Does outreach encouraging families to engage with community-based organizations increase engagement and school attendance?

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Abstract

Students can face many barriers to attending school, such as lack of affordable transportation, childcare for siblings, or negative experience at school. The Show Up, Stand Out (SUSO) program is designed to help students and families overcome those barriers, but many families who are referred to SUSO do not accept voluntary support services. We tested whether a timely letter informing families about SUSO's services would increase engagement with the community-based organizations providing services through SUSO. We found that the letter did not increase engagement and may have reduced the number of families accepting services. We also tested whether the letters improved attendance. We found that the letters led to a decrease in unexcused absences in the short term, with some effects on year-end truancy, but had no effect on (1) total absences in the short term and (2) year-end chronic absenteeism.

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1 Introduction

Students in DC schools struggle with high rates of absenteeism that can impede their academic success. In the 2012-2013 school year, DC established the Show Up Stand Out (SUSO) program to help reduce unexcused absences and truancy. Through SUSO, the Office of Victim Services and Justice Grants (OVSJG) provides grant funding to seven community-based organizations (CBOs) to provide free support services to families of children who have reached five to nine unexcused absences during the academic school year.[1] Each school participating in the SUSO program is mapped to one CBO who serves all students who become eligible for the program. The goal of the CBO services is to help students who are experiencing high rates of unexcused absences, before the law requires more punitive interventions like referrals of families to DC's Child and Family Services Agency (CFSA) or of teenagers to the juvenile justice system.

Figure 1 shows the need for supportive attendance interventions. Each dot represents a school that participates in SUSO, and the colors are shaded by that school's percentage of students who missed more than 10 school days without an excuse.[2] The map shows these percentages in the 2016-2017 school year, the year before we partnered with SUSO to test ways they could improve their engagement with families. The schools in red are those where over 40% of students are truant. The prevalence of schools with high concentrations of truancy, many of which are located in Census tracts where families face poverty (light blue) and other stressors,[3] illustrates the importance of families receiving services to help with their child's attendance.

Despite providing services free of charge, the CBOs that SUSO partners with struggle to engage families. In 2016, CBOs, on average, only engaged about 7.4% of families they reached out to with offers of help. SUSO CBOs do report improved attendance among families who accept their offers of help. However, the low uptake rate among eligible families—close to 7%—means that families who do engage may be more likely to improve their child's attendance regardless of the help they receive.[4] OVSJG would like to increase the rate at which CBOs successfully engage families of students who have reached five to nine unexcused absences during the academic school year in the SUSO program.[5] Ultimately, OVSJG hopes to improve attendance among families who qualify for SUSO, which would also support Mayor Muriel Bowser's *Every Day Counts!* campaign.[6]

Interviews with CBO staff indicated two major barriers to family engagement: (1) distrust of an unknown person and organization that had access to information about their child; and (2) a mistaken belief that the SUSO program is administered by or refers children to the Child and Family Services Agency (CFSA), and that engagement thus increases the likelihood of losing custody or benefits due to some violation of a rule or regulation. Staff also reported that families seemed surprised by their child's absence rates, and that the outreach prompted them to follow up

[1] Show Up, Stand Out is a free program administered by the Office of Victim Services and Justice Grants. To learn more visit: http://www.showupstandout.org/who-we-are/.

[2] In DC's official attendance definitions, they define chronically absent as missing 10 or more school days and define truant as "Having accrued at least 10 unexcused absences during the school year." We follow this terminology and use truant to refer to 10 or more unexcused absences and chronically absent/chronic absenteeism as 10 or more excused or unexcused absences. Report: OSSE attendance report.

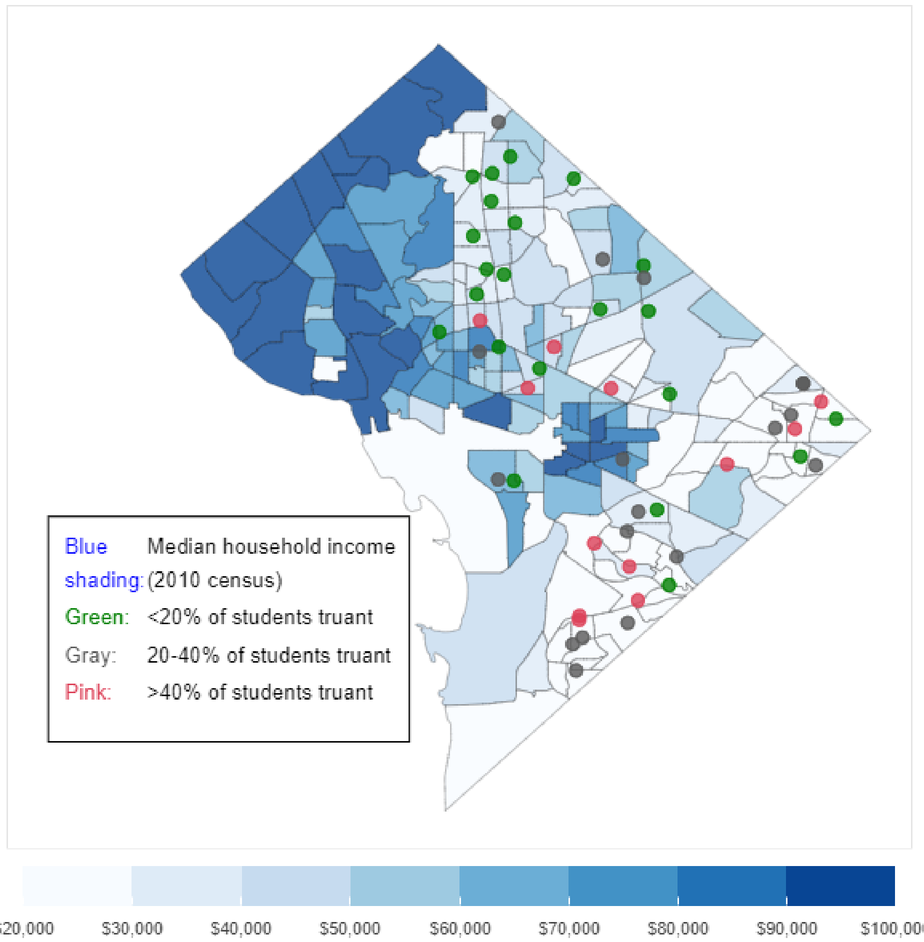
[3] Since DC has a robust school choice system, where parents have a right to send their child to an in-boundary school that might be within the same Census tract, but where they can also opt into a lottery to send the child to a school that might be outside their neighborhood, the poverty rates of the Census tract in which a school is located are only a rough approximation for the poverty faced by students attending the school. When we review demographic characteristics of students in the sample, we highlight these high poverty rates.

[4] To avoid selection bias, the present analysis focuses on all families eligible for CBO-based support, regardless of whether the families engage with the CBOs.

[5] Engagement is completed when a guardian in a family provides written consent to CBO assistance.

[6] https://attendance.dc.gov/

Figure 1: **Schools enrolled in SUSO program with dots shaded by percent of the school's students with more than 10 unexcused absences in SY 2016-2017**. The green dot shows schools with low concentrations of truant students, the gray with medium concentrations, and the red with high concentrations. The map is restricted to the schools enrolled in the SUSO program, which are a subset of all DC Public and Public Charter schools.



with the school to correct mistaken absence information.

We designed an outreach letter with OVSJG to address some of these perceived barriers by introducing the family to the CBO and emphasizing the supportive nature of the CBO's services (Appendix Section 9.2). This study had two objectives: (1) determine what effect, if any, the letter had on CBO-family engagement; (2) determine what effect, if any, the letter had on student attendance.

2 Intervention

In DC Public Schools and many Public Charter Schools, parents are supposed to receive a letter from their schools after their child has had three unexcused absences. Families become eligible for SUSO when their child has reached five unexcused absences. Therefore, this study examines the effect of outreach on parents who have likely already been informed about their child's unexcused absences, but whose children have continued to miss school.

We hypothesized that a friendly introductory letter from a caseworker at a CBO would reduce family distrust and thus increase engagement with SUSO programs. We also hypothesized that the letter would reduce absenteeism, either through the family engaging with the CBO or through making absences more salient to the family regardless of whether they engage.[7] The letter emphasized (1) the partnership between the school and the CBO (rather than the SUSO program), stating that the CBO and school are "joining forces," and (2) the services that a specific case worker could help a family obtain, listing concrete resources like bus and metro passes, food, and housing. In addition to the resources, the letter told families: "I know getting your child to school every day can be hard, but **you are not alone in this struggle**..."(emphasis original). Phrases like these were meant to strike a more supportive than punitive tone.

The letter also gave the family contact information for the caseworker and the CBO. The letter was sent automatically as soon as the family was marked as "enrolled" (eligible for engagement) in SUSO's case management system, referred to as the Efforts to Outcomes or ETO database.[8]

While the letter was designed to test *engagement* with CBOs, the letter might also impact the underlying attendance challenges. The letter may affect a child's school attendance by encouraging their parents to engage with a CBO or by changing behavior without the family engaging with a CBO. Yet because the letter is sent to families who have already been informed about mild absenteeism issues (3 absences), we are targeting a different population than other absenteeism interventions, which often target students when absences first become apparent rather than in later stages of absence accumulation (Appendix Section 9.1 discusses these past interventions in greater detail).

3 Evaluation Design

All analytic decisions were pre-registered in Version 1 or Version 2 of the Open Science Foundation (OSF) pre-analysis plan unless otherwise specified.

3.1 Randomization

Children's families were randomly assigned either to a control arm receiving no letter, or to a treatment arm receiving a letter. CBO staff followed up with each family as usual. The contents

[7] In Version 1 of the pre-analysis plan, available at this link, we noted on page 3: "The eventual objective is to increase attendance rates, and if the Office of the State Superintendent of Education for the District of Columbia approves a pending request for attendance data for this purpose, we will also assess the impact of the intervention on attendance rates," but did not specify mechanisms. In Version 2 of the pre-analysis plan, available at this link, we noted on page 7 that "The letter may affect a child's school attendance by encouraging their parents to engage with a CBO (pathway A). The letter may also affect a child's attendance by changing behavior without the family engaging with a CBO (pathway B)."

Although there are some randomized control trials assessing the impact of direct mail on service uptake, we were unable to find a randomized control trial of a letter followed by personal contact related to the provision of government-funded services. However, with respect to direct mail and school attendance, Rogers and Feller (2018) found that students whose parents were sent a letter with attendance records had fewer subsequent absences than those in a control group who did not receive the letter.

[8] Efforts to Outcomes is a product of Social Solutions, Inc. When a student reaches 5 unexcused absences and thus becomes eligible for SUSO, the school is supposed to refer the student to the CBO, who then enters the student information into the ETO database. As we show later, however, either because schools were slow to refer students to CBOs or because CBOs were slow to enter the data data into ETO, students entered into the the ETO database had a range of unexcused absences.

of a sample letter are presented in Appendix B. Since engagement and attendance may vary non-randomly over time and by school and CBO, we assigned primary guardians to treatment or control via permuted-block randomization, stratified by school (and thus also by CBO), as they were referred for SUSO-related services.

3.2 Sample and Balance

Using this randomization, we used an intent-to-treat method of assignment to conditions, and letters were sent automatically using data entered into the centralized case-management system (ETO).[9]

Table 1 shows the sample size resulting from this randomization. The sample size differs slightly for different outcomes we discuss in Section 3.3. The majority of our results focus on the analytic sample for the attendance outcomes (third row). This sample consisted of 1,198 students who could be exact or fuzzy matched to the Office of the State Superintendent of Education (OSSE) attendance data, which represents 94% of the original sample. The short-term attendance outcomes lose 10 students due to incomplete day-level attendance around the time of their referral—for instance, students who, due to out-migration from DC, have no attendance information during that two-week period.

Table 1: **Sample size for different analytic samples**

Sample	Description	Treatment N	Control N	Total
Engagement Analytic	All students/parents regardless of whether they have attendance data	649	625	1274
Attendance analytic end-of-year outcomes	Only students/parents that could be matched to attendance records	605	593	1198
Attendance analytic short-term outcomes	Above minus 9 students with incomplete attendance records surrounding the referral date (see Appendix)	597	591	1188

As discussed in greater detail in Section 9.6.2, we examined the balance between the treatment and control groups in two ways: (1) comparing the count of treatment and control students by school, the blocking variable for randomization, and 2) comparing the treatment and control students on attributes correlated with attendance (e.g., whether or not a student's family is experiencing homelessness).

3.3 Outcomes

For engagement, we look at whether or not the CBO successfully engaged the family within two weeks of the student becoming eligible for SUSO and also examine whether the CBO engaged the family anytime during the remainder of the school year.

[9]Interviews with CBOs suggested that members of both the control and the treatment groups sometimes have incorrect contact information, and so some letters were likely never delivered due to erroneous contact information.

For attendance, we examine two time horizons. First are short-term changes in attendance, measured using absences in the two weeks following the date that letters were delivered (Section 3.3.1). Second are longer-term changes in attendance, measured using absences in the remainder of the school year, a time horizon that varied based on when students became eligible for the intervention (Section 3.3.2).

For both time horizons of attendance outcomes, we use DC's administrative categories that distinguish between two types of absences:

- **Excused absences:** these are absences where the family provides a "valid explanation" to the school within a reasonable timeframe. Valid excuses can include student illness, a death in the student's family, medical or dental appointments, an out-of-school suspension, or several other reasons.[10]

- **Unexcused absences (the primary focus of SUSO):** these are absences without a valid excuse (or where the excuse is submitted outside a reasonable timeframe).

3.3.1 Short-term outcomes

First, and of primary interest, was the total count of *unexcused absences* in the two weeks following the intervention.

Second, to see whether reductions in unexcused absences come from shifts into excused absences, we examined the total count of *unexcused and excused absences* immediately following the intervention. This allows us to observe whether the intervention encouraged parents to follow proper protocols to excuse their child's absences (e.g., submit proof of a doctor's appointment for an excused absence) rather than actually have their child attend school more often.

For measuring impacts in the short term, we need a start date to begin tallying absence counts and the most relevant start date is when the family receives a letter. However, (1) this letter arrival date is only defined for treatment group families sent a letter and not control group families and (2) for some treatment group families, we do not know when the letter arrived. Therefore, and as pre-registered since we anticipated challenges defining this time window,[11] our analysis begins tallying absence counts using two start dates:

1. **Starting at letter delivery date ("observed delivery date" for shorthand):** The first start date begins the absence tally at the observed delivery date of the letter. For treatment group participants, USPS provides confirmation that a letter is delivered and the timestamp of delivery. Since randomization was done by blocking within a school and then consecutively assigning students to the treatment and control group, control group participants were assigned the delivery date of the treatment group students in their same school with the closest referral date.[12] This version of the outcome variable maximizes the chance that we

[10] See https://attendance.dc.gov/page/attendance-faqs for a full set of acceptable reasons.

[11] Pre-analysis plan version 2 at this link Section H, pages 16-17.

[12] The Jupyter notebook named 070_attendance_descriptives.ipynb in our code describes the process in greater detail, which involved (1) starting with a focal control group student, (2) pulling a pool of treatment group students in the same school as the focal control group student, (3) calculating the difference in referral date between the focal control student and each of the treatment group students in their school, (4) subsetting to treatment group student(s) with the closest referral date(s)–there could be more than one if multiple treatment students are referred on the same day, (5) if there are multiple treatment group students who have an equally close referral date to the control group students, finding the earliest date a letter was delivered to one of these students and the latest date a letter was delivered, (6) if those two dates differ, taking the average. This process was repeated using two pools of treatment students—ones whose delivery dates were observed (75%) and ones whose delivery dates were either

are correctly measuring attendance immediately following letter receipt but may introduce some bias caused by non-random variation in 1) when a student receives a letter and 2) if the delivery date is correctly recorded. The drawback is that, as we explain in greater detail later, about 25% of treatment group students are missing a delivery date. For these students, we assigned them the median delivery date of 7 days. In addition, about 15% of control group students are missing information in the ETO database about what school they were enrolled in at the time of referral,[13] so they could not be matched to a treatment student in their same school with the closest referral date. These non-matchable control students are also assigned the median delivery date.

2. **Starting at median letter delivery date:** The second start date began counting absences from the student's referral date plus one calendar week (what we call the "median delivery date" start date for shorthand). The median time between 1) the date a student was referred to SUSO, and 2) the date the student received the outreach letter, is seven calendar days (Figure 2). In contrast to starting at the delivery date, starting seven days after referral minimizes the bias caused by non-random variation in delivery time or a missing delivery date. However, for some students, this approach likely starts tallying additional absences before their family received a letter.

We examined the robustness of the results to either start date and found that it made little difference. For each start date, we then tallied absence counts over the two calendar weeks that follow that start date.[14] As an example, for the first start date, for a student who is referred on a Monday and has their letter delivered that Thursday, we started counting absences for that Thursday plus two calendar weeks (so, for instance, for a student with letter delivery date of Thursday, February 8th, we count absences from Thursday, February 8th–Thursday, February 22nd (inclusive of dates on each end), even if there is a holiday or school break during that period).

