

Attention as a First-Order Barrier to Adoption of Evidence-based Programs*

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Abstract

Research on organizations' decisions to adopt evidence-based programs exclusively draws upon self-selected samples. Consequently, research may have overlooked any first-order barriers to program adoption. Using four embedded natural field experiments in a random sample of all public schools in Denmark ($N=368$), we find that capturing the attention of decision-makers increases the adoption rate of an evidence-based book-reading program from 20% to 70%. We replicate the result in a second random sample. Once decision-makers pay attention, their primary interest is the immediate transaction costs, and not—as usually expected—evidence about the long-term benefits of the program.

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The credibility revolution has significantly improved the quality of evidence available to decision-makers across the globe (Angrist and Pischke 2010; Baron 2018). As a result, a critical challenge for social scientists is identifying strategies to promote the adoption of evidence-based policies, especially in light of the considerable scalability problems caused by non-adoption (Al-Ubaydli, List, and Suskind 2017; List 2022). Without scalability, elevating the standard of proof will have no significant effect on the operations of contemporary societies, thereby negating the relevance of decades of scientific advancement.

We argue that a hitherto overlooked first-order barrier to the adoption of evidence-based programs is lack of attention. In an experiment involving a random sample of all public schools in Denmark we were able to raise our baseline level of adoption of an evidence-based shared book reading program, READ (Andersen and Nielsen 2016) from 20%—which is close to the adoption rate in a previous study of the same program (Andersen and Hvidman 2024)—to 70% by gaining the decision-makers’ attention. After overcoming the attention barrier, our findings indicate that transaction costs (i.e. the immediate costs of adopting the program¹) are more important to the decision-makers than both the benefits of the program—which are often the focus when advertising evidence-based programs—and the running costs, which are often the focus of cost-benefit analyses.

Following the model for nonresponse in surveys developed by Dutz et al. (2021), we consider two types of nonparticipation in evidence-based programs: active nonparticipation is caused by weak incentives (individual costs exceeds the benefits), whereas passive non-participation is caused by individuals’ lack of awareness about the program. Even though evidence-based programs are defined by having documented benefits exceeding the costs, much of the existing research on evidence adoption focuses on whether decision-makers react adequately to the evidence documenting these benefits. In contrast, we find that the other

1. We label the immediate costs of adopting the program as "transaction costs." While traditionally linked to exchanges of goods and services, the term also covers coordination, communication, and decision-making costs—uses found in earlier economic literature as well (Atkin et al. 2017).

dimension—the passive nonparticipation caused by lack of attention—is a first-order barrier for adoption.

A major constraint in our understanding of the factors influencing adoption is caused by self-selection. A review of studies on adoption of evidence-based policies identified only 11 relevant studies (DellaVigna, Kim, and Linos 2024), and all of these studies are based on some sort of self-selected samples. For instance, in a field experiment of Brazilian municipalities, the sample consists of the 45% of all mayors who had chosen to participate in a conference (Hjort et al. 2021). The sample is further selected when experimental estimates are calculated for the 38% of leaders in the treatment group attending an information session (see also Vivalt and Coville 2023). Similarly, a study on the adoption of nudges is based on 30 US cities that had previously decided to participate in randomized trials (DellaVigna, Kim, and Linos 2024).² These sample selection implies that we have little knowledge about the factors influencing evidence adoption among the majority of organizations that do not self-select into studies. As a result, existing research fails to address first-order barriers to policy adoption.

In this study, we first invited 368 schools by email to adopt a new version of READ, called READit, which involved asking parents to sign up for the program, receive and distribute a bag containing books and information material for parents, and asking teachers to support parents in shared book reading at home. Thanks to a generous donation from a non-profit organization, neither schools nor parents were required to pay for participating, increasing the likelihood that the documented benefits would exceed the running costs. In Step 1, we randomly assigned invited schools to one of two versions of an email emphasizing either the student benefits or the transaction costs of adopting the program. Approximately 20 % of the schools became early adopters by confirming their participation in response to the first email. The difference between the two emails was 1.3 percentage points, but this was not

2. A notable exception is recent study by Garcia-Hombrados et al. (2024), which offers evidence-based policy information to a large sample of municipalities in Spain (with no selection into study). We return to a discussion of this study.

statistically significant.

