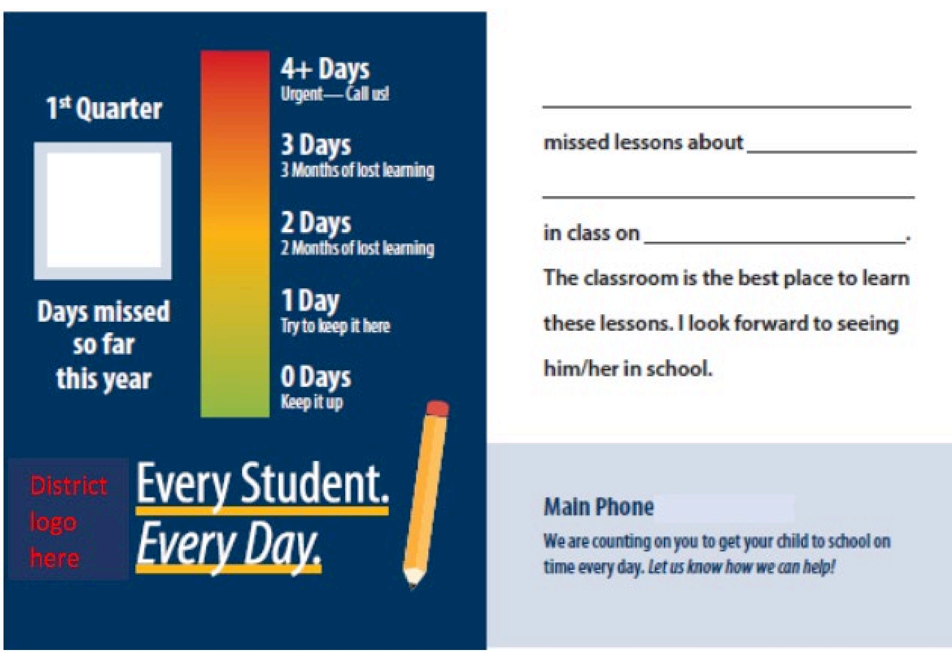
Intervention Design

Over the course of the spring and summer, teams of central office administrators and school-staff met to brainstorm plans to curb high absence rates among their kindergarten students. In the prior year, kindergarteners in each district missed an average of approximately 18 school days. Members of our research team facilitated a series of sessions to elicit hypotheses regarding the root causes of such high absence rates and to record district staff's ideas for possible interventions. Teams from both districts independently expressed a belief that absences were high because parents underestimated the amount of important academic content taught in kindergarten. In subsequent sessions, the district teams assessed the feasibility and potential impact of proposed interventions and selected one that they believed could do the most to reduce absence rates with relatively low costs and effort. We led sessions to help the teams design the selected interventions. Throughout development, the district teams agreed to share their ideas, ultimately arriving at a similar plan in both sites. The teams at both sites also consulted parents and teachers before finalizing their designs.

The two districts agreed to send parents a postcard following a student's absence. The postcards would feature a count of how many days the child had missed throughout the school year and a handwritten note from their teacher summarizing the academic content missed during the student's recent absence(s). School office staff would write student names and absence counts on the postcards so that teachers need only add a note regarding missed academic content. The child's cumulative count of absences would appear on a color-coded meter indicating how concerned the parent should be about their child's attendance, alongside a reference to the connection between attendance and lost learning. Both versions of the postcard included school contact information and encouraged families to reach out for assistance. Although the teams developed the postcards with kindergarteners in mind, they decided to expand the study to include additional early grades that might benefit from the intervention. All postcards were printed in English. Under the study design, one-half of schools (District A) or classrooms (District B) tested the postcards. In the control schools and classrooms no changes were made to how teachers addressed absences.

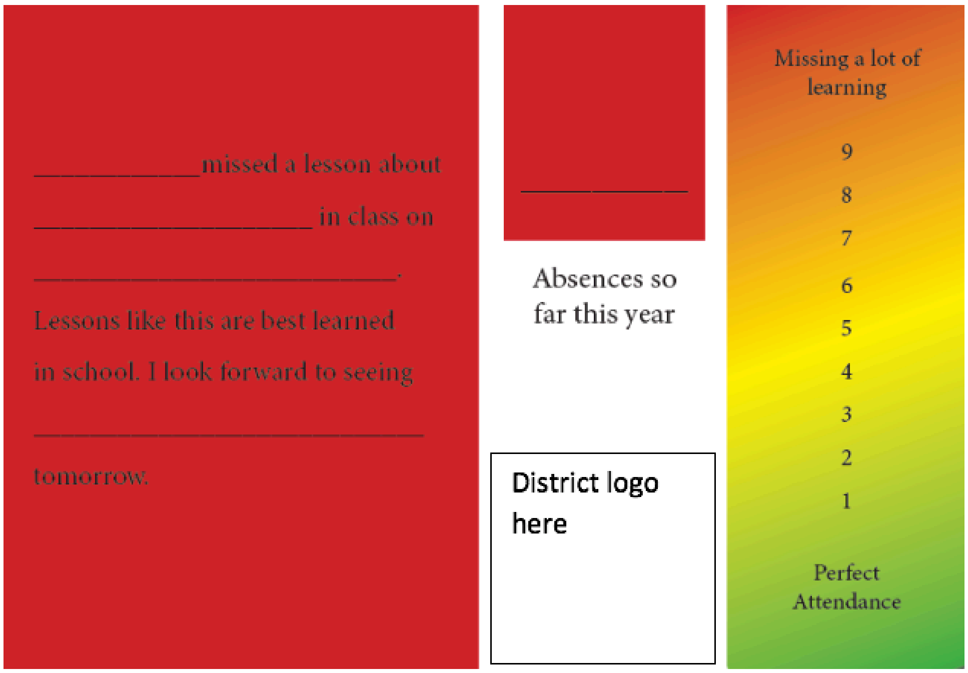
Though broadly similar, the intervention implemented in the two districts differed in delivery method and some of the content and visual design. District B chose to display students' cumulative absence counts on a scale from one to nine, with a note that the student is "missing a lot of learning" at the peak of the scale. District A's scale for its fall postcard ends at "4+" absences with a note reading, "Urgent--- Call us!"; the upper limit of the scale increased by four absences each quarter. Teachers in District B provided students with the postcard on their first day back following an absence, with instructions to bring it home to their parent or guardian. In District A, central office workers mailed the postcards on Fridays for all absences that occurred during the school week. Each team designed the postcards to feature district colors and logos. Figure 1 shows the layout of the postcard for each district.