3.3.2 Long-term outcomes

We also examined the letter's impact on two binary outcomes that were measured over the entire duration of the school year:

1. **Truancy:** This is a binary outcome indicating that the student has 10 or more unexcused absences during the course of the school year and that the student was not already truant at the time of referral.[15] This represents the longer-term effect of the letter on a key absence

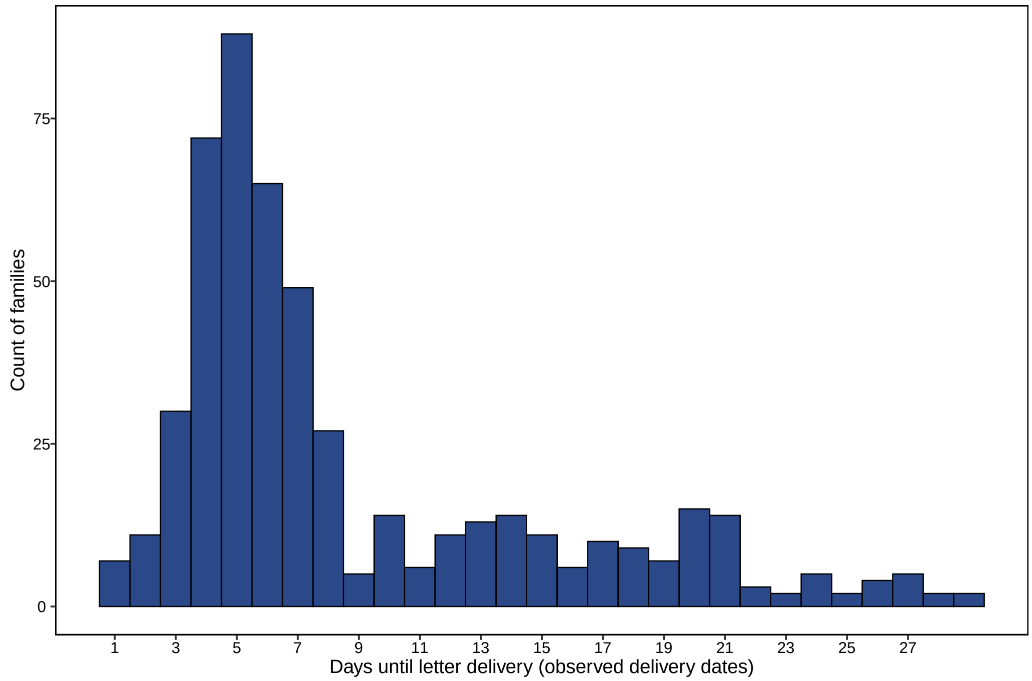
observed or imputed to 7 days post-referral (all). For treatment group participants, USPS provides confirmation that a letter is delivered and the timestamp for delivery. This system is not perfect, however, and only 75% of sent letters were ever marked as delivered. The results presented focus on the results from matching control group students with the closest-referral date treatment student(s) regardless of whether or not the treatment group student(s)'s delivery dates were observed or imputed.

[13] Some treatment group students were also missing a school but these students either (1) had an observed delivery date we could use or (2) were missing delivery date and assigned to the median.

[14] The DC school attendance data has a flag for which days are not school days (weekends and school holidays), so the "exposure" part of the analysis was able to correctly account for eligible school days at different times during the school year.

[15] Although the policy was that students be referred to SUSO as soon as they accrue 5 unexcused absences, this did not always happen. During the study, the actual process for referrals was as follows: schools manually, and not with perfect reliability or regularity, prepare paper lists of students reaching this count; SUSO CBOs then manually, and not with perfect reliability or regularity, pick up the paper lists and then enter the data into ETO. In addition, some schools initially presumed absences were excused unless parents failed to deliver a note within some number of days. As a result, we observed that some students were referred to SUSO after they had already accrued 10 or more absences.

Figure 2: **Distribution of time between student referred to SUSO and outreach letter to parents delivered**. The median time was 7 days.



metric targeted by the Mayor's *Every Day Counts!* campaign.

2. **Chronic absenteeism:** This is a binary outcome indicating that a student's total count of unexcused and excused absences is greater than 10% of school attendance days for the year. This represents the longer-term effect of the letter on another key absence metric targeted by the Mayor's *Every Day Counts!* campaign.

4 Research questions

4.1 Does the outreach increase family engagement with CBOs?

We hypothesized that the letter would increase engagement among families contacted. Engagement, in this context, means consenting to discuss the needs the family has and, where appropriate, to receive services or service connection. The engagement rate in the year prior to the intervention was roughly 7.4%, with significant variance across schools and CBOs.

4.2 Does the outreach increase student attendance?

We hypothesized that (1) the letter would decrease the number of unexcused absences and total absences in the two calendar weeks following the actual letter delivery date or the median letter delivery data, (2) the letter would reduce the likelihood of a student being truant, which means the letter reduced the student's unexcused absences, and (3) the letter would reduce the likelihood of a student being chronically absent, which means the letter either reduced the student's unexcused absences or reduced that student's excused absences.[16]

Figure 3 shows that outreach may increase or decrease student attendance as a result of increased engagement with SUSO. For instance, if participating in SUSO's program itself increases or decreases attendance, then a family's increased engagement with SUSO could have a downstream effect on attendance for that student. It is also possible that outreach could decrease attendance if it inadvertently decreases a family's engagement with SUSO, and through that lower engagement, decreases the student's attendance.

Outreach may also affect student attendance regardless of a family's decision to accept SUSO services. One mechanism is *by increasing the salience of absences the family already knew about.* Families are juggling their child's attendance with many other responsibilities and pressures. The letters are designed to remind families that other people—especially their child's school—care about their child's attendance. Furthermore, the letter may remind families that they are not alone in this juggling act and that there are organizations ready and willing to partner with them to navigate past obstacles to attendance. Another mechanism is *by alerting families to absences that they might not have been aware of.* The letter, which says that the organization knows that "getting your child to school every day can be hard" but that does not list the child's actual absence information, may prompt the family to follow up with the school to learn more about their child's absences and, for instance, provide valid excuses for absences that were previously tabulated as unexcused.

5 Methods

5.1 Bayesian Estimation

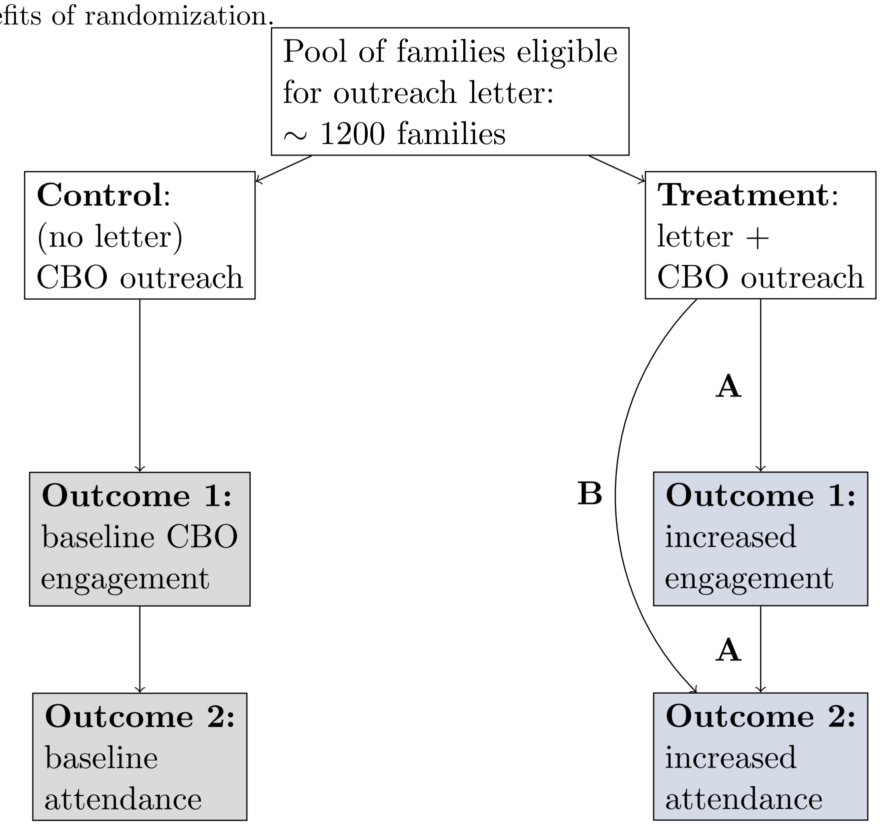
We employed a standard Bayesian analysis to test the hypothesis that subjects in the treatment arm (receiving a letter) 1) have a lower count of unexcused absences in the two-week period following the intervention, 2) have a lower total count of unexcused and excused absences in the two-week period following the intervention, 3) have a lower probability of truancy and chronic absenteeism than subjects in the control arm (not receiving a letter).

Appendix Section 9.3 describes the derivation of the test statistics for each outcome. For the count outcomes, we use a Bayesian version of a Poisson model that includes one parameter for a student's count of absences and another parameter for a student's "exposure," or his or her risk of generating that count. For most students, this exposure was ten school attendance days during the two calendar weeks. For some students, this exposure was less if, for instance, their referral came shortly before a series of school holidays. Our modeling strategy allows us to account for the fact that, for instance, a student who has 0 absences out of 10 school attendance days in which he could have been absent differs from a student who has 0 absences but only 4 school attendance days in which he could have been absent.

We also estimated the size of the effect, including the distribution of uncertainty around that effect size. Finally, we estimated the range of odds of our model being correct across 10,000 simulations from the posterior probability distribution.

[16]Since chronic absenteeism combines excused and unexcused absences, this outcome on its own cannot tell us which type of absence letter might have a stronger impact on. However, the combination of analyzing the letter's effect on truancy with examining the letter's effect on chronic absenteeism can help us better understand the impact on different types of absences. For instance, if the letter has no effect on truancy but a large effect on chronic absenteeism, this would suggest that some of the letter's impact is through reducing excused but not unexcused absences. Conversely, if the letter has a large effect on truancy but no effect on chronic absenteeism, this would suggest that the letter might be better at reducing unexcused absences than reducing excused absences.

Figure 3: **Effect of outreach to families:** We hypothesized two pathways by which the letter could impact attendance: (A) the letter improves attendance via the family's engagement with the CBO; and (B) the letter improves attendance despite no engagement with the CBO (for instance, by increasing the salience of the absences). Lines to the outcomes not of interest are omitted for easier viewing. We analyze outcomes that occur via either pathway–since it is not random which families do or do not engage with CBOs after receiving a letter, subsetting to these families would erase the benefits of randomization.



All analyses are intent-to-treat (ITT)—that is, we examined the effect on outcomes of being assigned to receive a letter regardless of 1) whether or not the letter was delivered and 2) whether or not the family engaged with the CBO.

6 Findings

6.1 The outreach may have decreased family engagement with CBOs

Figure 4 shows the raw differences in the proportion of students engaged across the two groups. SUSO requires that CBOs engage students within 14 days of referral. The plot shows that students in the treatment group—those whose families received a letter—appear both less likely to accept SUSO's offers of help (engage) within this mandated window (right hand side) and also less likely to engage at any time during the school year (left hand side).

Figure 4: **Engagement following referral: treatment versus control group**

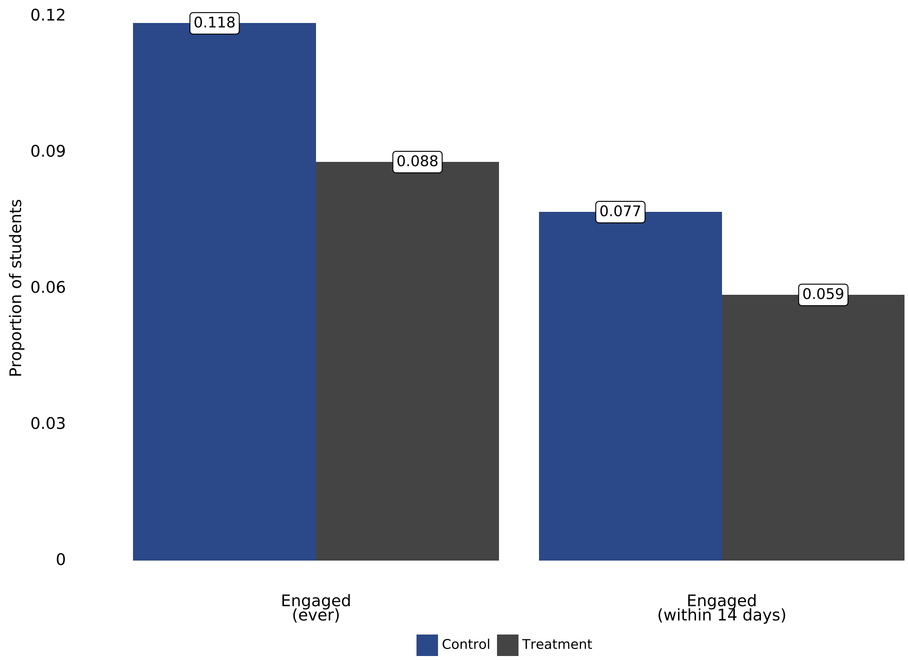


Figure 5 shows the results of Bayesian A/B testing with $m = 10,000$ draws centered at the observed engagement rates from the experiment. The x-axis shows the difference in the rates of engagement between the treatment and control group, with negative values showing lower engagement in the treatment group. The plot then shades in dark blue the proportion of the posterior that lies below 0—that is, the proportion that suggests the treatment group had less engagement than the control group.

Overall, most of the posterior values suggest the treatment group had less engagement than the control group. Figure 6 confirms this interpretation, showing that with a reasonably high degree of certainty (0.80, or 80% of draws), the letter had the *opposite* of its intended effect and may have decreased family engagement with CBOs who offered help.

Figure 5: **How different was engagement with SUSO between the control and treatment group (results from repeated draws from the posterior distribution)** The shading in this figure and similar posterior distribution figures that follow represent draws where the difference is above or below 0.

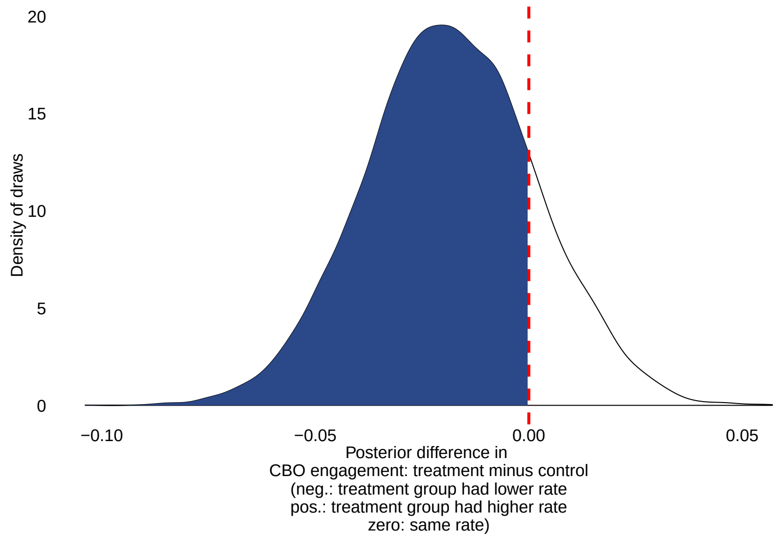
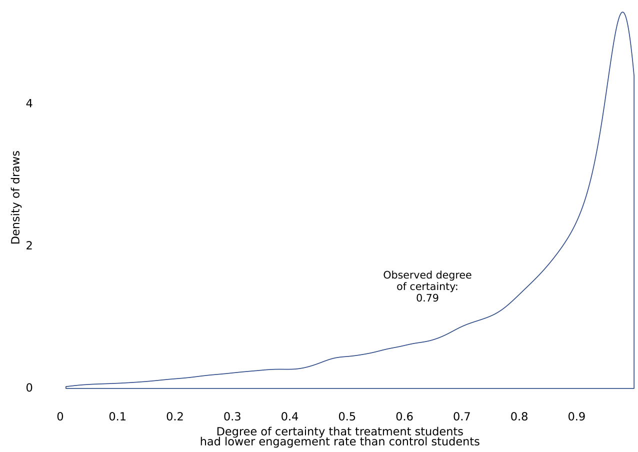


Figure 6: **How different was engagement with SUSO between the control and treatment group (degree of certainty about that difference)**



6.2 The outreach improved attendance in the short-term, but only for unexcused absences and not total absences

The letter offering help to the families decreased engagement with the CBOs providing that help, but it may have led families to make changes related to attendance regardless of engagement.

We therefore also examined the letter's impact on attendance. In particular, the letter was triggered by the student reaching a high count of unexcused absences, and so the primary attendance metric we hope it can improve are unexcused absences immediately following the letter. We also examined total absences: the combined count of unexcused and excused absences. If unexcused absences fall, but total absences do not, then some of the letter's effect comes from parents shifting to providing more valid excuses for absences.

6.2.1 Impact on SUSO target: unexcused absences

First, we look at whether students who received the outreach letter had fewer unexcused absences in the two weeks following letter delivery. We find that the treatment had a statistically and substantively meaningful impact on reducing unexcused absences.

First, comparing the treatment and control groups descriptively, Figure 7 shows the distribution of unexcused absences using the observed letter delivery start date.[17] The figure shows that the treatment group appears to have more students who accrue zero additional absences in the two weeks following the letter. Examining these absences as a rate, Table 2 shows that treatment group students had about 0.013 absences/day fewer during those two weeks than control group students.

[17] As described earlier, our main results use the observed date of the letter delivery as the start of the "clock" to track attendance. Due to issues like a large proportion (∼25%) of students missing a documented delivery date, we also analyze the results' robustness to an alternate start date of one week following referral, which was the median letter delivery time. We then count the number of additional absences the student accrued beginning at that date and ending two calendar weeks later. Appendix Section 9.6 shows the distribution for the median delivery start date, which is very similar.

Figure 7: **Distribution of unexcused absences in the two weeks following the letter: treatment versus control group** The Figure shows that there seems to be a larger peak of 0 unexcused absences in the treatment group relative to the control group, but Table 2 normalizes these counts in a way that accounts for the unequal group sizes.