In Step 2, all schools that had not yet responded to the invitation were randomly assigned to one of two groups: one group received a reminder email, while the other received a physical bag containing books, which would be given to parents if they chose to participate in the program. The email-group increased adoption by 13.5 percentage points, but the physical copy increased adoption by 25.4 percentage points, thereby outperforming the email-group by 11.9 percentage points (an 88% increase). In Step 3 we started calling schools that had not yet responded in a randomized wait-list design. This increased adoption rate by 36.3 percentage points in the email group and 24.5 percentage points in the physical copy group yielding a total adoption rate of about 70% in both groups. In the end, we had about 20% *early adopters*, and 50% *late adopters*. In the early adopter group, 4,242 children received the book bag. We thus managed to reach 10,456 additional children at late-adopter schools, who received the bags thanks to the reminders. 30% of the schools never adopted the program (a group we label *non-adopters*).

How can we interpret these results? Was the late or non-adoption of the program driven by active nonparticipation, potentially caused by lower anticipated benefits compared to those perceived by early adopters (cf. the notion of “randomization bias”; Heckman 1992, 2020)? We exploit unique administrative data on all schools—a feature that allows for inference, even among non-adopters—and find that the expected benefits of READ were virtually the same for all groups of schools, based on the previously published evidence on subgroup effects (showing higher effects for students of immigrant parents and mothers with low education; Andersen and Nielsen 2016).

We also see that late adopters were at least as effective in implementing the program (making parents sign up and use the materials) as early adopters. Furthermore, among the 30% non-adopters, 45% (i.e., 14% of the full sample) could not be reached by phone, so they may never have noticed the invitation. 35% of non-adopters (11% of the full sample)

were reached by phone, but did not make an active decision, and only 20% (6% of the full sample) actively declined the invitation. These results support the interpretation that the difference between early, late and non-adopters are primarily driven by lack of attention to the invitation, with only 6 % actively declining it. Some numbers suggest that the average adult receives over 100 emails each day, and getting the attention of busy readers is therefore an essential prerequisite when communicating in writing (Rogers and Lasky-Fink 2023). Since the decision-maker has a limited attention span, attention may be a first-order barrier to the adoption of evidence-based programs.

We replicate these results in a second random sample of another 442 schools. The replication study allows us to evaluate whether the large effects in the primary study might be a result of chance. We also simplified the invitation procedure in the replication study to avoid potential interaction effects between the treatments in step 1-3 in the primary study. In the replication study we randomly allocated schools to a business-as-usual and an invitation reminder group. We invited both groups of schools by the same email. After 28 days we started calling schools in the reminder group that had not yet responded by phone. The adoption rate in the business-as-usual group was 26% compared to 56% in the reminder treatment group. Even with priors as low as 1% for the expectation that capturing decision-makers' attention more than doubles the adoption rate, the replication study increases the post-study probability (PSP) from 9% after the primary study to 74% after the replication study (with priors of 10%, the replication study raises PSP from 50% to 97%) (Moonesinghe, Khoury, and Janssens 2007; Maniadis, Tufano, and List 2014).

Once decision-makers considered adopting the program we directed them to sign-up at a web-site containing information organized in five boxes. We leveraged our natural field experimental setup (Harrison and List 2004) by randomizing the order of the boxes at the web-site. The information about transaction costs of implementing the program was decision-makers' first choice more often than any of the other boxes. We define transaction costs as the costs

involved in adopting and implementing the program (up until the time when it is running). Even if there are different possible explanations of this result (which we discuss later), we see this as an indication that once the attention barrier is overcome, transaction costs are of primary importance to decision-makers. Transaction costs involve making employees change their existing behavior and routines. This may be a barrier if, for instance, the transaction costs are borne by employees whereas benefits accrue to the managers or owners (Atkin et al. 2017).

Our study relates to several literatures. First, it relates most directly to the literature on evidence adoption. In the discussion section, we revisit previous studies on evidence adoption (including Atkin et al. 2017; Bloom et al. 2013; Bloom et al. 2020; DellaVigna, Kim, and Linos 2024; Garcia-Hombrados et al. 2024) and argue that our findings are consistent with these earlier studies as they also point to transaction costs and attention as barriers to adoption. Yet, by studying the full population of organizations, we find, as mentioned, attention to be a first-order barrier to the organizations that would never appear in studies building on self-selected samples.

Second, our results also inform the long-standing debate about “randomization bias,” which refers to the tendency of individuals who anticipate the greatest effects from interventions to be more likely to participate in experimental studies (Heckman 1992, 2020; Heckman and Smith 1995; Allcott 2015). Without data on those who opt out at the first stage, it is difficult to assess what factors influence selection into experiments. This issue extends to the challenge of scaling up evidence-based programs because there is limited understanding of the factors that contribute to increase adoption by the majority of relevant organizations. On the one hand, we show substantial selection into the study, since only about 20% of the schools adopted the program based on the initial e-mail invitation. On the other hand, our results do not indicate that schools decided not to participate because of the expected costs and benefits of running the program. We also see no indications that late adopters implement

the program to a lesser extent. Instead, lack of attention seems to be the main explanation for adoption.