Figure 1. Sample Postcards, District A



District A. Front of the postcard. The back of the postcard only contained address information.

Figure 1. Sample Postcards, District B



District B. Front of the postcard. The back of the postcard had a note encouraging parents to call someone at the school for support.

Randomization and Implementation

In the fall round of the intervention, District A launched the postcard intervention with kindergarten and first grade students. All schools in the district were eligible (N=18) except for one school with very low absence rates and a markedly different demographic makeup. We randomized nine schools into treatment and nine into control. Every Friday, staff in treatment schools mailed postcards to parents of students who missed at least one day that week. Families of students in control schools were not assigned to receive any additional information. In early March, District A launched the intervention in pre-kindergarten. The same school-level treatment assignments from the fall round of the study were used.

District B launched the intervention for kindergarten students in the fall semester. All kindergarten classrooms were eligible (N=31), and we randomly assigned 16 to treatment and 15 to control. Students in the treatment classrooms received postcards from their teachers on the day they returned from an absence. Teachers instructed students to deliver the postcards to their parents or guardians. Students in control classrooms received no postcards. In the spring, District B launched the intervention in pre-kindergarten, first, and second grade classrooms (N=76). Half the classrooms in these grades were randomized to the treatment condition.

Our analysis sample includes data from the 13 weeks following the launch of the treatment in each district-grade combination. We chose this time period because it was the longest we could follow for both districts' fall and spring launches.

We lack granular implementation data, but the districts reported that teachers completed the postcards. Assuming teachers completed and distributed postcards according to plan, we present the distribution of postcards sent per student in the Supplemental Materials (Figure S1).

Randomization Procedure

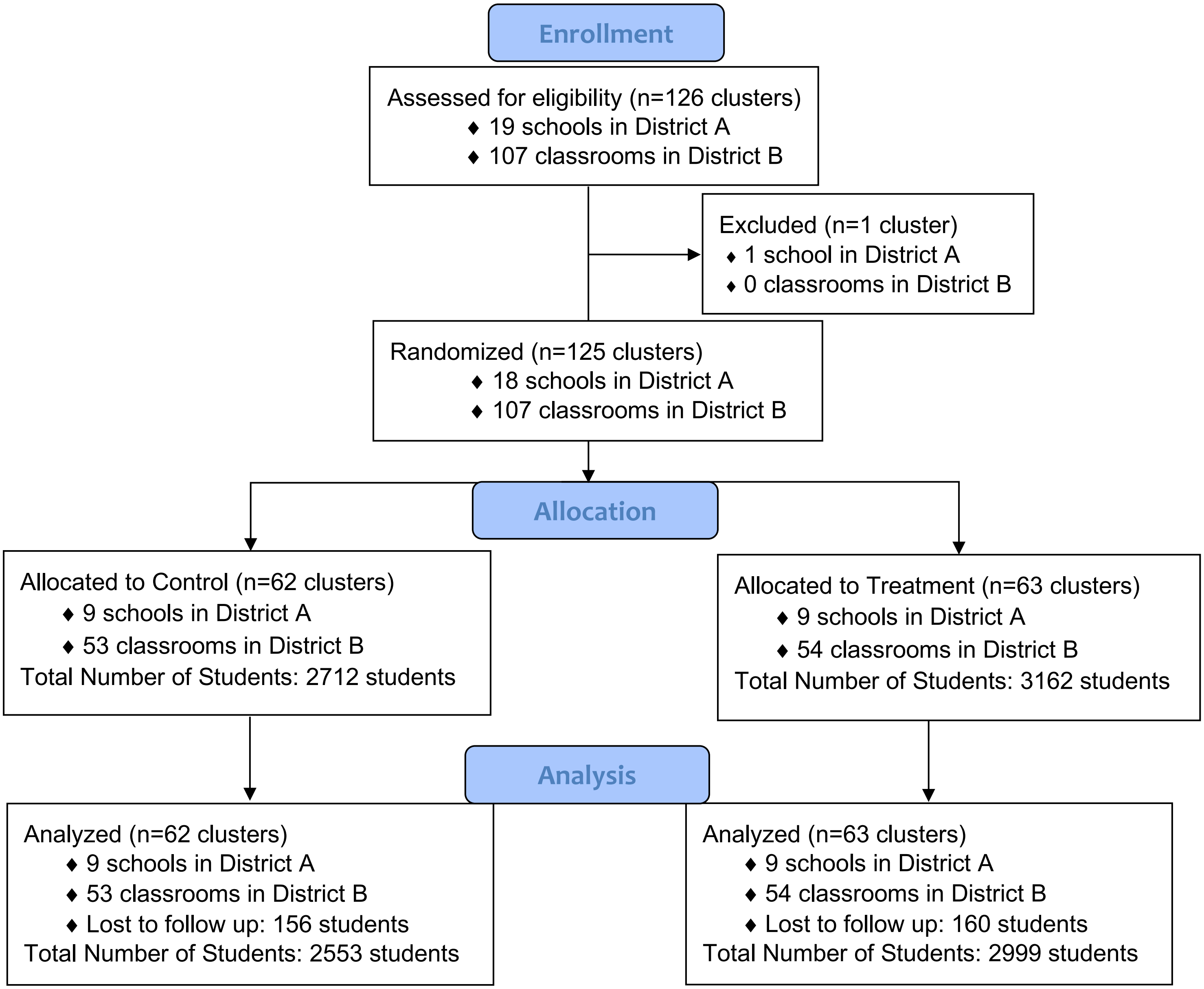
In District A, we randomized schools, blocking on levels of the schools' average absence rates in the prior year. We constructed the blocks by evenly dividing schools into three groups based on their average absence rate and then randomly assigned half the schools in each block to treatment and the other half to control. School treatment assignments applied to both the fall and spring rounds of piloting. In District B, we randomized kindergarten classrooms, blocking on school. For the spring round of the intervention in District B, we blocked on special-education classroom status, school, and grade. The special education classrooms enrolled a smaller number of students, the majority of whom had IEPs. See Supplemental materials for additional details.