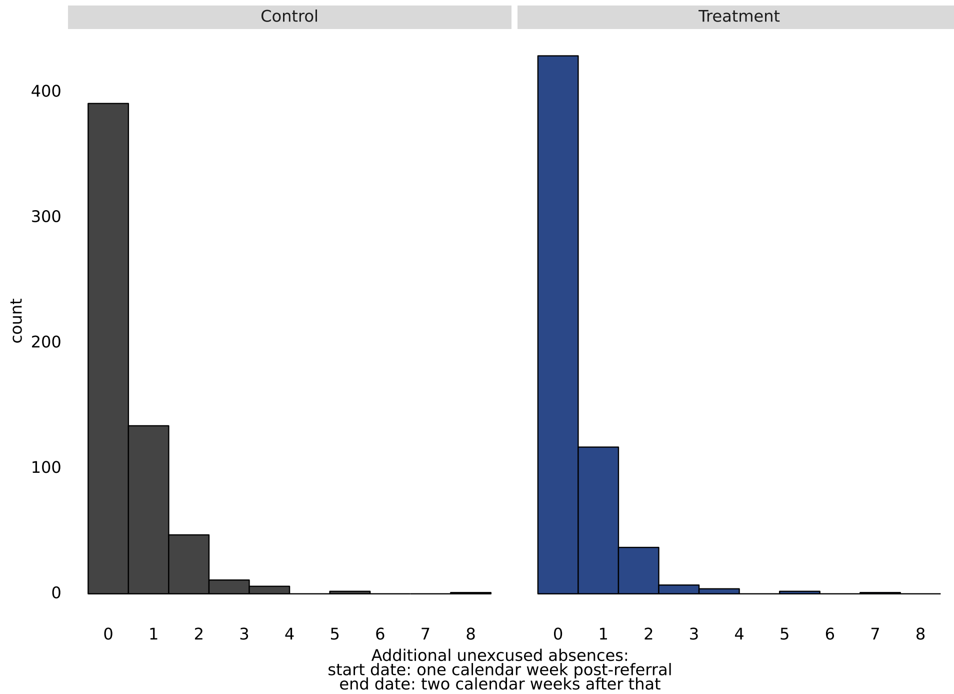


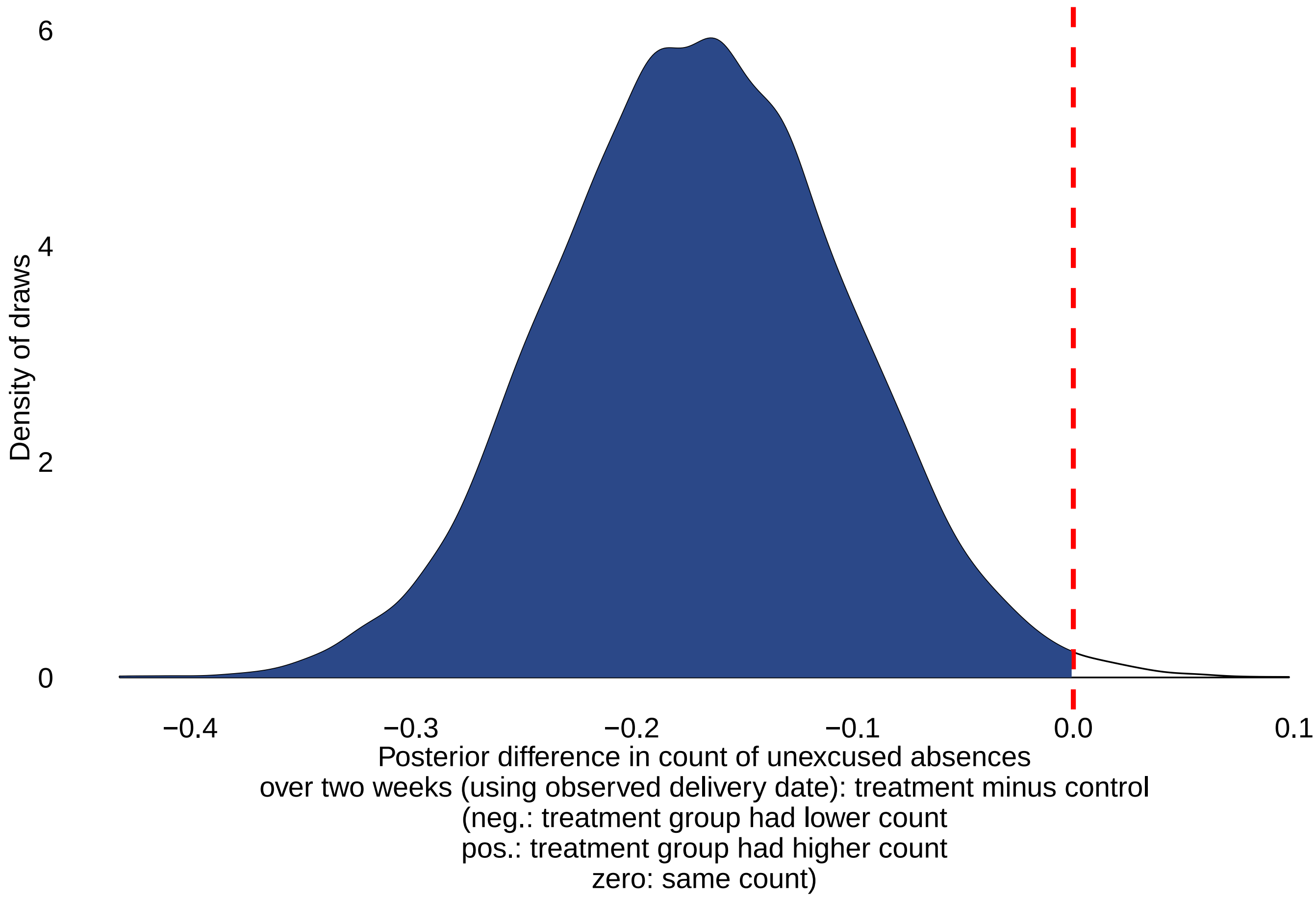
Table 2: **Rate of unexcused absences in the two weeks following the letter**

Absence outcome	**Count (summed across students)**	**School attendance days (summed across students)**	**Rate**
Treatment	252	4996	0.050 absences/ school day
Control	318	5088	0.063 absences/ school day

To see whether these differences are statistically significant, we use Bayesian A/B testing to assess our certainty that the treatment did indeed cause reductions. Figure 8 shows the results of Bayesian A/B testing with $m = 10,000$ draws centered at the observed absence counts from the experiment. The x-axis shows the difference in the posterior rate of absences over school attendance days between the treatment group and the control group. Negative values show that the treatment group had a lower count of unexcused absences (adjusting for the number of school days in those two weeks). For ease of interpretation, we scale these posterior differences from absences per day to absences over a two-week period. The plot then shades in dark blue the proportion of the posterior that lies below 0—that is, the proportion that suggests the treatment group had fewer unexcused absences than the control. We want this proportion to approach 1.

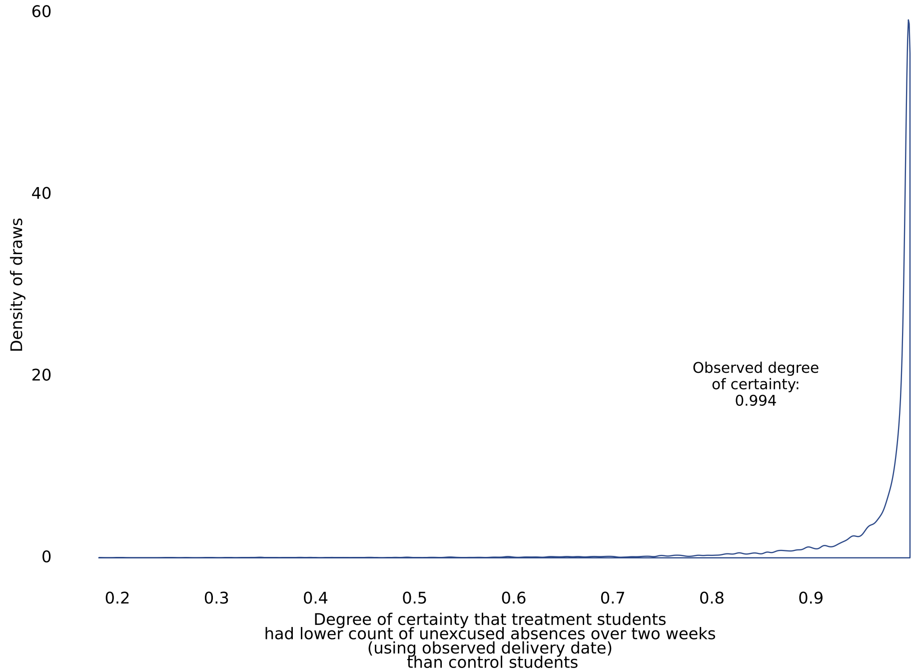
Overall, most of the posterior values suggest the treatment group had fewer unexcused absences than the control group. This makes us highly certain that the treatment reduced unexcused ab-

Figure 8: **How different were short-term unexcused absence counts between the control and treatment group? (results from repeated draws from the posterior distribution)**



sences. Figure 6.2.1 supports that high degree of certainty. The plot shows that the distribution of $m = 10,000$ draws peaks at a high degree of certainty—0.994—that the treatment caused fewer unexcused absences in the short-term.

Figure 9: **How different were short-term unexcused absence counts between the control and treatment group? (degree of certainty about that difference)**



6.2.2 Impact on total absences

The previous results show that the letter outreach had a significant short-term impact on reducing unexcused absences. Here, we find that it had no differences on total absences.

Examining the two groups descriptively, Figure 10 shows the distribution of total absences for each group in the two weeks following the letter, which show less pronounced differences than unexcused absences.[18] Similarly, while the treatment group had 0.13 *fewer* unexcused absences per day than the control group (Table 2), Table 3 shows that there were few differences in total absences (0.01 per day higher in the treatment group).

[18] Appendix Section 9.6 presents a similar figure using the median letter delivery start date.

Figure 10: **Distribution of total absences in the two weeks following the letter: treatment versus control group** The figure shows few observable differences, though Table 3 provides a comparison normalized for the unequal group sizes.

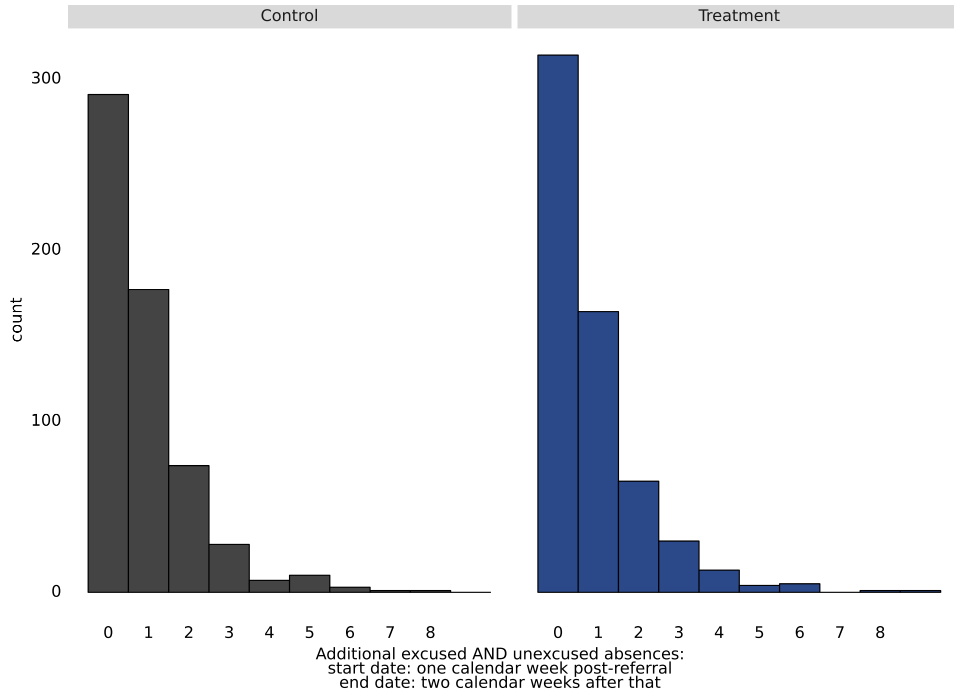


Table 3: **Rate of total absences in the two weeks following the letter**

Absence outcome	**Count (summed across students)**	**School attendance days (summed across students)**	**Rate**
Treatment	522	4996	0.104 absences/school day
Control	524	5088	0.103 absences/school day

We now see whether the differences are statistically significant. In contrast to the impact on unexcused absences, the treatment appeared to have no impact on total absences (Figure 11). Put differently, the reductions in unexcused absences were offset by increases in excused absences, leading to no difference in total absences.

In particular, Figure 11 shows a peak at 0 when we examine the differences between the treatment and control. We are left to conclude the treatment neither reduced nor increased total absences. Similarly, our certainty that the treatment reduced this absence count is close to 0.5 (0.687) (Figure 6.2.2), which is close to what we would observe if we flipped a coin about whether the treatment caused an increase or decrease in this measure.

Future research could explore mechanisms behind the treatment reducing unexcused absences but having no impact on total absences. For instance, did the treatment, which told parents about their child's unexcused absences, prompt parents to fix faulty school attendance records that failed to reflect legitimate excuses?

Figure 11: **How different were short-term total absence counts between the control and treatment group? (results from repeated draws from the posterior distribution)**

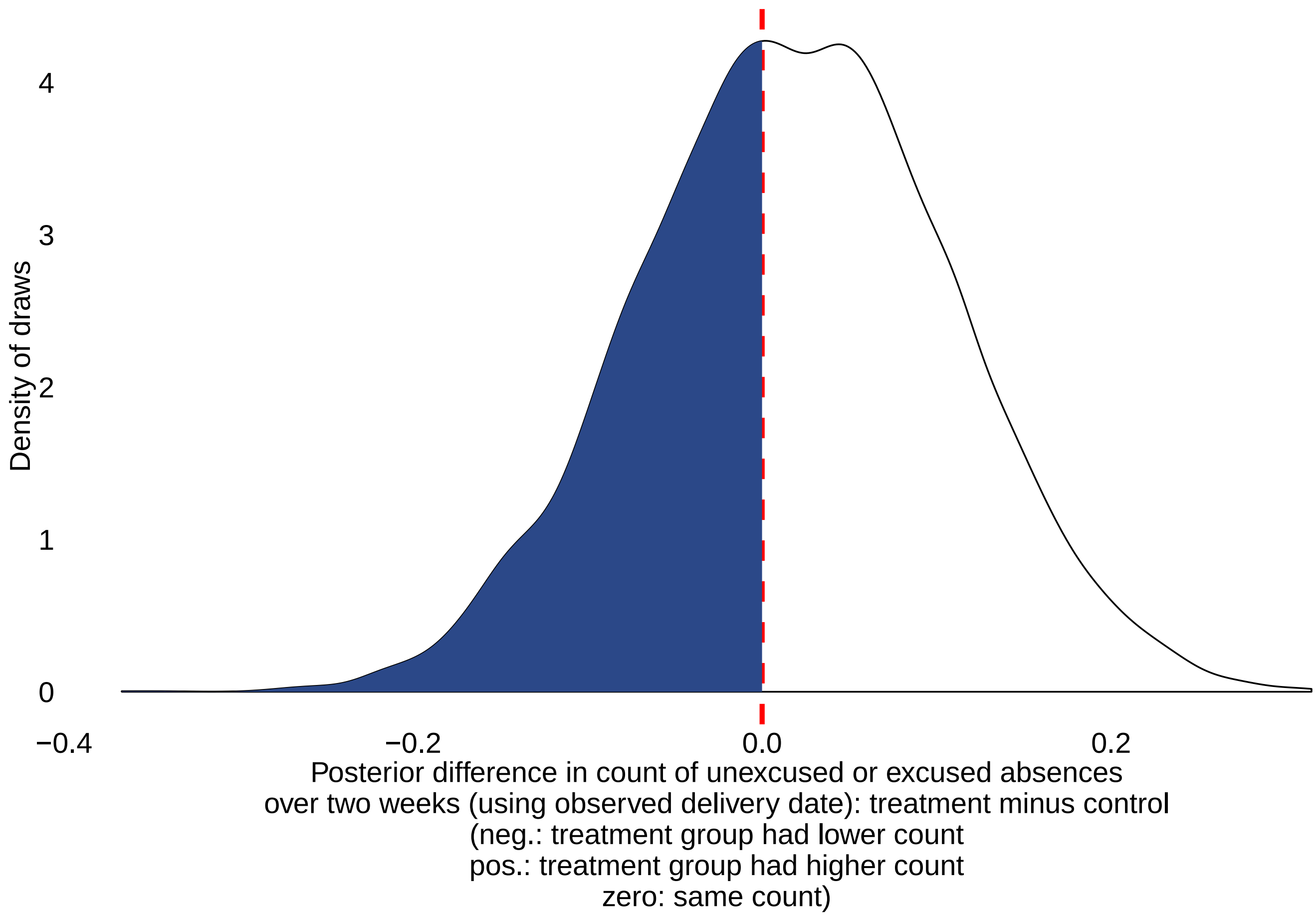
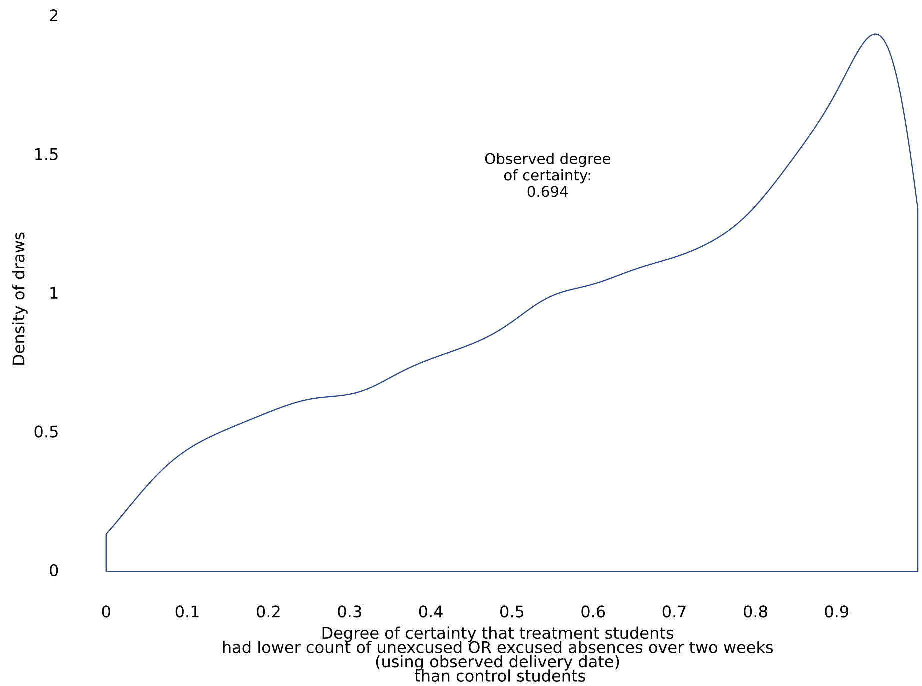