Third, the paper also relates to theories about bounded rationality, satisficing (Simon 1956), and incrementalism (Lindblom 1959). In the attention economy of today (Hayes 2025), policymakers are faced with an information-rich-world with high search costs (Simon 1996) but they have limited attention to spare. Therefore, they may be choosing among a smaller set of options – options that might not include the optimal one (Simon 1978). In this environment, it becomes important to know how to draw decision-makers' attention towards evidence-based interventions. Delegation of decisions within an organization may be a rational way of handling the problem of each individual's limited attention. In our case, the school principal (not the school board) has the formal responsibility of making the decision about adoption of the program, but he or she may delegate that responsibility to a head teacher. However, registration files from our telephone calls (in Step 3) reveal that in practice many actors are involved. Analyses of these files revealed two noteworthy findings. First, in only 23% of the schools (in both the email reminder and physical sample groups) were the secretary or principal able to recall the invitation. Second, explanations for not having processed the invitation included changes in the principal's email address, failure in distributing emails to key actors, no practice for handling external invitations, reconstruction at the school, or illness among the employees. This multitude of explanations suggest that many schools do not have or follow procedures for handling such invitations, which again suggests that these organizations do not overcome the problem of limited attention span.

We draw on the vast experiences from the survey literature on how to increase response rates through emails and phone-calls (Edwards et al. 2023) as well as the marketing literature. This paper addresses the challenge of capturing attention—not just exposure—a key focus in marketing research. While marketing research highlights content (Rogers and Lasky-Fink 2023), layout (Dunaway and Searles 2023), and media types (Kruikemeier, Lecheler, and

Boyer 2018), even free offers often go unnoticed (Hopkins and Gorton 2024). Responding to calls for large-scale engagement studies (Hopkins and Gorton 2024), we use a field experiment to test stepwise strategies for increasing policy take-up. This contributes to research on real-world attention beyond lab-based eye-tracking studies (Dunaway and Searles 2023). We expand on these streams of literature by using the insights in the context of evidence adoption and drawing the attention of decision-makers towards evidence-based interventions.

Finally, our study relate to research on nudging. DellaVigna and Linos (2022) document an average effect size of 1.3 percentage points for government-initiated nudges. This is close to our difference in adoption between two versions of our initial invitation email. Given that the marginal costs of implementing nudges through letters are often minimal, there is strong justification for leveraging insights on how formal letters can be effectively presented. However, our results indicate that they may be ineffective for the majority of the target group if they do not attend to the letters in the first place. Using phone calls or other measures to reduce information acquisition costs and gain the attention of the target group may affect a much larger share of the target group than smaller (yet cheaper) changes to the invitation letters.

Methods

Institutional background

We study a shared book reading intervention designed to encourage parents to spend more time reading with their children. The intervention known as *READ* was previously tested in a randomized controlled trial, demonstrating an improvement in children's reading proficiency (Andersen and Nielsen 2016). To facilitate shared book reading, the intervention provides families with a selection of books and offers parents guidance on how to assist their child with reading. In this trial, we add to this core feature by providing families access to a

program website where they can access digital books as well as supporting material. The digital books are intended to enable parents to continue shared reading activities even after they have completed the physical books. We call the new intervention *READit*.

In a previous study in 2016, we offered the program to a random sample of all public schools. The adoption rate after one email reminder was 28.6% (Andersen and Hvidman 2024). The results presented in the present study are robust to controlling for whether schools were invited or accepted the invitation in the previous study.