Among students in the final sample, we found no significant differences in pre-treatment characteristics between the treatment and control groups. We checked for balance on student-level characteristics jointly across treatment assignments using logistic regression. In the balance-checking model, we specified treatment assignment as a binary outcome with pre-randomization student characteristics as independent variables, block fixed effects, and clustering at the unit of randomization. The covariates were not jointly predictive of treatment assignment (p = .68; Table 1)

Table 1. Baseline Characteristics by Treatment Status

Baseline Characteristics	Control Mean	Treatment Mean	P-value for Difference
Male	0.52	0.53	0.80
English Language Learner	0.09	0.15	0.09
Special Education	0.16	0.16	0.98
Free/Reduced Price Lunch	0.64	0.66	0.73
School-Grade Prior Avg. Days Absent	17.70 (3.61)	17.00 (3.66)	0.49
School Prior Avg. Days Absent	16.80 (3.05)	16.15 (3.51)	0.52
Pre-treatment Days Absent	3.85 (3.92)	4.01 (4.41)	0.65
Prior Days Absent	19.14 (15.49)	18.04 (14.47)	0.33
Black or African American	0.45	0.40	0.41
Hispanic	0.11	0.12	0.86
White	0.27	0.27	0.95
% Missing School-Grade Prior Avg. Days Absent	0.22	0.25	0.55
% Missing School Prior Avg. Days Absent	0.03	0.02	0.85
% Missing Pre-treatment Days Absent	0.62	0.66	0.76
% Missing Prior Days Absent	0.5	0.54	0.33
N	2553	2999	
Joint test of orthogonality: $\chi^2(45) = 40.14$; p-value = .68			

Figure 2. Consort Diagram



We also checked for differential student attrition between treatment and control assignment. To investigate this, we specified a logistic regression with attrition from the analytic sample as the binary outcome and treatment assignment as the independent variable. Attrition was defined as not being enrolled in the school district at the end of the study period. We found no evidence of differential attrition by treatment assignment (p = .241).

Analytic Plan

Our primary outcome was the total number of absences (including excused and unexcused) recorded for a student during the 13 school-week analysis period. We used a linear regression model to estimate the average treatment effect of assignment to the postcard intervention. Our model included fixed-effects for the randomization blocks and adjusted for student-level demographics (race, gender, ELL status, IEP status, and for District B's free or reduced-price lunch eligibility), prior student absences, and prior school and school-grade average absences. We chose to include these demographic variables because they are well known predictors of educational outcomes, including attendance (U.S. Department of Education, 2019; Romero & Lee, 2007; Utah Education Policy Center, 2012). Furthermore, for students in all grades prior year absences are a very strong predictor of current year absences (Romero & Lee, 2007; Utah Education Policy Center, 2012; Ehrlich, Gwynne, & Allensworth, 2018). School and grade level averages were included because attendance can vary widely by grade and school, even within the same district (Belfanz & Byrnes, 2012; Chang & Romero, 2008). Many pre-kindergarten and kindergarten students did not have prior attendance data, as it was their first year in the school system. We include an indicator in the model for students who are missing prior absence information. In our primary regression model, we included fixed effects for the randomization blocks and clustered standard errors at the level of randomization, as suggested by Abadie et al. (2017).