Figure 12: **How different were short-term total absence counts between the control and treatment group? (degree of certainty on that difference)**



6.3 The outreach did not improve attendance over the duration of the school year

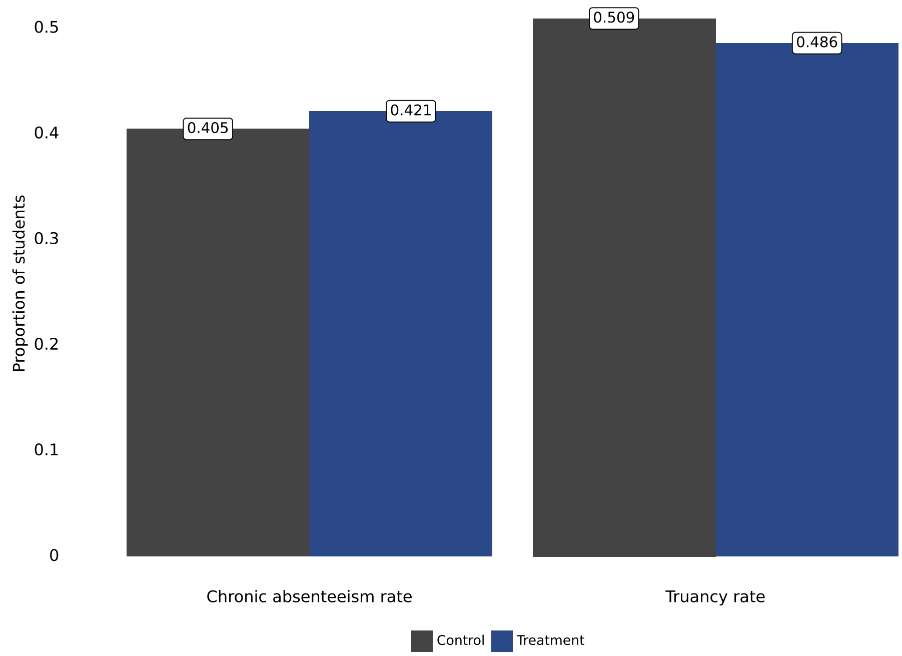
The previous results suggest that the letter reduced student's unexcused absences in the two weeks following its delivery. That reduction is offset by the fact that the treatment had no effect on total absences. This means that, rather than changing who showed up for school, the treatment changed parents' propensity to provide valid reasons for why the student did not show up.

In addition, while short-term effects are important, the motivation behind SUSO and other attendance engagement efforts is to reduce unexcused absences over the course of the school year. Reducing unexcused absences helps students avoid being classified as chronically truant, which occurs when a student has 10 or more unexcused absences. Reducing truancy helps students and families avoid punitive interventions, which can include referrals to CFSA for students aged 5-13 (the target population for SUSO). Therefore, in addition to examining the short-term outcomes, we also investigated the impact on two year-end attendance metrics that are important targets of DC's *Every Day Counts!* campaign: truancy (10 or more unexcused absences) and chronic absenteeism (missing more than 10% of the school year, or about 18 days, regardless of whether the absences are excused or unexcused).[19]

Figure 13 compares chronic absenteeism and truancy between treatment and control group students. Treatment group students had slightly lower truancy at year end—about 2.3 percentage points lower. Our Bayesian A/B testing shows that we are only moderately confident that this represents a real difference. Figure 6.3 plots the posterior distribution of differences in the truancy

[19] Appendix Section 9.6.3 shows the dates that students were referred, which ranged from January of 2018 to early June, meaning that these outcomes reflect absences that accrue anywhere between less than a month to 6 months after a letter is delivered.

Figure 13: **End-of-year truancy and chronic absenteeism: differences between treatment and control group in raw proportions** The figure shows that while the treatment group students had slightly lower end-of-year truancy, and slightly higher end-of-year absenteeism, the differences are not large.



rate between the treatment group and the control group. The figure shows a slight shift to the left, which means the treatment group's truancy is lower. Figure 6.3 quantifies our degree of certainty that the treatment caused lower truancy. In particular, our degree of certainty that the treatment group had lower truancy is 0.79. Thus, while there is some evidence suggesting that the reductions in year-end truancy are not due to chance, our degree of certainty about this statement is much lower than our certainty that the treatment reduced short-term unexcused absences (0.994), and about the same as our certainty that the treatment decreased engagement (0.80).

Focusing on chronic absenteeism, which was slightly higher in the treatment group, Figure 16 plots the posterior distribution of differences in the chronic absenteeism rate. The mass towards the right shows that across most draws, the control group has lower chronic absenteeism.

Figure 17 quantifies the degree of certainty that the treatment group has lower chronic absenteeism—the desired outcome of the intervention—across m = 10,000 draws, with a peak at 0.278 that the letter caused a *decrease* in end-of-year chronic absenteeism. Put differently, our degree of certainty that the letter caused an *increase* in year-end chronic absenteeism is 0.72—far less certain than the short-term effects and less certain than the attendance and engagement outcomes.

Figure 14: **How different was end-of-year truancy between the control and treatment group? (results from repeated draws from the posterior distribution)**

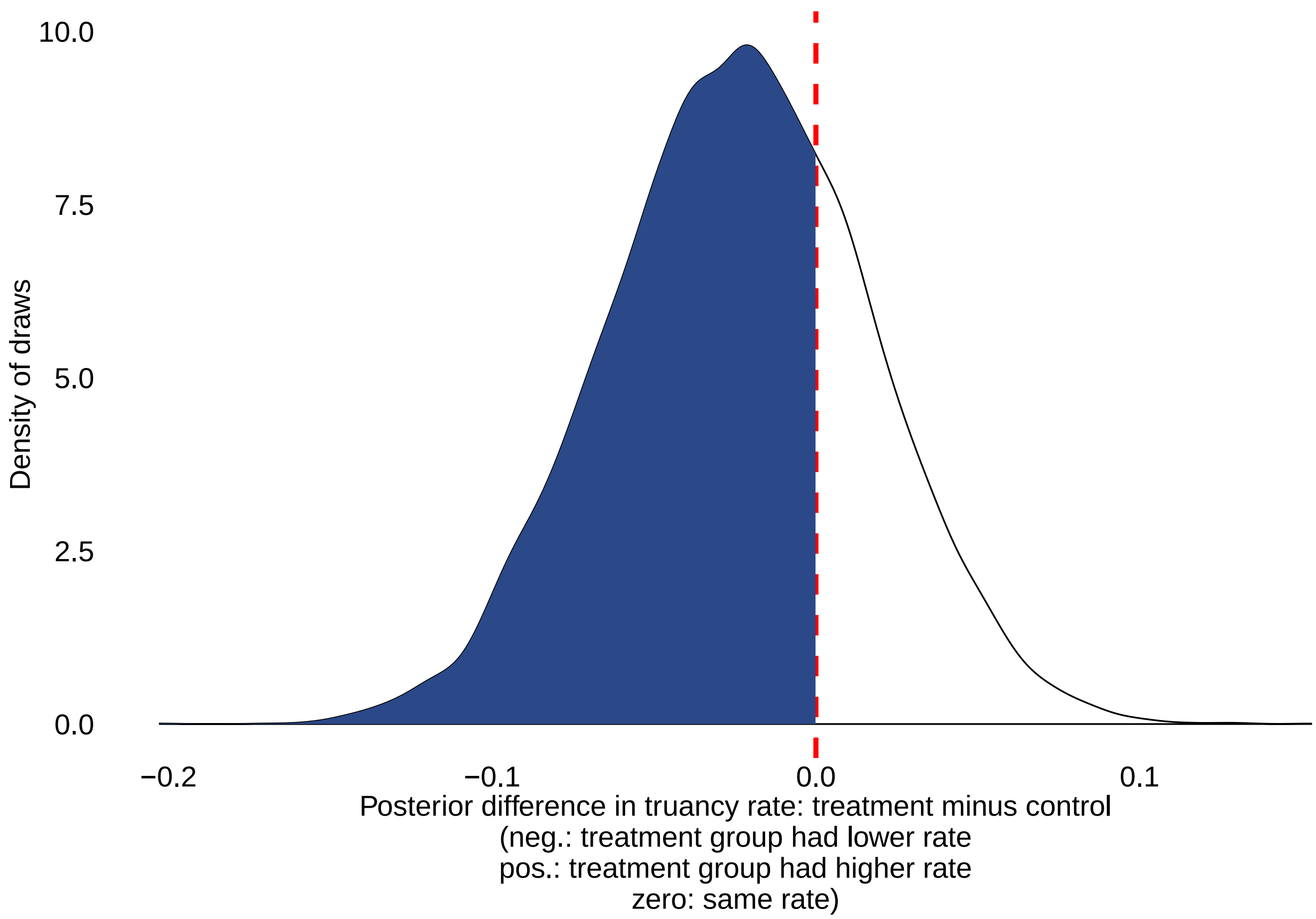


Figure 15: **How different was end-of-year truancy between the control and treatment group? (degree of certainty on that difference)**

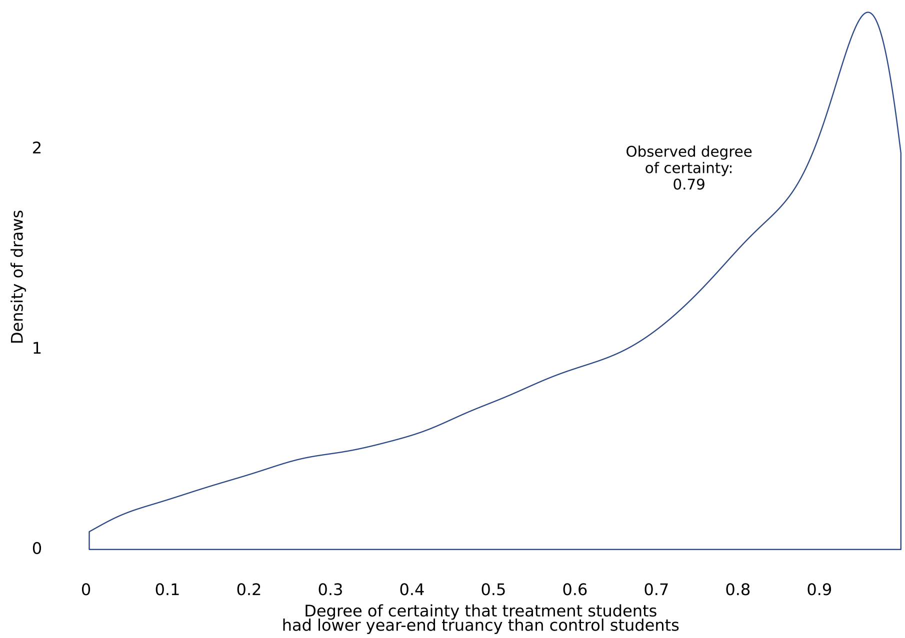


Figure 16: **How different was chronic absenteeism between the control and treatment group? (results from repeated draws from the posterior distribution)**

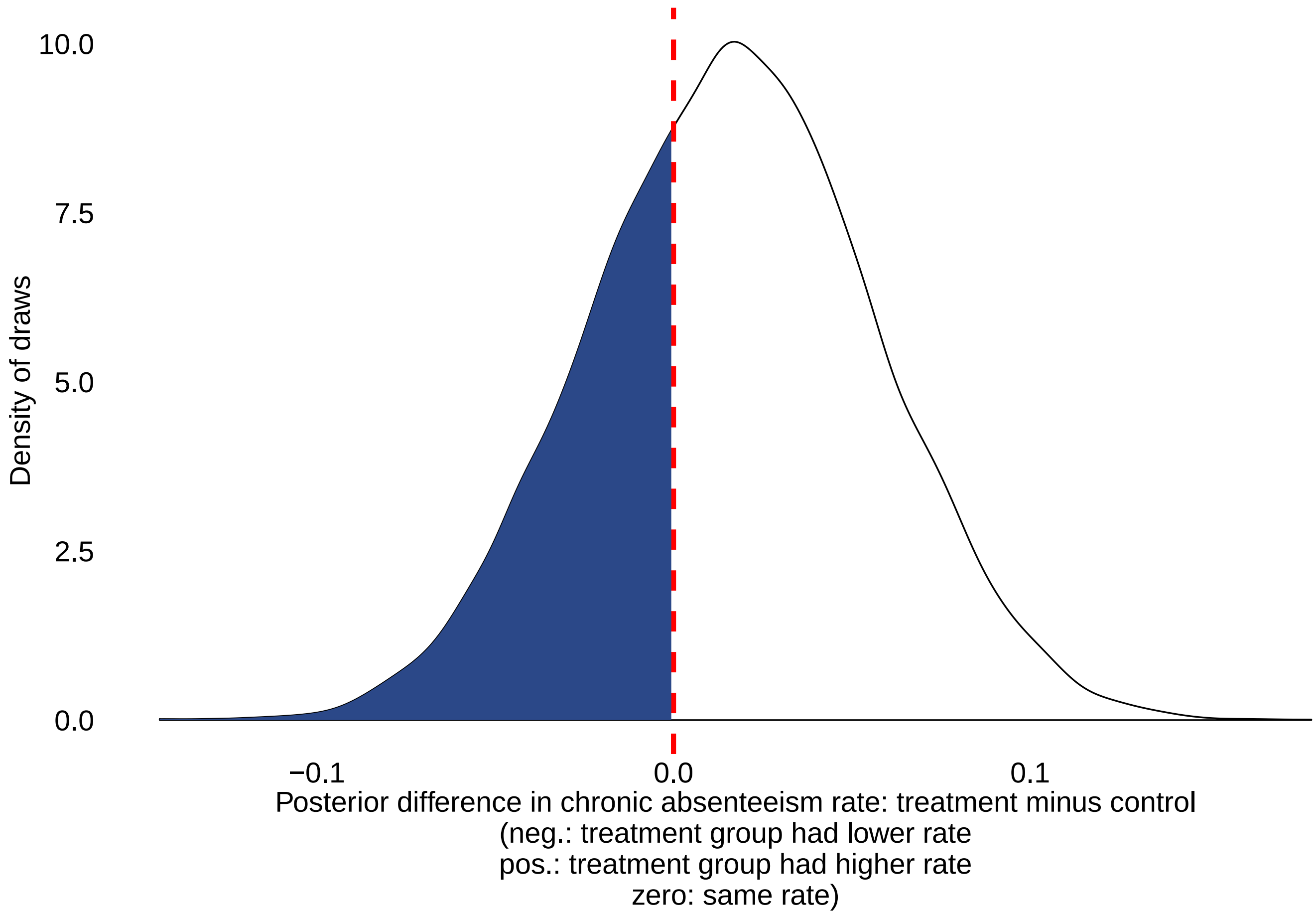
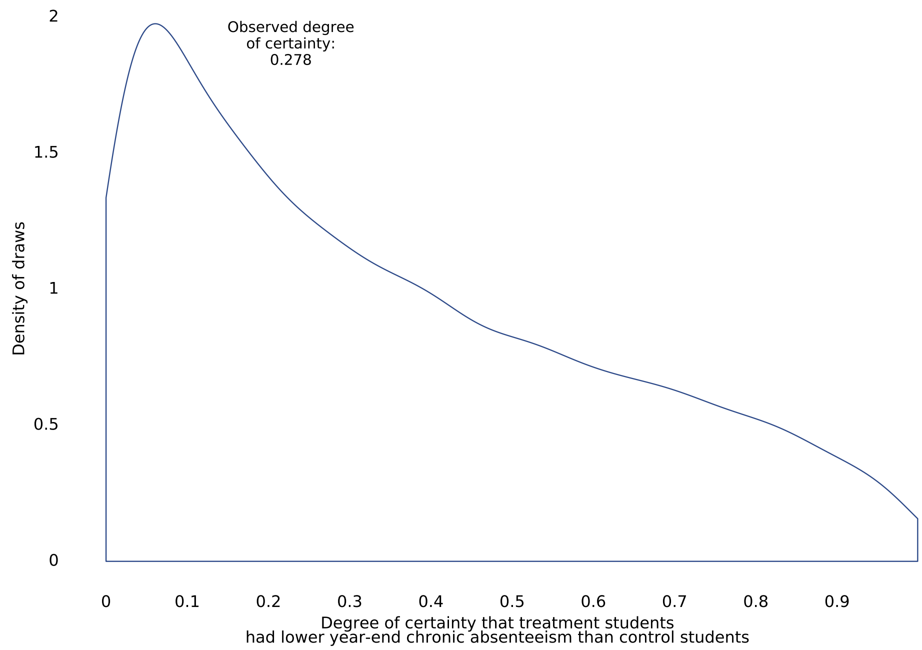


Figure 17: **How different was chronic absenteeism between the control and treatment group? (degree of certainty on difference)**



6.4 Summing up

Summarizing these results together, we are highly confident that the letter reduced unexcused absences in the short term and reasonably confident that it (1) reduced engagement with CBOs and (2) reduced year-end truancy. Our evidence shows, however, that there was almost certainly no impact on total absences either in the short term or at the end of the year.

6.5 Robustness checks that supplement Bayesian estimation with regression

While the main analysis used a Bayesian A/B test, other studies discussed in Appendix Section 9.1 use regression to examine the effect of outreach to parents on student attendance outcomes. For comparability with these past studies, we also analyzed each of the attendance outcomes using regression. In addition, since the balance checks revealed imbalance that we discuss in Appendix Section 9.6.2, the regression allows us to adjust for covariates that remain imbalanced due to the size of the randomized sample.

Reflecting debates about whether analyses of randomized treatments should control for variables that may remain imbalanced between the two groups, we used three regression specifications.

First is a specification that only includes the treatment indicator, which we pre-registered:

$$y_i = \alpha + \beta \times \text{sent letter } (1 = \text{yes})_i + \epsilon_i \tag{1}$$

where i indexes $i = 1, 2, ..., N$ students and where β is the coefficient of interest (the intent-to-treat estimate on students sent the letter).

Second is a specification that includes all pre-treatment attributes described in Appendix Table 8 except for indicators for which school the student is in (the blocking variable). This is because, with approximately 61 schools in our analytic sample, the school fixed effects end up absorbing a lot of degrees of freedom. We did not pre-register this analysis so we interpret the results with more caution:

$$y_i = \alpha + \beta \times \text{sent letter } (1 = \text{yes})_i + \gamma_{X_i} + \epsilon_i \tag{2}$$

where notation is the same as above and γ is a vector of coefficients on student-level pre-treatment covariates.

Third is a specification that, in addition to using student attributes, also includes an indicator for the blocking variable (the school the student was enrolled in at the time of referral), which we denote as δ:

$$y_i = \alpha + \beta \times \text{sent letter } (1 = \text{yes})_i + \gamma X_i + \delta_i + \epsilon_i \tag{3}$$

For each, the model we fit depends on the outcome variable. For the absence count outcomes, we fit two types of models. First was linear regression—while the outcome is not normally distributed, past studies use this model and we use it here for the purposes of comparability (Bergman and Chan, 2017). Second is a negative binomial regression. We used negative binomial because the outcome is a count of absences; negative binomial allows the mean of this count of absences (e.g., 1 day) to differ from the variance of this count of absences.

Table 4 shows that the linear regression model confirms the strong results from the Bayesian A/B testing: the letter significantly reduced unexcused absences in the two-weeks following receipt. Put in more intuitive terms, the treatment causes a reduction in unexcused absences of between 0.11 and 0.12 days over two weeks, depending on the specification, a result in line with past effects of outreach to parents about attendance (Bergman and Chan, 2017; Rogers and Feller, 2018). Table

5 shows that the negative binomial regression produces similar findings, though is less well-powered at our sample size.

Appendix Section 9.8 presents the results for (1) the short-term combined count of absences (which show no statistically significant change), (2) year-end truancy (which shows coefficients consistent with a decrease but that are not statistically significant), and (3) chronic absenteeism (which shows coefficients consistent with an increase but that are not statistically significant).

Table 4: Linear regression results: does the letter decrease unexcused absences in the two-weeks following letter delivery? (observed delivery date)

	Dependent variable: unexcused absence counts (observed delivery date)		
Treatment (1 = yes)	−0.116	−0.110	−0.115
	(0.052)	(0.052)	(0.052)
	p = 0.028**	p = 0.036**	p = 0.028**
Hispanic (ref: Black)		−0.063	−0.074
		(0.123)	(0.130)
		p = 0.612	p = 0.573
White (ref: Black)		−0.383	−0.464
		(0.418)	(0.416)
		p = 0.360	p = 0.265
Other race/ eth (ref: Black)		−0.262	−0.259
		(0.274)	(0.279)
		p = 0.340	p = 0.354
Female (ref: male)		−0.006	−0.017
		(0.052)	(0.053)
		p = 0.913	p = 0.747
Limited English prof. (1 = yes)		−0.273	−0.390
		(0.148)	(0.152)
		p = 0.065*	p = 0.011**
Homeless (1 = yes)		0.126	0.103
		(0.074)	(0.077)
		p = 0.091*	p = 0.180
Free lunch (ref: CEP)		0.113	0.002
		(0.151)	(0.477)
		p = 0.457	p = 0.997
Paid lunch (ref: CEP)		−0.111	−0.017
		(0.230)	(0.306)
		p = 0.629	p = 0.955
Reduced lunch (ref: CEP)		−0.289	−0.255
		(0.399)	(0.560)
		p = 0.470	p = 0.650
At-risk (1 = yes)		0.155	0.195
		(0.069)	(0.070)
		p = 0.026**	p = 0.006***
Economically disadvantaged (1 = yes)		−0.201	−0.138
		(0.262)	(0.382)
		p = 0.443	p = 0.718
Constant	0.538	0.530	0.240
	(0.037)	(0.297)	(0.502)
	p = 0.000***	p = 0.075*	p = 0.634
Observations	1,188	1,188	1,188
R^2	0.004	0.031	0.120
Adjusted R^2	0.003	0.021	0.061
Residual Std. Error	0.905 (df = 1186)	0.897 (df = 1175)	0.878 (df = 1113)
F Statistic	4.880** (df = 1; 1186)	3.099*** (df = 12; 1175)	2.050*** (df = 74; 1113)
School Fixed Effects?	No	No	Yes
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 5: Negative binomial regression results: does the letter decrease unexcused absences in the two-weeks following letter delivery? (observed delivery date)

	Dependent variable: unexcused absence counts (observed delivery date)		
Treatment (1 = yes)	−0.213	−0.188	−0.217
	(0.108)	(0.107)	(0.105)
	p = 0.049**	p = 0.079*	p = 0.039**
Hispanic (ref: Black)		−0.217	−0.199
		(0.275)	(0.291)
		p = 0.431	p = 0.494
White (ref: Black)		−23.200	−28.400
		(49,268.000)	(611,039.000)
		p = 1.000	p = 1.000
Other race/ eth (ref: Black)		−0.874	−0.841
		(0.785)	(0.776)
		p = 0.266	p = 0.279
Female (ref: male)		−0.032	−0.061
		(0.107)	(0.105)
		p = 0.764	p = 0.563
Limited English prof. (1 = yes)		−1.004	−1.206
		(0.395)	(0.405)
		p = 0.011**	p = 0.003***
Homeless (1 = yes)		0.198	0.174
		(0.140)	(0.144)
		p = 0.158	p = 0.228
Free lunch (ref: CEP)		0.015	−0.065
		(0.303)	(1.460)
		p = 0.961	p = 0.965
Paid lunch (ref: CEP)		−0.416	0.206
		(0.505)	(0.626)
		p = 0.411	p = 0.743
Reduced lunch (ref: CEP)		−23.250	−27.870
		(44,724.000)	(536,445.000)
		p = 1.000	p = 1.000
At-risk (1 = yes)		0.418	0.503
		(0.157)	(0.156)
		p = 0.008***	p = 0.002***
Economically disadvantaged (1 = yes)		−0.253	−0.022
		(0.627)	(1.322)
		p = 0.687	p = 0.987
Constant	−2.767	−2.819	−3.952
	(0.074)	(0.681)	(1.125)
	p = 0.000***	p = 0.00004***	p = 0.0005***
Observations	1,188	1,188	1,188
Log Likelihood	−1,105.000	−1,081.000	−1,031.000
θ	0.745*** (0.106)	0.875*** (0.133)	1.316*** (0.245)
Akaike Inf. Crit.	2,214.000	2,187.000	2,212.000
School Fixed Effects?	No	No	Yes
Note:			*p<0.1; **p<0.05; ***p<0.01

7 Discussion

We tested whether outreach to parents offering help addressing their child's attendance challenges (1) prompted parents to engage with community-based organizations who offered them help, and (2) caused improvements in student attendance. We find that the outreach letter may have caused families to be *less likely* to accept the community-based organizations' offers of help. Instead, the letters may have led families to take corrective action focusing on providing valid reasons for why a student was missing school, with the letter causing a short-term reduction in unexcused absences but no changes in short-term total absences. Finally, the letter's effects were strongest over the two weeks following its delivery, and had weakened significantly by the end of the school year. In sum, the results show that while one-way outreach to families about their child's attendance challenges *can* produce some changes in behavior, families might require more intensive efforts—for instance, help with transportation; sustained two-way communications with teachers—for their children to have more sustained improvements in attendance. For the broader literature, the results are consistent with past studies that show (1) some short-term effects of letters in the window immediately following delivery (Appendix Section 9.1) but that (2) "information alone" is rarely enough to meaningfully change long-term outcomes (Bird et al., 2021).[20]

8 Policy Implications

Because our findings suggest that the letters may not encourage families to participate in SUSO, OVSJG put a temporary stop on sending letters to families. In light of these results, OVSJG is investing in additional technical assistance and training for CBOs to increase the number of families accepting support services.

We are currently discussing with OVSJG what the attendance results—which show the letter has strong effects on unexcused absences in the short term, but weak effects on the program's target of year-end truancy and no effects on total absences—mean for the program and its implementation.

[20] More specifically, Bird et al. (2021) find that, despite initial positive results of "nudge interventions" for education outcomes like FAFSA completion and college enrollment, these efforts fail to scale to meaningful changes in outcomes in large samples. The higher-dose interventions we discuss above—transportation vouchers for families experiencing homelessness; two-way text messaging between families and teachers—move beyond nudges.

9 Appendix

9.1 Other Parent-Focused Absenteeism RCTs

Table 6: **Other parent-focused absenteeism interventions** For a review of a variety of anti-absenteeism interventions, see Balu and Ehrlich (2018)

School District	Intervention	Sample	Outcome/results
Philadelphia (Rogers and Feller, 2018)	Treatment: Mailer to parents with either general reminder, information about total absences, or information about relative absences; Control: No mailer	All students in grades 1-12 with following eligibility: Enrolled in non-charter, non-specialized school; Not flagged as homeless; No IEP; Did not have perfect attendance in previous year; Deliberately targeted at students who, based on previous year absences, they thought would be most impacted: Lower bound: missed at least three more days than the modal number of absences in their specific-school grade; Upper bound: did not miss more than 2 SD's above their grade-school mean	Number of days absent (found average of 1 day reduction) over any time horizon; Largest effect was in the week immediately following delivery of the mailer
West Virginia (Bergman and Chan, 2017)	Treatment: text message to parents with by-class absences (also tested notifications about missed assignments and low grades); Control: no alert	Started with all students in grades 5-11 and enrolled those whose parents consented to randomization ($N \sim 10,000$) and enrolled students whose parents consented to study ($N \sim 1137$)	Number of additional classes attended (34 more classes; or 12% increase over control group mean.)
New York City (Balu et al., 2016)	Treatment: text messages automatically triggered/sent on same day as absences	High school students whose parents had active cell phones	No statistically significant changes in absences during the study period
Large urban public school district (Lasky-Fink et al., 2020)	K-12 students who reach truancy threshold (generally 5 unexcused absences)	Treatment: different modified letters informing parents about truancy (simplification; emphasis on parent efficacy; absences can add up; absences can add up and superintendent signature; absences can add up and tips on improving attendance; absences can add up and benefits of good attendance)	Number of days absent; found statistically significant reduction of 0.05 days of absences in 10 days following each mailing

9.2 Example Letter

December 21, 2017

Dear Jane Parent,

I know getting your child to school every day can be hard, but **you are not alone in this struggle**. Some DCPS School and BoysTown Washington DC are joining forces to provide you with what you need to make this a great school year.

I work for BoysTown Washington DC. We have already connected with many Some DCPS School families to support them with:

- school supplies
- bus and metro passes
- clothes for school
- food
- housing resources
- summer camp
- and more.

I will reach out to you soon to ask how I can help your family and child succeed. If you want to reach me before then, you can call me at 202-630-4679.

I am here for you.

Joe Caseworker
4801 Sargent Road NE
Washington, DC 20071

9.3 Derivation of A/B Test Statistics

Both formulas are implemented in The Lab's Python toolkit at this link: https://github.com/thelabdc/OVSJG-SUSO-public/blob/master/src/suso/abtesting.py

9.3.1 Binary Outcome

We employ a standard Bayesian analysis of A/B tests. Specifically, given two groups A and B, we want to measure which group is more likely to respond. The two groups, in this case our control (not receiving a letter) and treatment (receiving a letter), differ only by our intervention, and so we attribute this difference in likelihoods to our intervention. In more concrete terms, we wish to measure whether p_A (the probability of a response from group A) is greater than p_B (the probability of a response from group B). Here p_A and p_B are hidden from us and observed only through actual responses. Thus, having no belief as to the actual values of p_A and p_B, we begin by placing a uniform prior on the two probabilities:

$$\begin{aligned} p_A &\sim \text{Beta}(1,1) \\ p_B &\sim \text{Beta}(1,1) \end{aligned}$$

During our experiment, we accrue α_A responses and β_A non-responses in group A. A standard calculation then yields a posterior of $p_A \sim (\alpha_A + 1, \beta_A + 1)$. Similarly for p_B. Then the probability that is given by: $p_A > p_B$

$$\begin{aligned} &\int_0^1 \int_{p_B}^1 \frac{x^{\alpha_A} y^{\alpha_B} (1-x)^{\beta_A} (1-x)^{\beta_B}}{\mathrm{B}(\alpha_A + 1, \beta_A + 1)\mathrm{B}(\alpha_B + 1, \beta_B + 1)} \mathrm{d}x \, \mathrm{d}y = \\ &\sum_{j=0}^{\alpha_B + 1} \frac{\mathrm{B}(1 + \alpha_A + j, \beta_A + \beta_B + 2)}{(1 + \beta_B + j)B(1 + j, 1 + \beta_B)B(\alpha_A + 1, \beta_A + 1)} \end{aligned} \tag{4}$$

where B is the beta function.

9.3.2 Count Outcome

We employ a Bayesian A/B test analysis that is similar to that for the binary outcomes, but that reflects the nature of the absence count data. As with above, we begin with two groups A and B. Each group has a Poisson parameter λ that reflects the group's count of events adjusting for the group's exposure, with exposure being the count of times group members are at risk of experiencing an event. In the present case, the event is the count of absences and the exposure is the count of how many school days the student could have been absent. We wish to measure whether λ_A– group A's absence count adjusting for exposure–is greater than λ_B–group B's absence count adjusting for exposure. In turn, each group has two parameters that govern $\lambda : \alpha$, the count of events, and β, the exposure that generates risk for the event.

Because we do not directly observe these parameters, we begin by placing an uninformative prior on their distribution and then update that prior based on the data collected in the experiment. After updating the prior, we find the probability that the exposure-adjusted count is greater in group A than in group B via the following equation, where B is the beta function:

$$Pr(\lambda_A > \lambda_B) = \sum_{k=0}^{(\alpha_A - 1)} \frac{(\beta_A + \beta_B)^{-(k+\alpha_B)} \beta_A^k \beta_B^{\alpha_B}}{(k + \alpha_B)B(k + 1, \alpha_B)} \tag{5}$$

9.4 Glossary of Acronyms

Table 7: **Glossary of acronyms used**

Acronym	Stands for	Details
SUSO	Show Up, Stand Out	A program sponsored by OVSJG aimed at working with parents of students who have accumulated 5 unexcused absences to help improve attendance
OVSJG	Office of Victim Services and Justice Grants	Agency whose mission includes funding CBOs to help reduce truancy
CBO	Community-Based Organization	SUSO partners with 7 community organizations to whom it gives grants to work with families on truancy/absenteeism prevention. The CBOs visit the families and ask if they are willing to engage, which means consenting to receipt of supportive services.
OSSE	Office of the State Superintended for Education	The administrative agency in DC that defines and measures absenteeism

9.5 Variables Used to Check Balance and to Control for in Regression

Table 8: **Covariates used to check balance and in regression specifications that are covariate adjusted**

Type of variable	Variable
Blocking Variables	School
Student characteristics	Race
	Gender
	Flag for Limited English Proficiency (LEP)
	Flag for experiencing homelessness
	Free and Reduced Price Lunch category (free; reduced; paid)
	Flag for economically disadvantaged
	Flag for at-risk (students experiencing homelessness, in foster care, receiving Temporary Assistance for Needy Families or Supplemental Nutrition Assistance Program, or is a high-school student who is chronologically older than they should be given their grade).

9.6 Balance Checking and Descriptive Analyses

9.6.1 What are the characteristics of students who are referred to SUSO/end up in our study sample (pre-analysis plan Section H4)?

Figures 18 and 19 contrast three groups of students:

1. Students in our RCT sample (either treatment or control group)

2. Students not in our RCT sample but in SUSO-eligible schools

3. Students not in our RCT sample and not in SUSO-eligible schools

Figure 18 shows their demographic attributes—highlighting, for instance, that the sample's race/ethnicity proportions are similar to that in SUSO-eligible schools (high % African-American). Figure 19 shows different measures of socioeconomic status. Students at SUSO schools who end up in the sample are significantly more likely to qualify for Free or Reduced Price Lunch (FRPL) than students at SUSO schools who do not end up in the sample. Likewise, students who qualify for the "at-risk" category—which can come through FRPL eligibility or more severe challenges like homelessness–are over-represented in the sample relative to students at that sample school.

Figure 18: **Attributes across three groups of students: students in sample and in SUSO-eligible school, students not in sample but in SUSO-eligible school, students neither in sample nor in SUSO-eligible school** The figure shows that students who end up referred to SUSO have various attributes that put them at higher risk for absenteeism. The x axis refers to the proportion of students in the category.