To test whether the treatment significantly affected student attendance, we calculated exact p-values using a randomization inference procedure (Athey & Imbens, 2017). In Table 2, we report the results of these fixed-effects models both without and with the additional covariates above (models 2 and 3 in Table 2). We checked the sensitivity of our results to the model specification by estimating average treatment effects using a Poisson model (reported in table S2 in the supplemental materials). We also ran a model with no covariates (model 1 in Table 2), which was expected to yield similar results due to the randomization of the treatment assignment. Since schools and classrooms were randomized rather than students, some siblings ended up in different treatment conditions. To determine whether this impacted the results we also ran models where students from the same household were removed. The statistical and practical significance of the estimate is consistent across all these specifications.

We explored whether the treatment effect differed significantly by grade-level, district, or season (fall vs. spring). In testing these differences, we fit linear regression models as described above with the addition of interaction terms between treatment assignment and sub-group membership indicators. We also report the results of models including interactions between treatment status and student-level demographics. Our analyses of the heterogeneity of treatment effects should be considered exploratory as we lacked a priori hypotheses regarding these subgroups.

In the design process, the district-based teams sought specifically to reduce absences in Kindergarten. Though they later decided to pilot the intervention in other early grades, the two teams hypothesized that the issue of parents underestimating academic content especially drove Kindergarten absences. For this reason, we consider differences between the intervention's effects on Kindergarten relative to other grades to be of particular interest. We emphasize that, lacking a pre-registered analysis plan, the observed differences should be considered exploratory, but we call attention to them as a potential focus of future work given the views expressed by district teams in the intervention design process.

Results

During the 13 weeks we analyzed, students in the control group missed an average of 5.42 days of school, with an average absence rate of 8.45%, while the treatment group missed an average of 4.98 days, with an absence rate of 7.75%. Average absences for control students were highest in pre-kindergarten (6.99 days), decreasing monotonically to 3.91 days in second grade. Control group students averaged 6.20 days absent in District A and averaged 4.16 days absent in District B. Average absences in the control group were similar in the fall and spring rounds of the intervention (5.44 and 5.39 days, respectively).

Table 2 presents estimates of the average treatment effect from linear models. Our preferred model (3) adjusted for covariates (see the analytic plan above) and included fixed-effects for the randomization blocks. We estimated that students in classrooms or schools assigned to the treatment condition missed 0.45 fewer days of school, on average, than students in the control group (SE = 0.16, exact p-value = .01). This corresponds to an 8.30% relative reduction in absences, with students in treatment groups missing an average of 4.98 days compared to 5.42 days for control group students. This model produced an estimate slightly larger than the raw difference, a reduction of 0.44 days (see model 1), while model (2) with only block fixed effects estimated a 0.42 day reduction. In models with fixed effects for the randomization blocks, we present standard errors clustered at the unit of randomization following Abadie et al. (2017).

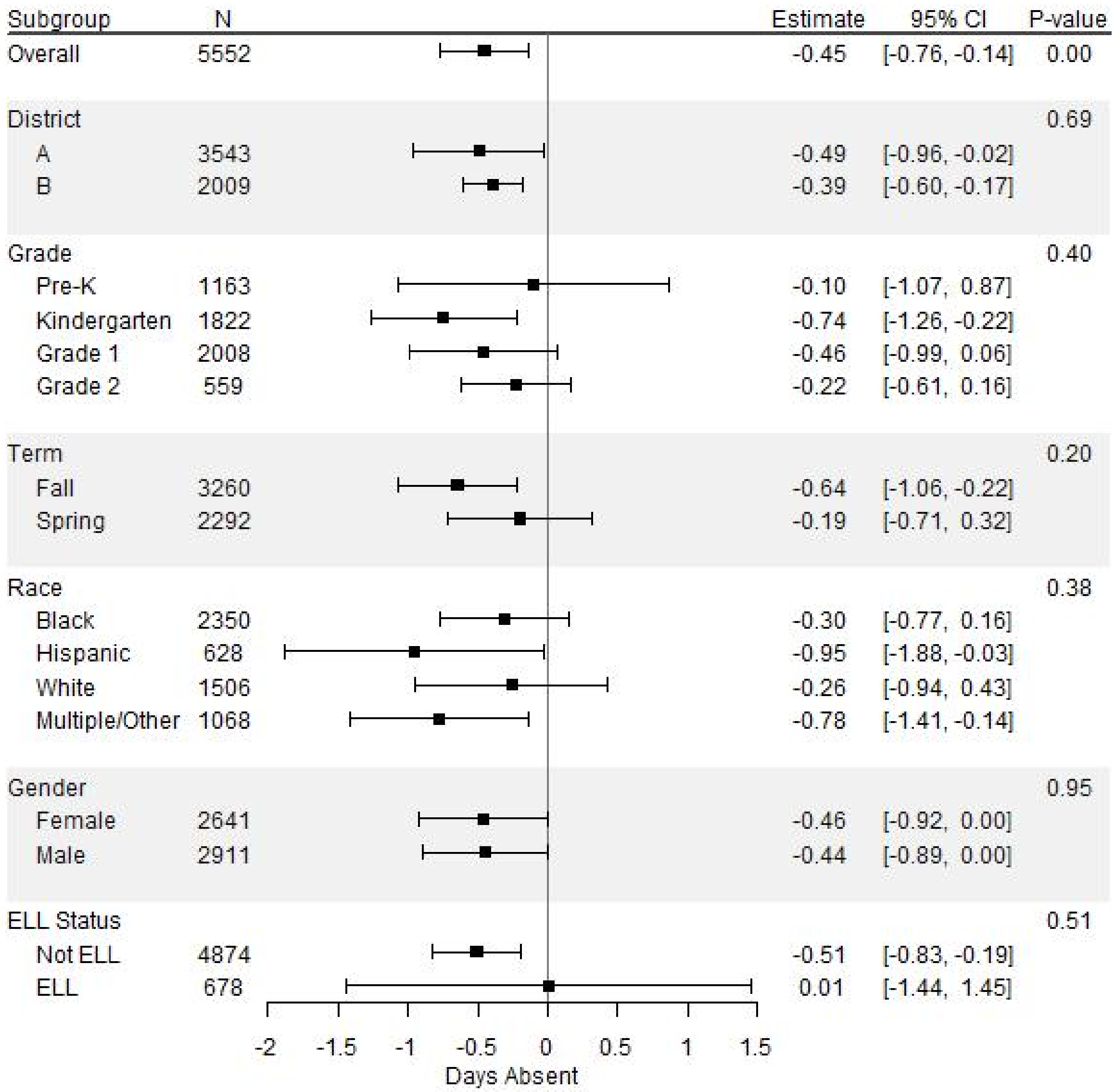
Table 2. Estimated Impact of the Intervention on Days Absent