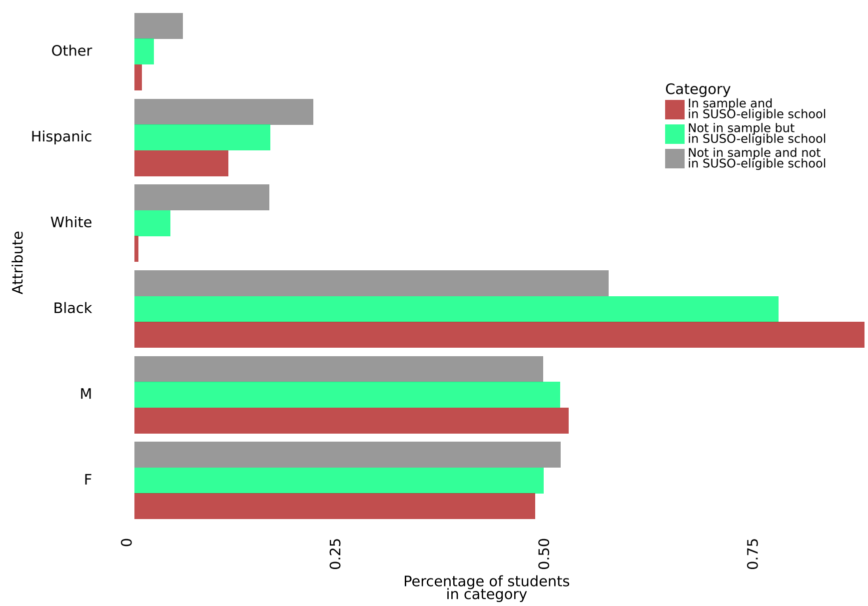
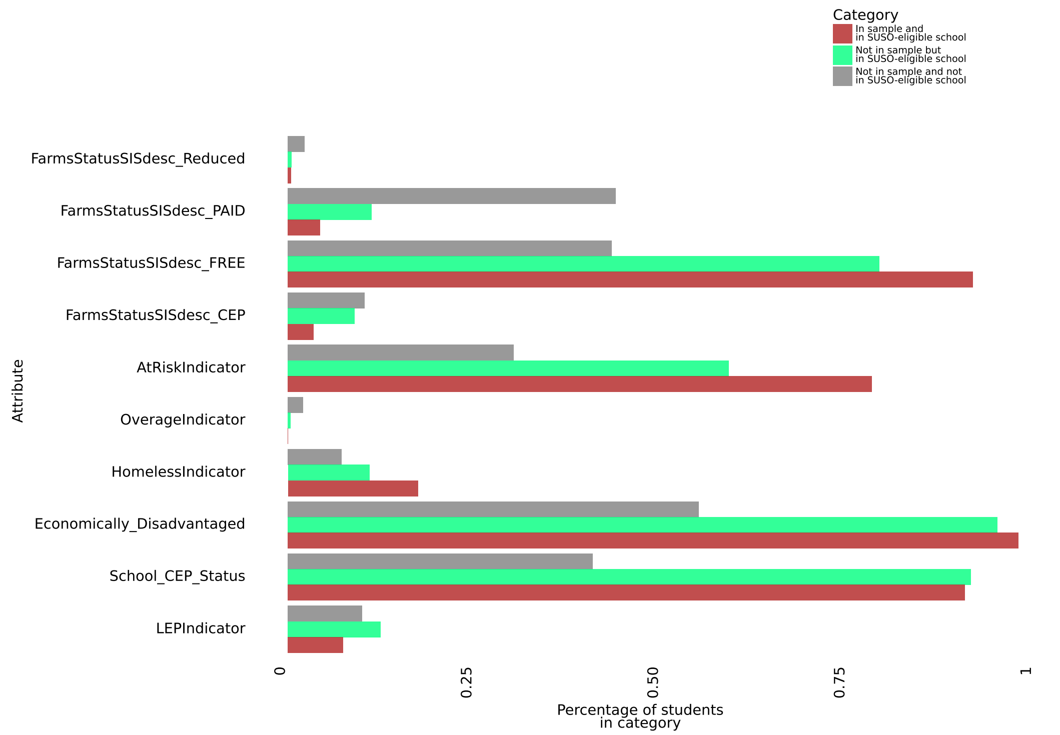


Figure 19: **Attributes across three groups of students: students in sample and in SUSO-eligible school, students not in sample but in SUSO-eligible school, students neither in sample nor in SUSO-eligible school (continued)** The figure shows that students who end up referred to SUSO have various attributes that put them at higher risk for absenteeism. The x axis refers to the proportion of students in the category.



9.6.2 Were our treatment and control groups balanced along various characteristics (pre-analysis plan Section H4)?

The experiment used permuted-block randomization stratified by school. What this means is that, for each school, we drew a random order of treatment/control assignments (for instance: letter; no letter; no letter; letter), and then assigned parents to the respective group as they became eligible. While most are close to a 50–50 split between assigning parents to the treatment group (letter) or control, vagaries of the draw process can result in imbalance.

Figure 20 plots the total count of students randomized versus the difference in control and treatment proportions. As expected, it shows that as the number of students randomized from a school increases, there is closer to a 50-50 balance in the number of students between the treatment group and the control group.

Finally, since imbalance matters most if it results in samples where, for instance, students with risk factors for absenteeism are concentrated heavily in one group, Figure 21 focuses on the demographic attributes highlighted above, and looks at the treatment group proportion minus the control group portion within the attendance analytic sample. The figure, translated into percentage points, shows that the treatment group had, for instance, fewer Black students and fewer students falling into the "at-risk" category, pointing to the need for covariate adjustment.

Figure 20: **Count of treatment minus control group students: relationship with school size**

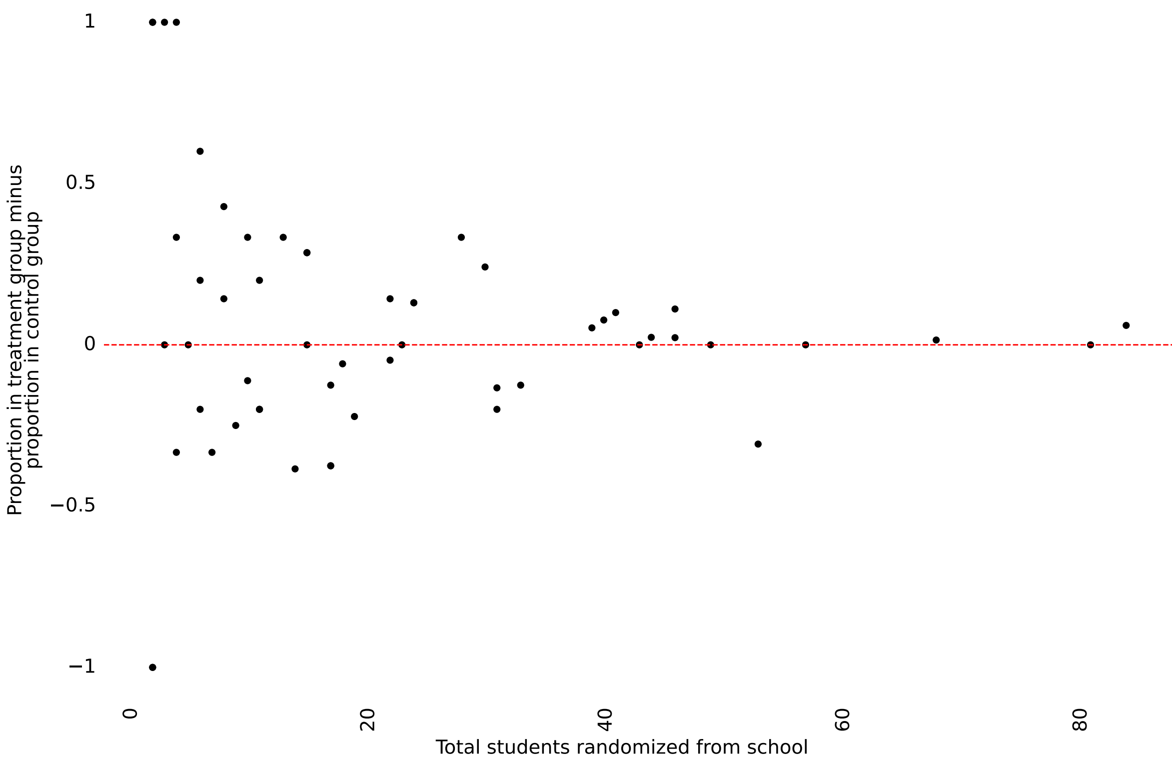
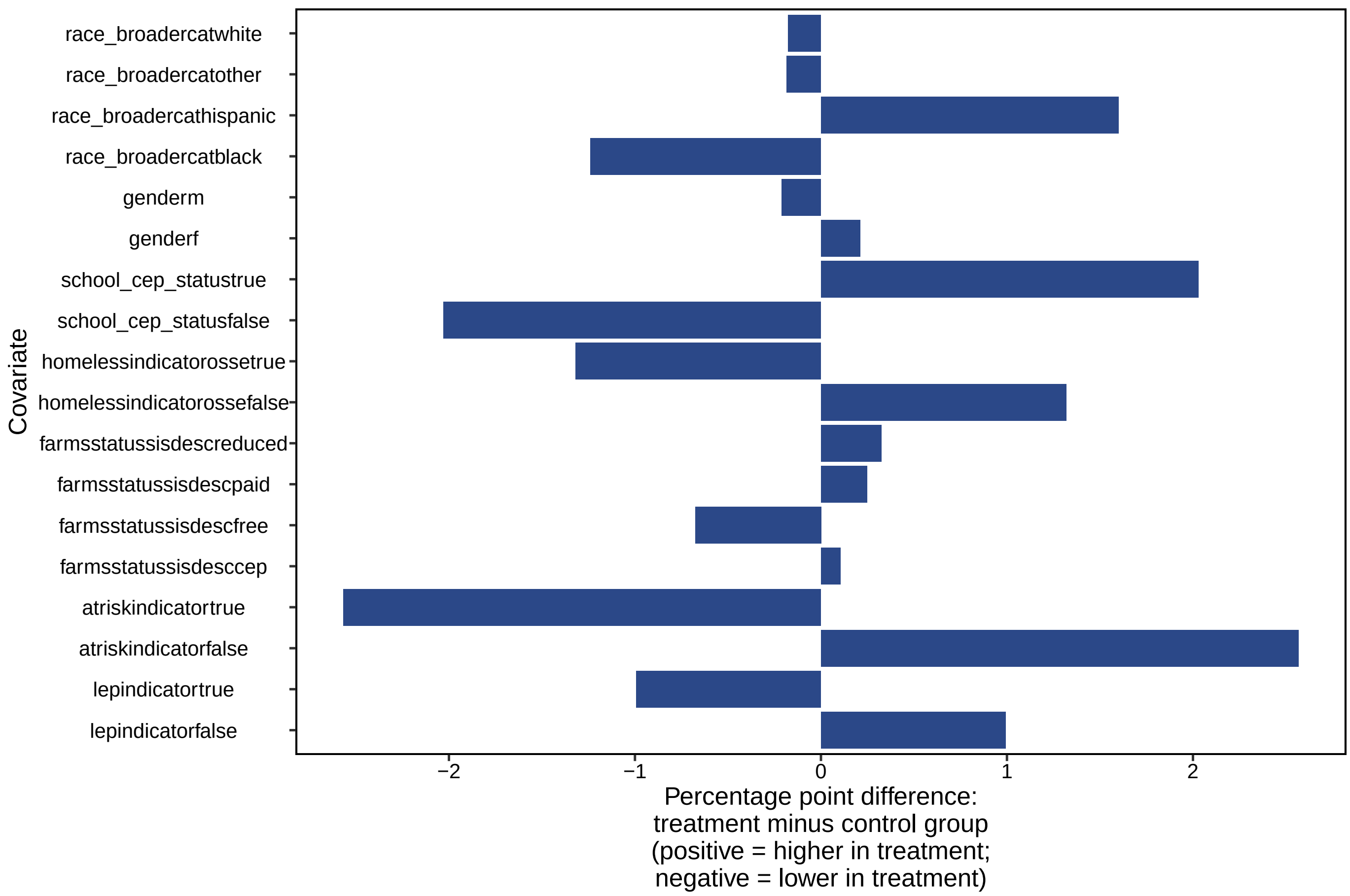


Figure 21: **Balance between treatment and control group students on demographic attributes**



9.6.3 When were students referred to SUSO? (not registered in the pre-analysis plan)

Students are supposed to be referred to SUSO as soon as they reach 5 unexcused absences. However, due to the complexity of the referral process—namely, schools manually looking at lists of students who should be referred and then passing that information along to SUSO–there's considerable variation in compliance with that threshold.

Figure 22 shows that although most students were referred once they reached 5 unexcused absences, there's significant variance around that peak. The students who had 0 unexcused absences at the time of their referral to SUSO might have been cases where a student was referred, and that referral prompted a parent to correct erroneous absence recordings. The final OSSE data would reflect those corrected attendance counts.

Figure 22: **Count of unexcused absences at time of referral**: This provides the count of unexcused absences up until the date the student was referred to SUSO. The absence data is drawn from the OSSE audited attendance data, which can reflect (1) changes that schools make to absences (e.g., marking them as excused after the fact), and (2) any changes as the result of the auditing process. Therefore, it is possible that students with, for instance, fewer than 5 absences at the time of referral were originally recorded as having *more* unexcused absences but then had these unexcused absences changed to excused after the fact.

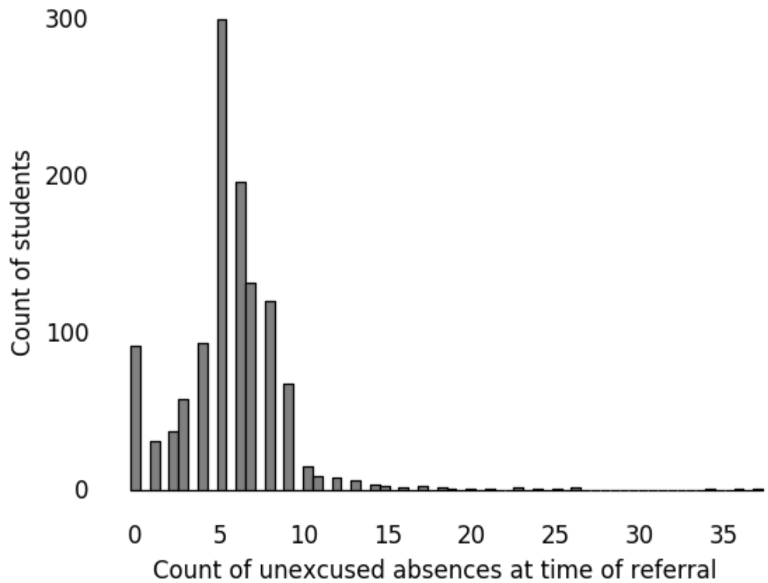


Figure 23 plots the same count of unexcused absences at the time of referral (y-axis) against the referral date. The deviations from the recommended referral at 5 absences are dispersed throughout the year rather than, for instance, only concentrated right at the beginning of the study period (which could indicate that the evaluation prompted schools to catch up and refer students who they should have referred in the fall). Finally, Figure 24 shows the distribution of referral dates, and that more students became eligible for the study by reaching five absences in the earlier months than in the later months.

Figure 23: **Count of unexcused absences at time of referral versus date of referral**