	Model 1	Model 2	Model 3
Estimated Treatment Effect (control mean = 5.42 absences)	-0.44	-0.42	-0.45
95% CI	(-0.64, -0.23)	(-0.77, -0.08)	(-0.76, -0.14)
Model p-value	<.001	0.02	<.001
Exact p-value	0.063	0.038	0.01
Clustering	X	X	X
Block Fixed Effects		X	X
Full Covariates			X
N	5552	5552	5552

The Poisson models we include as a robustness check find reductions in absences between .4 and .45 days. The treatment effect is statistically significant in all specifications. Similarly, our preferred model with cross-treatment households removed yielded a statistically significant treatment effect estimate of .42 fewer absences.

Figure 3 presents results from exploratory analyses of differential treatment effects (with 95% confidence intervals) by district, grade-level, season, and student demographic subgroups. We observe a larger treatment-vs-control difference for kindergarteners than for students in other grades, with treatment group kindergarteners recording approximately one fewer absence on average. We saw no evidence of differences in the treatment's effect by district, race, gender, season, or English-language-learner status.

Figure 3. Estimated Impact of the Intervention on Days Absent by Subgroup



In some cases, the pilot program continued for additional weeks, but we chose to truncate those data to match the length of the shortest pilot period. Estimates of the treatment's effect on absence rates in the truncated district-grades are similar when including the additional weeks. However, since the outcome in our main model is days absent, analyzing the same length of time for all district-grades was desirable. As we only observed the students over a 13 week period, we do not have an annual measure of chronic absenteeism, however, we can look at students who were absent at least 10% of days during the study period as a proxy for being “on track” to be chronically absent. These results are presented in the Supplemental Materials, Table S3.

Discussion and Conclusion

In this study we evaluated an intervention designed to reduce student absences in pre-kindergarten through 2nd grade. School staff sent postcards to parents following a student’s absence detailing how many days of school the child had missed alongside a handwritten note from the teacher summarizing the academic content covered. We found that the intervention reduced absences by 0.45 days (95% CI, 0.14 to 0.76) relative to a control mean of 5.42 days absent. The point estimate corresponds to an 8.3% reduction in absences.

Our findings add to a growing literature suggesting that messaging parents can help reduce student absenteeism (Robinson et al., 2018; Rogers et al., 2017; Rogers & Feller, 2018). We extend the previous research by evaluating an intervention that (1) addresses a wider set of parental attitudinal and informational issues; and (2) is designed and implemented by the school districts themselves, rather than by an external group of researchers or a third-party vendor. In addition to including information on total absences to correct parental underestimation of a child’s absences, the postcards also provided parents with information to make them more aware that important academic content is taught every day, even in early grades. The teacher’s handwritten and personalized portion of the postcard may have further signaled to parents that the teacher notices their child’s absences and wants their child in class. As our present experimental design does not allow us to estimate the impact of these distinguishing features, we identify them as avenues for future research.

The estimated impacts are similar to those of prior studies. Robinson et al. (2018) and Rogers & Feller (2018) found the messaging interventions reduced absences relative to controls by about 7.7% and 3.5% to 6.5% (depending on treatment arm), respectively. These studies exclude students with low prior-year absence rates. Our inclusion of these students may lead to understating the postcards' effects relative to the impact reported for these similar messaging interventions. Costs per day gained were approximately $6 and $11 for these mailing interventions that did not require teacher time. In contrast, we estimated higher costs per day gained. The school districts provided estimates of staff time (including training), printing, and any mailing costs associated with the interventions. In all, District A spent about $2.09 and District B about $5.37 per student per week on the intervention. (See Supplemental Materials for more details on these cost estimates.) This translates to about $56 in District A and $179 in District B per student-day of attendance gained due to the intervention.[2] A large share of these costs could be recouped through increased state funding in districts that are funded based on their average daily attendance.