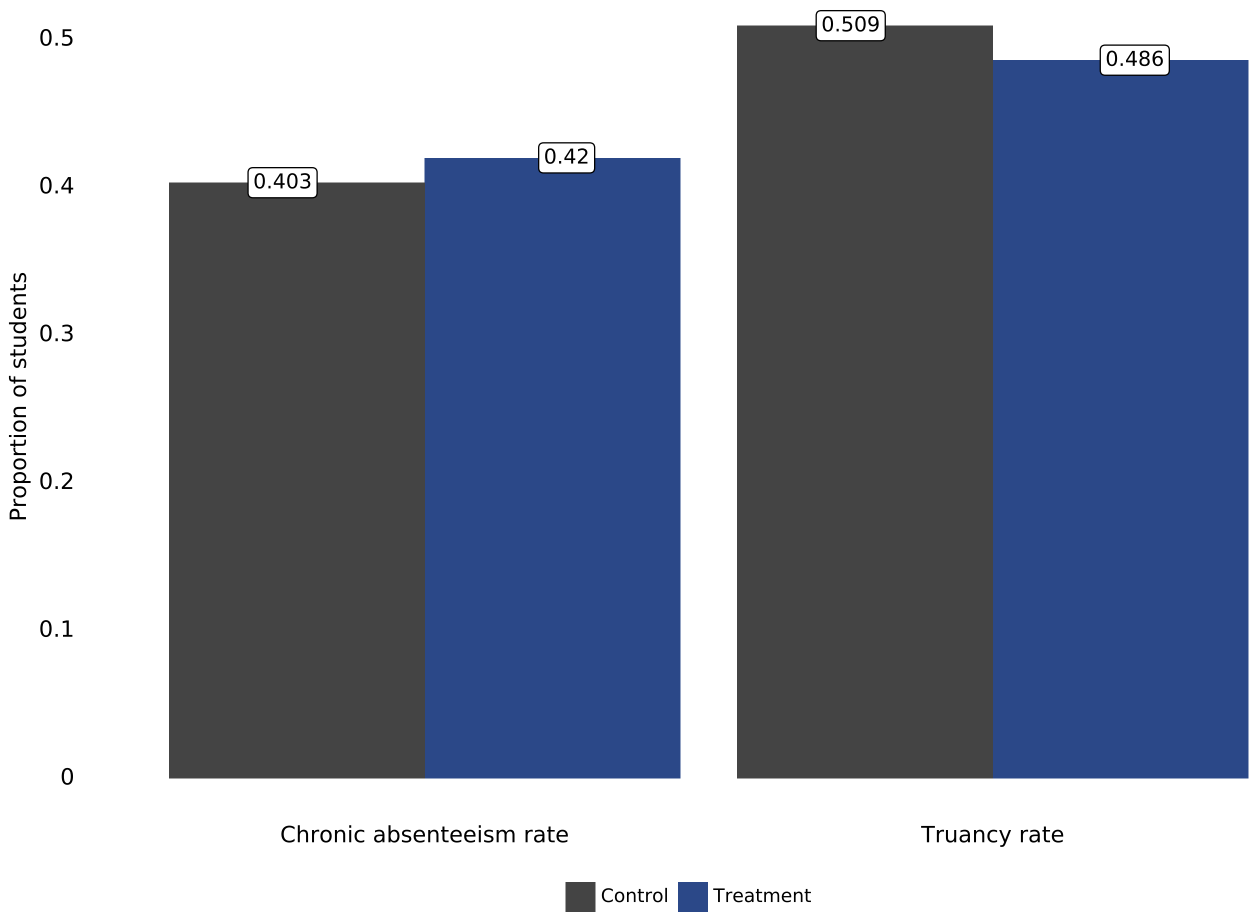
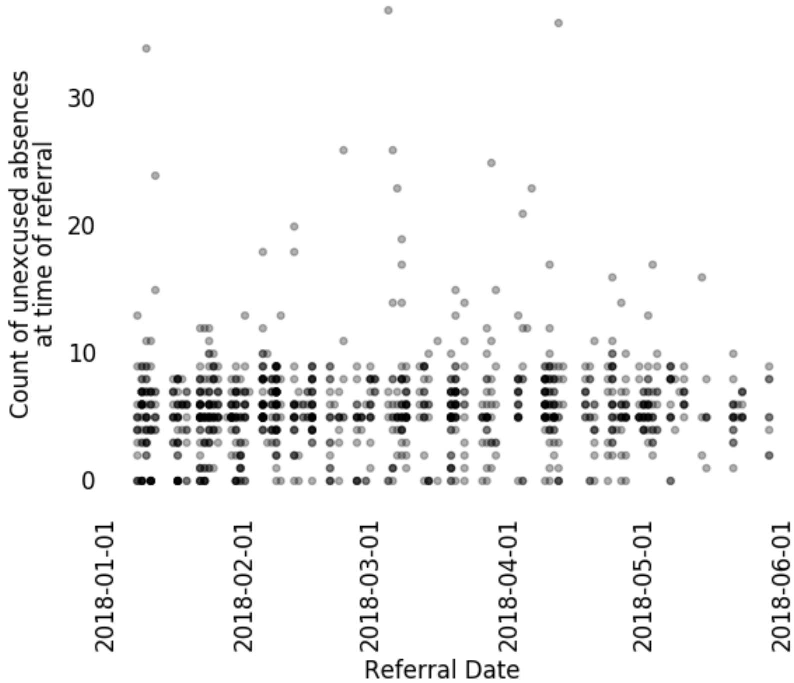
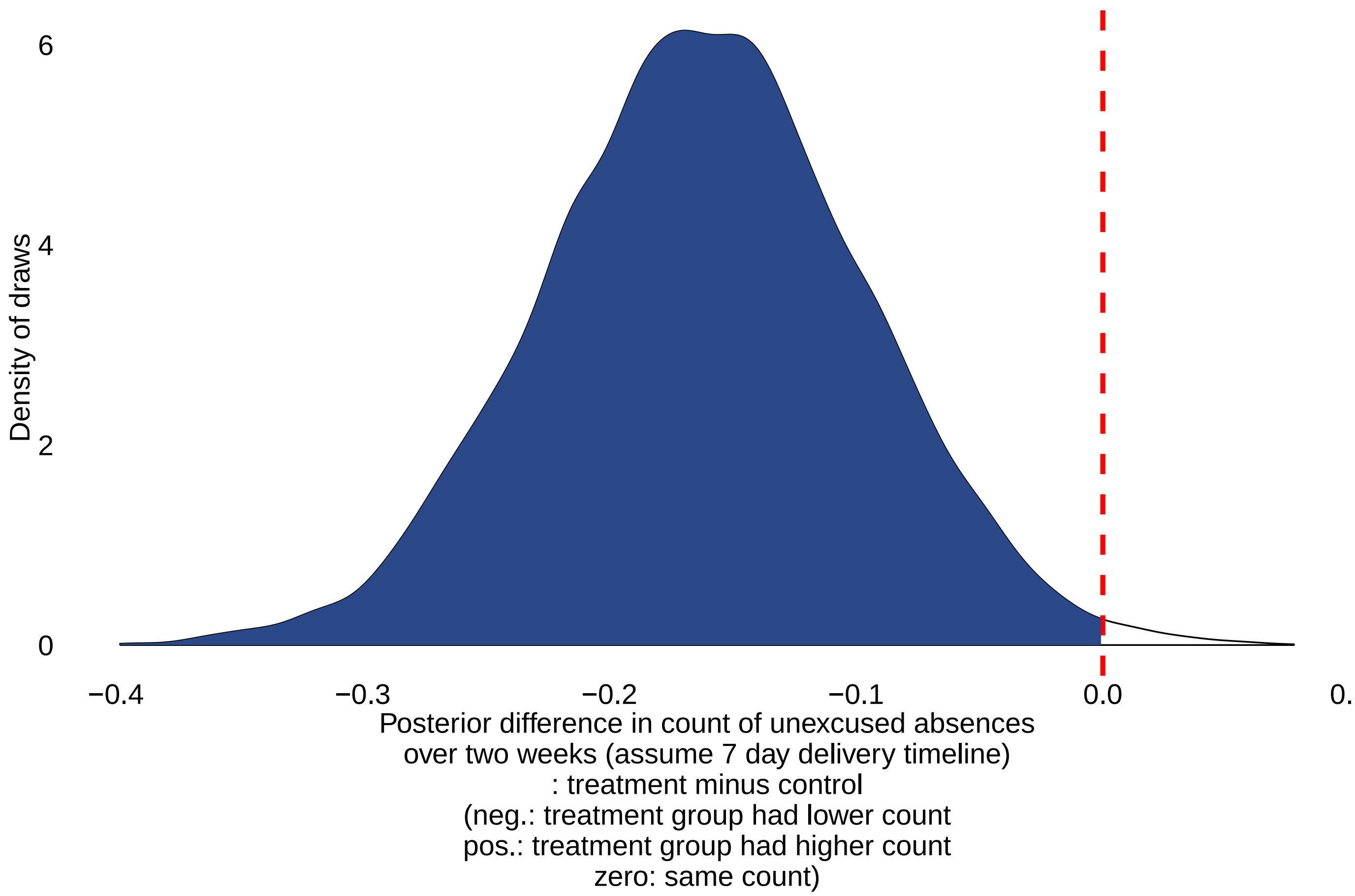


Figure 24: **Distribution of referral dates**

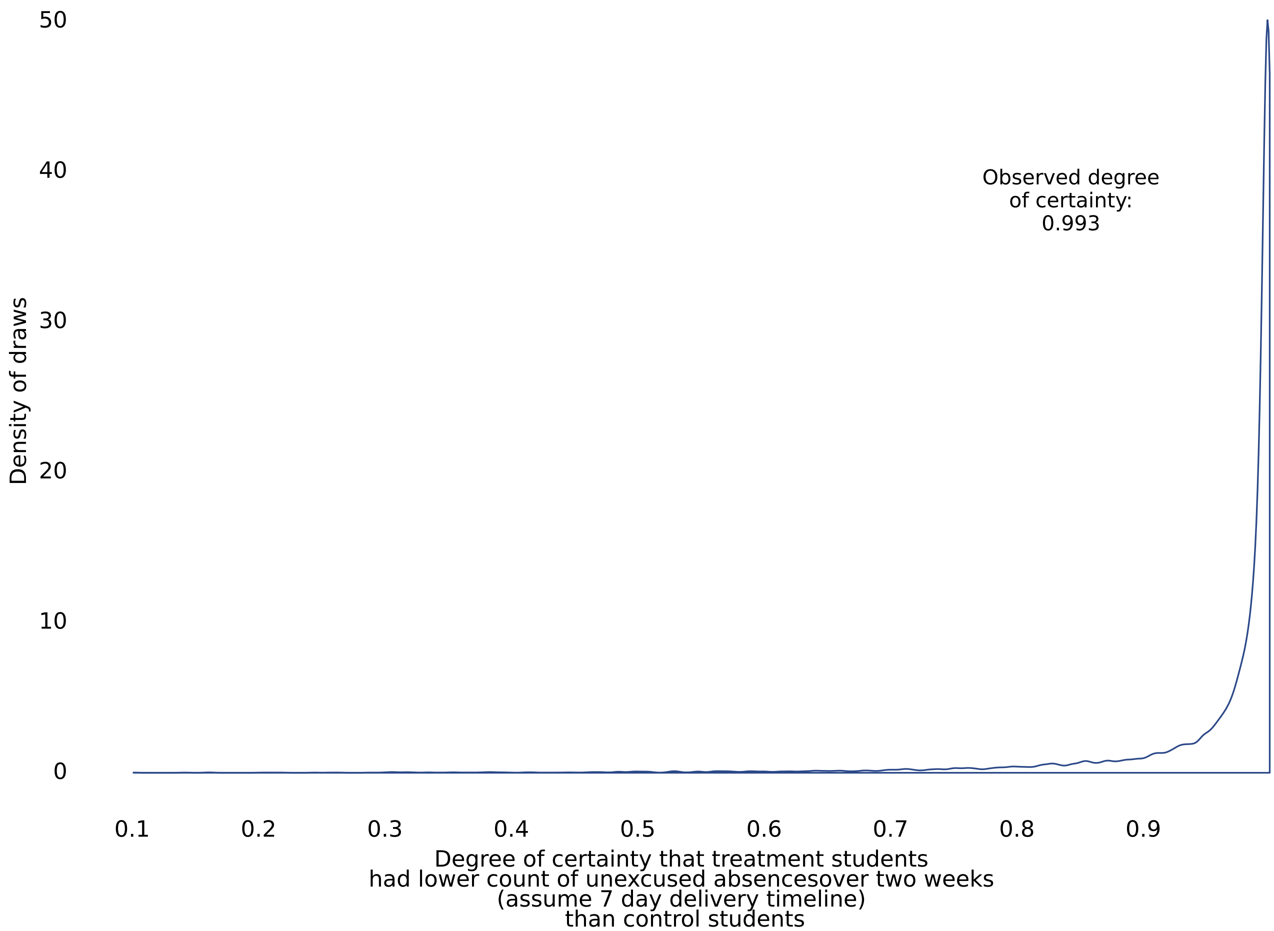


9.7 Robustness checks: starting two-week clock at median letter delivery date (7 days) rather than observed letter delivery date

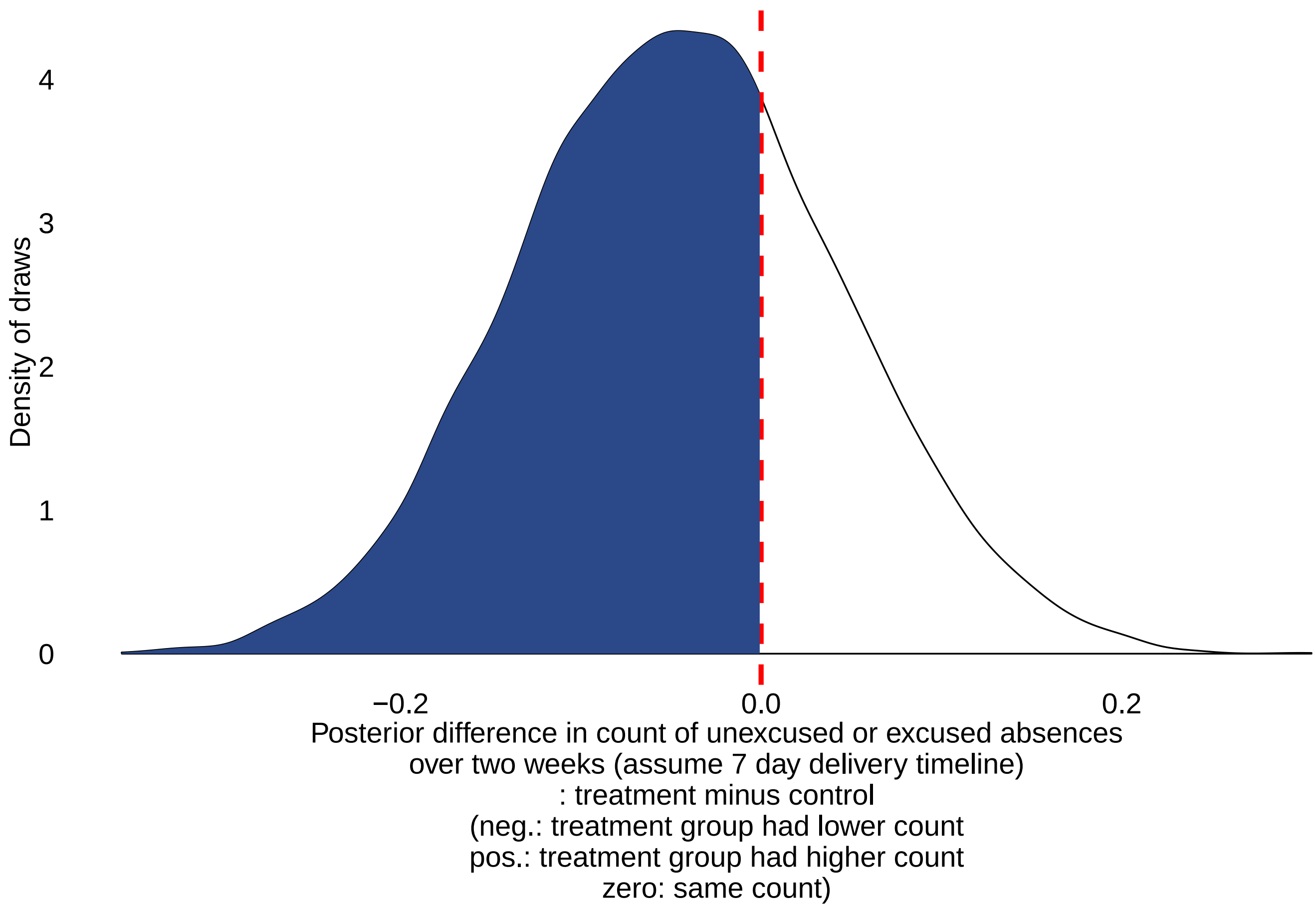
The following figures show that the main results are robust to starting the "clock" for tabulating a student's two-week count of absences at the median date of letter delivery (7 days post SUSO referral) rather than the observed letter delivery date.

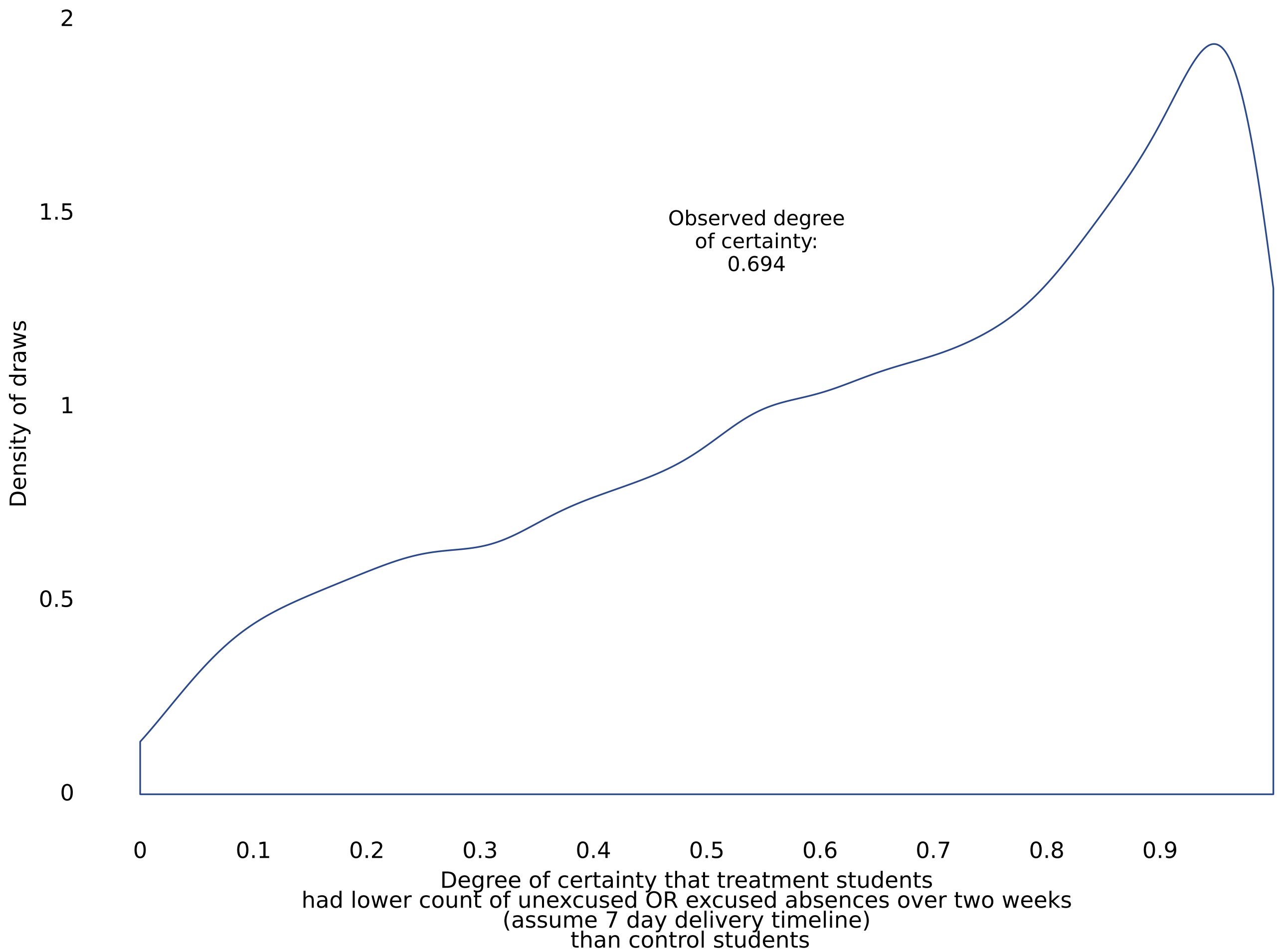


Density of draws
6
4
2
0
−0.4
−0.3
−0.2
−0.1
0.0
Posterior difference in count of unexcused absences
over two weeks (assume 7 day delivery timeline)
: treatment minus control
(neg.: treatment group had lower count
pos.: treatment group had higher count
zero: same count)



Observed degree
of certainty:
0.993
Density of draws
0
10
20
30
40
50
0.1
0.2
0.3
0.4
0.5
0.6
0.7
0.8
0.9
Degree of certainty that treatment students
had lower count of unexcused absencesover two weeks
(assume 7 day delivery timeline)
than control students


Density of draws
4
3
2
1
0
−0.2
0.0
0.2
Posterior difference in count of unexcused or excused absences
over two weeks (assume 7 day delivery timeline)
: treatment minus control
(neg.: treatment group had lower count
pos.: treatment group had higher count
zero: same count)


Density of draws
2
1.5
1
0.5
0
Observed degree
of certainty:
0.694
0
0.1
0.2
0.3
0.4
0.5
0.6
0.7
0.8
0.9
Degree of certainty that treatment students
had lower count of unexcused OR excused absences over two weeks
(assume 7 day delivery timeline)
than control students

9.8 Robustness checks: regression results to supplement A/B testing

The Bayesian A/B test results yield two main findings. First, in the two calendar weeks following letter delivery, the treatment appeared to significantly reduce unexcused absences but had no impact on total absences. Second, the effect on the short-term unexcused absences was stronger than that on the long-term outcomes–in particular, while the letter temporarily slowed down students' accrual of unexcused absences, it potentially had no effect on year-end truancy (a student staying under 10 unexcused absences).

The main issue that could undermine these results is that despite randomization where we randomized treatments within each school, the small size of the sample ($\sim$ 1200 students) means that the treatment group and control group may still differ on observed (and potentially unobserved) characteristics. Appendix Section 9.6.2 shows this imbalance – in particular, the treatment group had fewer at-risk students, which is a designation used to reflect students who are experiencing homelessness, are in foster care, or whose families are receiving cash welfare or SNAP benefits, and fewer Black students. Bayesian A/B testing does not allow us to adjust for these pre-treatment attributes that are imbalanced between the treatment and control group. With regression analysis, we can adjust for these pre-treatment attributes. The main text presented the results for the primary outcome of short-term unexcused absences. This section shows the remainder of the regression results. These results, in line with those from the A/B tests, show (1) no effect on total absences in the short-term; (2) a much weaker effect on end-of-year truancy and chronic absenteeism.

Table 9: Logistic regression: does the letter decrease year-end truancy?

	Dependent variable: logistic regression (binary truancy)		
Treatment (1 = yes)	−0.093	−0.073	−0.054
	(0.116)	(0.118)	(0.129)
	p = 0.420	p = 0.539	p = 0.673
Hispanic (ref: Black)		−0.300	−0.537
		(0.282)	(0.342)
		p = 0.288	p = 0.117
White (ref: Black)		−14.330	−16.580
		(386.800)	(971.500)
		p = 0.971	p = 0.987
Other race/ eth (ref: Black)		0.314	−0.225
		(0.624)	(0.708)
		p = 0.615	p = 0.751
Female (ref: male)		−0.212	−0.211
		(0.119)	(0.129)
		p = 0.074*	p = 0.104
Limited English prof. (1 = yes)		−0.111	−0.265
		(0.343)	(0.394)
		p = 0.746	p = 0.501
Homeless (1 = yes)		0.212	0.272
		(0.168)	(0.187)
		p = 0.207	p = 0.147
Free lunch (ref: CEP)		0.607	1.143
		(0.346)	(1.606)
		p = 0.080*	p = 0.477
Paid lunch (ref: CEP)		−0.348	0.349
		(0.545)	(0.709)
		p = 0.523	p = 0.622
Reduced lunch (ref: CEP)		1.725	2.639
		(0.947)	(1.806)
		p = 0.069*	p = 0.144
At-risk (1 = yes)		0.665	0.824
		(0.159)	(0.175)
		p = 0.00003***	p = 0.00001***
Economically disadvantaged (1 = yes)		−1.055	−0.892
		(0.626)	(1.486)
		p = 0.092*	p = 0.549
Constant	0.037	0.099	−0.725
	(0.082)	(0.702)	(1.210)
	p = 0.652	p = 0.888	p = 0.549
Observations	1,198	1,198	1,198
Log Likelihood	−830.000	−805.800	−723.000
Akaike Inf. Crit.	1,664.000	1,638.000	1,598.000
School Fixed Effects?	No	No	Yes
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 10: Logistic regression: does the letter decrease year-end chronic absenteeism?

	Dependent variable: binary chronic absenteeism		
Treatment (1 = yes)	0.069	0.122	0.127
	(0.117)	(0.122)	(0.131)
	p = 0.556	p = 0.318	p = 0.332
Hispanic (ref: Black)		−0.849	−1.106
		(0.321)	(0.361)
		p = 0.009***	p = 0.003***
White (ref: Black)		−1.251	−1.361
		(1.158)	(1.198)
		p = 0.281	p = 0.256
Other race/ eth (ref: Black)		−1.130	−1.475
		(0.798)	(0.840)
		p = 0.158	p = 0.080*
Female (ref: male)		−0.221	−0.242
		(0.122)	(0.132)
		p = 0.070*	p = 0.067*
Limited English prof. (1 = yes)		0.236	0.249
		(0.386)	(0.418)
		p = 0.541	p = 0.552
Homeless (1 = yes)		0.622	0.675
		(0.168)	(0.186)
		p = 0.0003***	p = 0.0003***
Free lunch (ref: CEP)		0.392	−0.937
		(0.353)	(1.289)
		p = 0.267	p = 0.467
Paid lunch (ref: CEP)		0.601	0.501
		(0.533)	(0.786)
		p = 0.260	p = 0.524
Reduced lunch (ref: CEP)		−0.389	−1.278
		(1.200)	(1.643)
		p = 0.746	p = 0.437
At-risk (1 = yes)		0.901	0.970
		(0.177)	(0.192)
		p = 0.00000***	p = 0.00000***
Economically disadvantaged (1 = yes)		−0.762	0.500
		(0.611)	(1.044)
		p = 0.213	p = 0.633
Constant	−0.386	−0.687	−0.323
	(0.084)	(0.688)	(1.230)
	p = 0.00001***	p = 0.318	p = 0.794
Observations	1,198	1,198	1,198
Log Likelihood	−812.100	−771.300	−708.400
Akaike Inf. Crit.	1,628.000	1,569.000	1,569.000
School Fixed Effects?	No	No	Yes
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 11: Linear regression results: does the letter decrease unexcused absences in the two-weeks following letter delivery? (median delivery date)

	Dependent variable: unexcused absence counts (median delivery date)		
Treatment (1 = yes)	−0.101	−0.095	−0.106
	(0.050)	(0.049)	(0.049)
	p = 0.041**	p = 0.055*	p = 0.032**
Hispanic (ref: Black)		−0.032	−0.034
		(0.116)	(0.122)
		p = 0.782	p = 0.780
White (ref: Black)		−0.378	−0.432
		(0.394)	(0.389)
		p = 0.338	p = 0.268
Other race/ eth (ref: Black)		−0.227	−0.244
		(0.258)	(0.261)
		p = 0.380	p = 0.351
Female (ref: male)		0.005	0.005
		(0.049)	(0.049)
		p = 0.915	p = 0.912
Limited English prof. (1 = yes)		−0.235	−0.342
		(0.139)	(0.142)
		p = 0.092*	p = 0.017**
Homeless (1 = yes)		0.203	0.187
		(0.070)	(0.072)
		p = 0.004***	p = 0.010***
Free lunch (ref: CEP)		0.083	0.091
		(0.143)	(0.447)
		p = 0.564	p = 0.839
Paid lunch (ref: CEP)		−0.053	0.149
		(0.217)	(0.286)
		p = 0.80	p = 0.603
Reduced lunch (ref: CEP)		−0.288	−0.053
		(0.376)	(0.524)
		p = 0.444	p = 0.920
At-risk (1 = yes)		0.171	0.217
		(0.065)	(0.065)
		p = 0.009***	p = 0.001***
Economically disadvantaged (1 = yes)		−0.156	−0.157
		(0.247)	(0.357)
		p = 0.528	p = 0.661
Constant	0.512	0.447	−0.035
	(0.035)	(0.280)	(0.470)
	p = 0.000***	p = 0.111	p = 0.941
Observations	1,189	1,189	1,189
R^2	0.004	0.034	0.137
Adjusted R^2	0.003	0.025	0.079
Residual Std. Error	0.854 (df = 1187)	0.845 (df = 1176)	0.821 (df = 1113)
F Statistic	4.191** (df = 1; 1187)	3.502*** (df = 12; 1176)	2.350*** (df = 75; 1113)
School Fixed Effects?	No	No	Yes
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 12: Negative binomial regression results: does the letter decrease unexcused absences in the two-weeks following letter delivery? (median delivery date)

	Dependent variable: unexcused absence counts (median delivery date)		
Treatment (1 = yes)	−0.209	−0.189	−0.231
	(0.106)	(0.105)	(0.102)
	p = 0.050**	p = 0.071*	p = 0.024**
Hispanic (ref: Black)		−0.106	−0.045
		(0.265)	(0.282)
		p = 0.689	p = 0.874
White (ref: Black)		−23.110	−29.280
		(49,555.000)	(1,003,600.000)
		p = 1.000	p = 1.000
Other race/ eth (ref: Black)		−0.810	−0.796
		(0.772)	(0.769)
		p = 0.294	p = 0.301
Female (ref: male)		0.0005	0.001
		(0.105)	(0.102)
		p = 0.997	p = 0.993
Limited English prof. (1 = yes)		−0.846	−1.032
		(0.368)	(0.376)
		p = 0.022**	p = 0.007***
Homeless (1 = yes)		0.328	0.325
		(0.133)	(0.135)
		p = 0.014**	p = 0.017**
Free lunch (ref: CEP)		−0.113	0.027
		(0.290)	(1.413)
		p = 0.697	p = 0.985
Paid lunch (ref: CEP)		−0.301	0.457
		(0.463)	(0.533)
		p = 0.516	p = 0.391
Reduced lunch (ref: CEP)		−23.400	−28.480
		(45,735.000)	(919,022.000)
		p = 1.000	p = 1.000
At-risk (1 = yes)		0.512	0.612
		(0.160)	(0.158)
		p = 0.002***	p = 0.0002***
Economically disadvantaged (1 = yes)		−0.198	−0.281
		(0.589)	(1.299)
		p = 0.737	p = 0.829
Constant	−2.797	−2.919	−4.635
	(0.073)	(0.640)	(1.262)
	p = 0.000***	p = 0.00001***	p = 0.0003***
Observations	1,188	1,188	1,188
Log Likelihood	−1,085.000	−1,059.000	−998.000
θ	0.865*** (0.135)	1.052*** (0.179)	1.833*** (0.420)
Akaike Inf. Crit.	2,174.000	2,143.000	2,146.000
School Fixed Effects?	No	No	Yes
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 13: Linear regression results: does the letter decrease the combined count of excused and unexcused absences in the two-weeks following letter delivery? (observed delivery date)

	Dependent variable:		
	diff_excusedorunexcused_observeddelivery		
	(1)	(2)	(3)
Treatment (1 = yes)	−0.012	0.005	−0.009
	(0.073)	(0.072)	(0.072)
	p = 0.867	p = 0.947	p = 0.899
Hispanic (ref: Black)		−0.264	−0.243
		(0.170)	(0.180)
		p = 0.121	p = 0.177
White (ref: Black)		−0.660	−0.605
		(0.576)	(0.574)
		p = 0.252	p = 0.293
Other race/ eth (ref: Black)		−0.669	−0.730
		(0.378)	(0.385)
		p = 0.077*	p = 0.059*
Female (ref: male)		−0.063	−0.047
		(0.072)	(0.073)
		p = 0.381	p = 0.515
Limited English prof. (1 = yes)		−0.173	−0.349
		(0.203)	(0.210)
		p = 0.396	p = 0.096*
Homeless (1 = yes)		0.263	0.234
		(0.102)	(0.106)
		p = 0.011**	p = 0.029**
Free lunch (ref: CEP)		0.268	0.213
		(0.209)	(0.659)
		p = 0.200	p = 0.747
Paid lunch (ref: CEP)		−0.091	0.069
		(0.317)	(0.423)
		p = 0.774	p = 0.870
Reduced lunch (ref: CEP)		−0.470	−0.371
		(0.549)	(0.773)
		p = 0.393	p = 0.632
At-risk (1 = yes)		0.279	0.320
		(0.095)	(0.096)
		p = 0.004***	p = 0.001***
Economically disadvantaged (1 = yes)		−0.509	−0.343
		(0.360)	(0.527)
		p = 0.159	p = 0.516
Constant	0.887	0.961	0.487
	(0.052)	(0.409)	(0.694)
	p = 0.000***	p = 0.019**	p = 0.483
Observations	1,188	1,188	1,188
R^2	0.00002	0.039	0.123
Adjusted R^2	−0.001	0.029	0.064
Residual Std. Error	1.254 (df = 1186)	1.235 (df = 1175)	1.213 (df = 1113)
F Statistic	0.028 (df = 1; 1186)	3.989*** (df = 12; 1175)	2.102*** (df = 74; 1113)
School Fixed Effects?	No	No	Yes
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 14: Linear regression results: does the letter decrease the combined count of excused and unexcused absences in the two-weeks following letter delivery? (median delivery date)

	Dependent variable:		
	diff_excusedorunexcused_mediandelivery		
	(1)	(2)	(3)
Treatment (1 = yes)	−0.036	−0.018	−0.044
	(0.071)	(0.070)	(0.070)
	p = 0.612	p = 0.800	p = 0.526
Hispanic (ref: Black)		−0.333	−0.333
		(0.164)	(0.174)
		p = 0.043**	p = 0.056*
White (ref: Black)		−0.970	−0.873
		(0.557)	(0.554)
		p = 0.083*	p = 0.116
Other race/ eth (ref: Black)		−0.680	−0.756
		(0.366)	(0.372)
		p = 0.064*	p = 0.043**
Female (ref: male)		−0.023	−0.002
		(0.070)	(0.070)
		p = 0.740	p = 0.978
Limited English prof. (1 = yes)		−0.067	−0.160
		(0.197)	(0.202)
		p = 0.735	p = 0.430
Homeless (1 = yes)		0.326	0.289
		(0.099)	(0.103)
		p = 0.002***	p = 0.005***
Free lunch (ref: CEP)		0.169	0.062
		(0.202)	(0.636)
		p = 0.402	p = 0.923
Paid lunch (ref: CEP)		−0.040	0.216
		(0.307)	(0.408)
		p = 0.897	p = 0.597
Reduced lunch (ref: CEP)		−0.539	−0.326
		(0.532)	(0.746)
		p = 0.311	p = 0.663
At-risk (1 = yes)		0.256	0.310
		(0.092)	(0.093)
		p = 0.006***	p = 0.001***
Economically disadvantaged (1 = yes)		−0.530	−0.365
		(0.349)	(0.509)
		p = 0.130	p = 0.473
Constant	0.878	1.051	0.496
	(0.050)	(0.396)	(0.669)
	p = 0.000***	p = 0.008***	p = 0.459
Observations	1,189	1,189	1,189
R^2	0.0002	0.042	0.132
Adjusted R^2	−0.001	0.032	0.074
Residual Std. Error	1.216 (df = 1187)	1.196 (df = 1176)	1.170 (df = 1113)
F Statistic	0.258 (df = 1; 1187)	4.295*** (df = 12; 1176)	2.260*** (df = 75; 1113)
School Fixed Effects?	No	No	Yes
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 15: Negative binomial regression results: does the letter decrease the combined count of excused and unexcused absences in the two-weeks following letter delivery? (observed delivery date)

	Dependent variable:		
	diff_excusedorunexcused_observeddelivery		
	(1)	(2)	(3)
Treatment (1 = yes)	0.019 (0.082) p = 0.820	0.045 (0.081) p = 0.574	0.033 (0.079) p = 0.674
Hispanic (ref: Black)		−0.420 (0.216) p = 0.052*	−0.371 (0.225) p = 0.100*
White (ref: Black)		−1.413 (1.095) p = 0.198	−1.296 (1.079) p = 0.230
Other race/ eth (ref: Black)		−1.544 (0.756) p = 0.041**	−1.593 (0.758) p = 0.036**
Female (ref: male)		−0.081 (0.081) p = 0.316	−0.072 (0.080) p = 0.365
Limited English prof. (1 = yes)		−0.304 (0.273) p = 0.266	−0.479 (0.279) p = 0.087*
Homeless (1 = yes)		0.226 (0.107) p = 0.034**	0.180 (0.108) p = 0.098*
Free lunch (ref: CEP)		0.113 (0.237) p = 0.635	0.509 (0.917) p = 0.580
Paid lunch (ref: CEP)		−0.265 (0.382) p = 0.487	0.304 (0.487) p = 0.532
Reduced lunch (ref: CEP)		−18.790 (3,660.000) p = 0.996	−27.080 (326,873.000) p = 1.000
At-risk (1 = yes)		0.401 (0.117) p = 0.001***	0.447 (0.116) p = 0.0002***
Economically disadvantaged (1 = yes)		−0.497 (0.446) p = 0.266	−0.427 (0.783) p = 0.587
Constant	−2.270 (0.058) p = 0.000***	−2.154 (0.494) p = 0.00002***	−3.184 (0.798) p = 0.0001***
Observations	1,188	1,188	1,188
Log Likelihood	−1,543.000	−1,511.000	−1,458.000
θ	1.165*** (0.135)	1.371*** (0.172)	1.907*** (0.285)
Akaike Inf. Crit.	3,090.000	3,048.000	3,065.000
School Fixed Effects?	No	No	Yes
Note:			*p<0.1; **p<0.05; ***p<0.01

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