We see five primary limitations to our study. The first is our focus on absences over a period of a few months. We do not assess the impacts over the course of a year or whether attendance in subsequent years is affected by the intervention. It is also possible that parents and students become habituated to the treatments overtime, leading extrapolations of the estimated effects over time to be upwardly biased. And, although absences were the primary target of the

[2] The large difference in costs is almost entirely attributable to the difference in required teacher time reported by each district. District A instructed teachers to fill out all postcards for the week on Fridays, while District B asked teachers to take time each school day to fill out the cards.

intervention, many districts ultimately hope that increased attendance will improve academic outcomes; however, we do not have the data or sample size to measure those outcomes. We also cannot assess other types of benefits or disadvantages that might be associated with the investment of teacher time in the personalization of the postcards. Does this personalization increase parent engagement or make teachers more aware of the academic content missed by individual students? Or does this task take them away from other important work? The second limitation is that we do not have detailed information on the implementation itself. Based on conversations with district administrators and school leaders we are reasonably confident that teachers completed and sent the postcards, but we do not know whether they were filled out appropriately or as frequently as required. The third limitation is the fact that the postcards were only printed in English. One of the partner districts enrolls a substantial number of ELL students (the other has very few). The districts made the decision to provide only English postcards. Given the sizeable ELL population in one of the districts, it is possible that the intervention would have been more effective if the cards were available in multiple languages, particularly Spanish. This limitation has particular relevance in considerations of generalizing the intervention to contexts with substantial linguistic diversity. Fourth, our design did not allow us to determine whether impacts vary by grade, time of year, or other characteristics. A final but important limitation of this study is that it was not pre-registered. Pre-registration is considered best practice in randomized trials and is required by many journals for publication. The rapid-cycle nature of the design and implementation of this trial, along with this being one of the first set of randomized trials being done at scale within a larger project, made it difficult to pre-register as the analytic machinery that produces rapid results were being developed in parallel. Fortunately, as the project has matured, the analytic models and processes have been standardized and all ongoing trials are now being preregistered on a regular basis. Among other things, this intervention is distinguished by the fact that the school districts themselves designed and implemented the intervention. Though the per-student costs appear higher than similar programs implemented by third parties, it may be the case that this model of research-practice partnership generates effective intervention ideas that would otherwise go untested and/or that district-led design and implementation of interventions can build district capacity for enacting future reforms. We believe this is an interesting area of future research.

Throughout this paper, we have repeated the hypothesis that rates of absenteeism may be particularly high in kindergarten and pre-kindergarten due to parents' beliefs about the amount of important academic content taught in those grades. We presented this hypothesis because of its role in the districts' design of their interventions; other work in the literature on absenteeism has also explored this mechanism, as we discussed in the introductory section of this paper. However, we point out here that students miss school for a range of reasons, including illness, transportation challenges, and family-related issues (Ehrlich et al., 2014). Furthermore, some obstacles to regular attendance may disproportionately affect students from particular demographic groups. Addressing absenteeism likely requires interventions that address these challenges, with a focus on the potential heterogeneity of obstacles faced by students (and their families) of varying backgrounds. This marks an important opportunity for future research.

The new school accountability rules put pressure on districts and school administrators to find ways to lower student absenteeism. Our results suggest that simply providing parents with a personalized note from their child's teacher tallying a student's total absences and describing the missed academic content lowered absence rates in early elementary grades by 8.30 percent. This would translate to an additional 1.24 days of attendance over the course of a 180-day school year. In states that base district funding on average daily attendance, districts might cover a substantial share of the intervention cost through increases in state funding. For instance, California funds districts in part based on average daily attendance (ADA). In the 2018-19 school year, the state of California provided $8,235 per ADA. Assuming a 180-day school year, the postcard intervention would increase ADA by about 0.007 for each student, yielding an additional $58 in per student funding for the district, which may cover the intervention's per student cost. Schools could reduce time demands by automating the process further—such as embedding some functionality within enrollment management systems. Given the high opportunity cost of teachers' time, any such reduction would meaningfully improve cost effectiveness.

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Supplemental Materials for:

Your child missed learning the alphabet today: A randomized trial of sending teacher-written postcards home to reduce absences

This supplemental appendix provides additional details on the randomization, analysis models, and cost estimates.

Details on Randomization Procedure

In District A, we randomized half of schools to treatment, blocking on levels of the schools' average absence rates in the prior year and used this assignment for both the fall and spring rounds. In District B for the fall round, we randomized half of kindergarten classrooms to treatment, blocking on school. In the spring round of the intervention in District B, we randomized half of pre-kindergarten, first, and second grade classrooms, blocking on grade, school, and special-education classroom status.

In District B in the fall, when there were an odd number of classrooms within a grade, one classroom was randomly placed in a block with the other "remainder" classrooms. In the spring, one classroom from each block with an odd number of classrooms was randomly placed in a block with other "remainder" classrooms from the same school with the same special education status. For any of these remainder blocks with an odd number of classrooms, one classroom was randomly placed in a block with other remainder classrooms with the same special education status. Then the "remainder" classrooms were randomized. This procedure ensured the best possible balance on school when perfect balance at the school-grade level was not possible.

Table S1. Grade-Levels Included in Study by Term

		Grade			
		Pre-K	Kindergarten	1	2
District A	Fall		X	X	
	Spring	X			
District B	Fall		X		
	Spring	X		X	X

Details of main analysis and sensitivity checks

Covariates included in the main analysis and sensitivity check include indicators for race; gender; special education status; English Language Learner (ELL); free and reduced-price lunch (FRPL) eligibility, and grade-level; and school and school-grade average prior absences; student's prior absences; and missingness indicators (when necessary). With the exception of gender, these student-level variables have been shown to be related to absenteeism. National attendance data for students in elementary school shows that black, American Indian, and Hispanic students have higher rates of chronic absenteeism than their white and Asian peers, students with IEPs have higher chronic absence rates than non-IEP students, and ELL students have lower chronic absence rates than non-ELL students (U.S. Department of Education, 2019).

Prior school and school-grade average absences were calculated using absence data for all students across those schools and grades who were enrolled for at least 20 days in the preceding school year. Similarly, student-level priors were only included if the student was enrolled for 20 or more days. Students enrolled fewer than 20 days in the prior school year were treated as if they had missing prior information.

In addition to the main OLS models presented in the paper, we also fit a Poisson model. The results are robust to different model specifications.

Table S2. Comparison of Alternate Specification to Main Model: Estimated Impact of the Intervention on Days Absent

Model	OLS	Poisson
(1) No controls	-0.44	-0.45
	(.10)	(.10)
(2) Block fixed effects	-0.42	-0.41
	(.18)	(.17)
(3) Full covariates	-0.45	-0.40
	(.16)	(.14)

Details on Cost Estimate

In District B teachers in the treatment group attended one 45-minute training session and spent 1.25 hours per week filling out postcards, a total of 17 hours in the 13-week pilot period. The average salary (plus benefits) for full-time teachers in the participating districts - who work about 184 days per year, 7 hours per day - was $84,033, yielding a cost of $1109 per teacher for the pilot period. Each teacher serves approximately 20 students, a cost of $55.46 per child. Teaching support staff (e.g. teacher's assistants or paraeducators) invested the same amount of time and earn an average of $12.68 per hour, leading to an additional $10.78 per student. Each school assigned a clerical worker to one hour of training and one hour per-week preparing postcards for teachers, for a total of 14 hours during the pilot period. They earn $18.46 per hour and serve 122 treatment students on average, yielding a cost of $2.12 per student. Central office staff reported working 3 hours per person on designing the postcards and training staff and they earn about $71 per hour (this includes the cost of benefits). Assuming a four-person team from the central office, this adds a cost of $0.82 per student. Treatment students missed an average of 3.91 schooldays in the pilot period, requiring that number of postcards to be printed at a cost of $0.16 each, totaling $0.63 per student in the pilot period. Combining these estimated costs yields a total of $69.80 per student, approximately $179 per day of attendance gained. For these calculations, we used employee-level salary data for the district that is publicly available on the state treasurer's website and estimates of employee time-investment provided by the district teams.

We can construct an estimate for district A similarly (salaries reported are adjusted to 2018 constant dollars to match those reported for district B): teachers in the treatment group attended one 30 minute training session and spent 20 minutes per week filling out postcards, a total of 4 hours and 50 minutes in the 13 week pilot period. The average salary (plus benefits) for full-time teachers in the district - who work about 187 days per year, 6.5 hours per day - was $87,158, yielding a cost of $347 per teacher for the pilot period. Each teacher serves approximately 20 students, yielding a cost of $17.33 per child. Teaching support staff (e.g. teacher's assistants or paraeducators) invested the same amount of time and earn an average of $29,353 annually, an additional $5.84 per student. Volunteers helped to prepare the postcards for teachers; we estimate no cost for this, though a small amount of staff time must have been necessary to train them. Central office staff reported spending a total of 15 hours on designing the postcards and training staff and we estimate the staff salary (plus benefits) at $203,804 per year. This adds a cost of $1.28 per student. Treatment students missed an average of 5.55 schooldays in the pilot period, requiring the postcards to be printed and mailed at a cost of $0.50 each, totaling $2.78 per student in the pilot period. Combining these estimated costs yields a total of $27.22 per student, approximately $56 per day of attendance gained. For these calculations, we used average salaries reported by the district (from Bureau of Labor Statistics data), the national average of benefit-to-salary expenditures from data published by the National Center for Education Statistics (2019), and estimates of employee time-investment provided by the district teams. Note that some of these costs would not increase over time or when scaling the intervention to serve additional students, so at scale, the per student cost would likely be lower.

Impact on Being On-track for Chronic Absenteeism

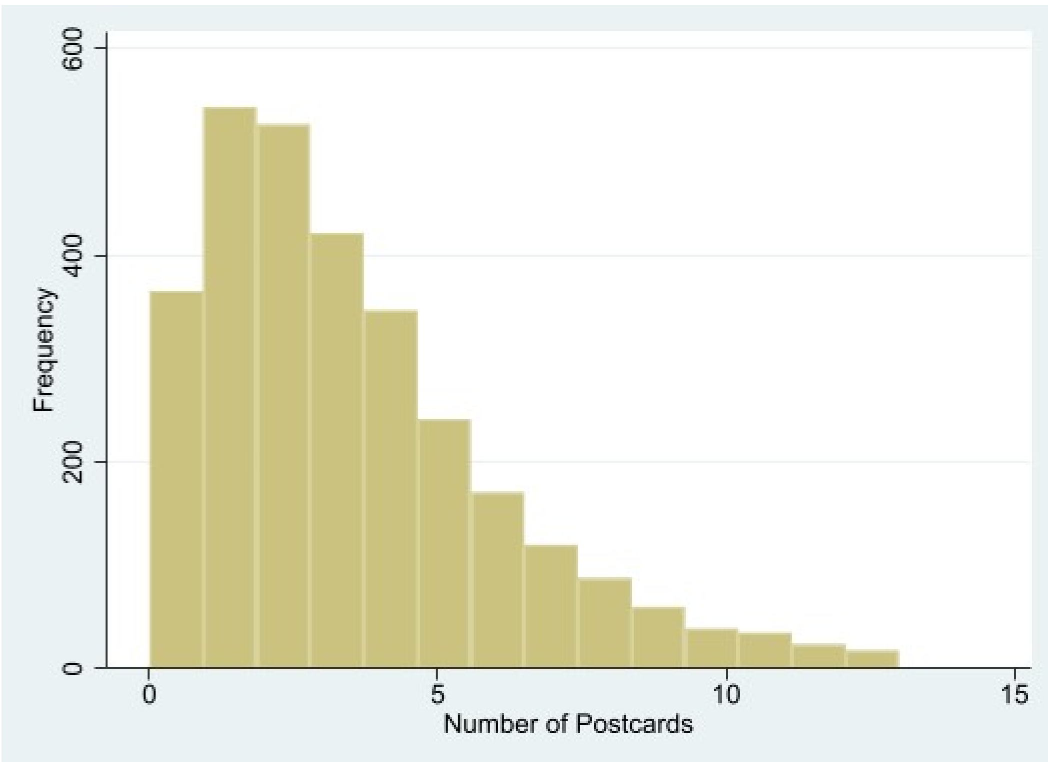
As our follow up period is only 13 weeks, we cannot directly assess impacts on an annual measure of chronic absenteeism. Instead, we can look at students who were absent at least 10% of the school days during the follow up period as a measure of being "on-track" for chronic absenteeism. Our logistic regression models suggest that the treatment lowers the odds of being on-track for chronic absenteeism by 17% to 20%, depending on the specification.

Table S3. Estimated Impact of the Intervention on Chronic Absent

	Model 1	Model 2	Model 3
Estimated Treatment Effect (control = 29.89% chronic absent)	-0.19	-0.20	-0.22
95% CI	(-0.25, -0.13)	(-0.35, -0.05)	(-0.36, -0.08)
Model p-value	<.001	0.01	<.001
Adjusted R-Squared	0.0017	0.0391	0.1706
Clustering	X	X	X
Block Fixed Effects		X	X
Full Covariates			X
Number of Clusters	125	125	125
N	5552	5552	5552

Distribution of Postcards

Though we lack granular implementation data, our partners reported that teachers did complete and send the postcards regularly. Figure S1 below shows the distribution of postcards sent per student, assuming that teachers sent postcards according to each districts' intervention plan.



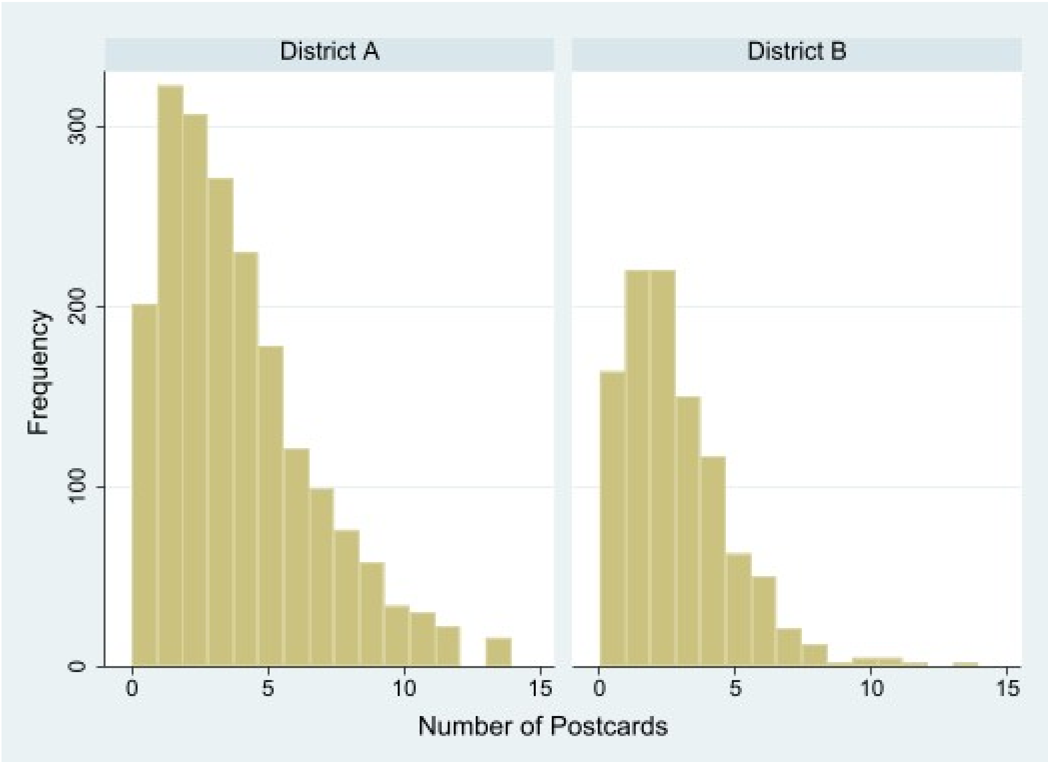


Figure 4.

Distribution of number of postcards sent home during the 13 weeks of study period.

Reference

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