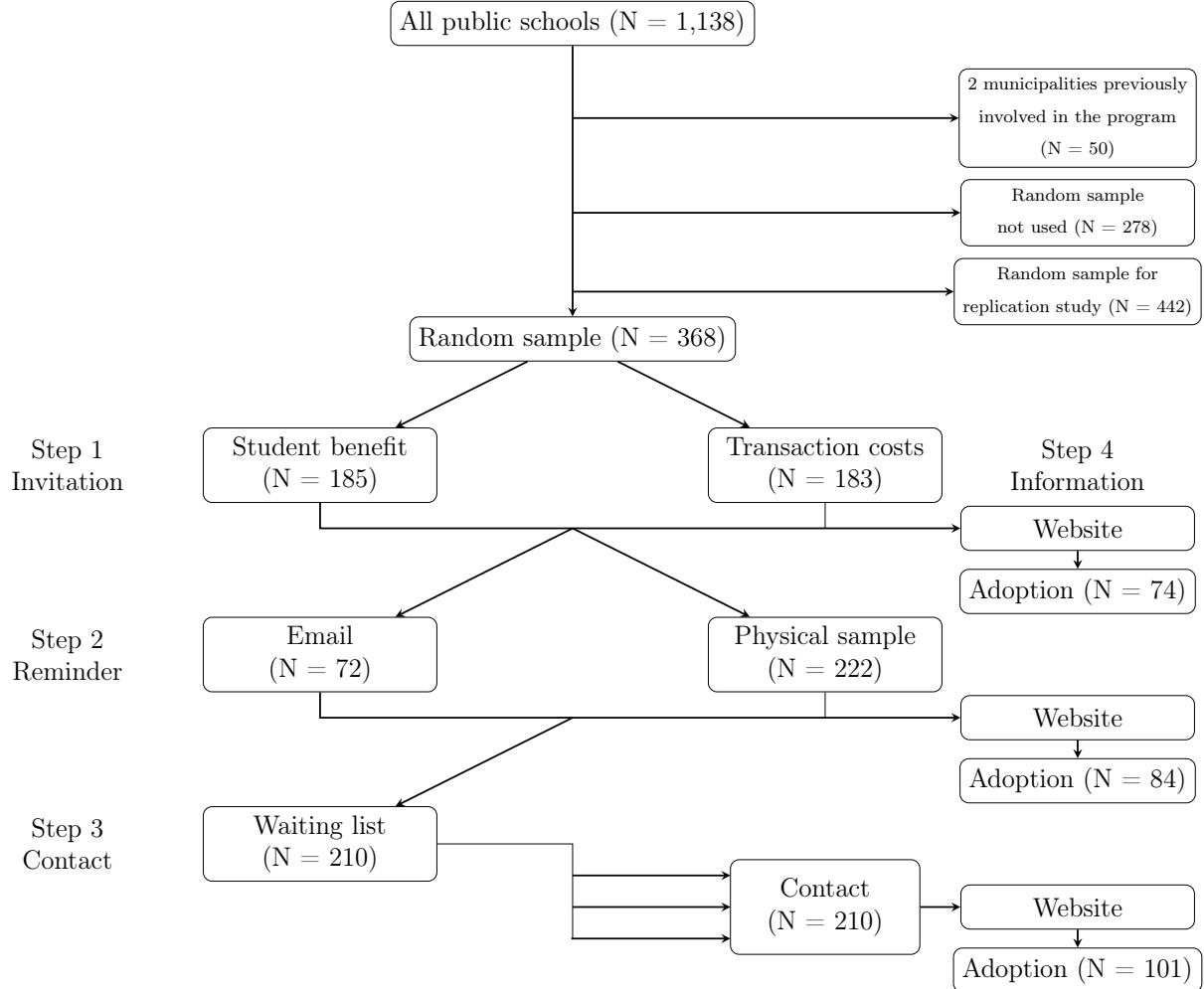
Our nationwide field experiment in Denmark provides an ideal context for studying evidence-based program adoption for several reasons. First, it allows us to study the adoption of an evidence-based reading intervention, which has proven effective in a randomized controlled trial conducted in a large Danish municipality (Andersen and Nielsen 2016).³ Second, a donation from an independent foundation allowed us to offer the program without charge. This makes the cost-benefit ratio for the schools very favorable and increases the likelihood of program adoption. Third, the setting allowed us to conduct these as natural field experiments, implying that (i) the experiments were conducted in the natural environment, which is identical to the environment if the program was implemented as national policy, (ii) there was no self-selection into the trial, and (iii) subjects were unaware that researchers evaluated the effects of the invitation (Harrison and List 2004).⁴ These features of the setting increase the external validity of the results.

To provide families with access to books, we depend on assistance from their respective schools. The schools are responsible for delivering the physical books to the children, assisting them in logging in to the online platform and encouraging them to read at home. In the first phase of the study, we invited a random sample of 368 Danish public schools, representing

3. We exclude two municipalities that were involved in the development of the program, READ, since schools in these municipalities already had attention to and detailed information about the program.

4. We informed schools, though, that we used administrative register data (such as a list on all public schools) for the purpose of research. The project was ethically approved by the Institutional Review Board at Aarhus University (no. 2021-99).

Figure 1: Flow diagram



34 % of all public schools in the country, to participate in the program.

Four embedded experiments

Figure 1 illustrates the design involving the four embedded experiments. Steps 1, 2, and 4 were described in the preregistration.⁵ Step 3 and the replication study were not included in the preregistration.

First, we selected a random sample of all public schools in order to have no self-selection

5. Link to preregistration: https://aspredicted.org/5JK_RG9

into the study.⁶ Table A1 in the Appendix shows that the selected sample is comparable to the rest of the schools in terms of observed student and school characteristics.

Step 1. In the first embedded experiment, we tested whether invitation emails with information about the low transaction costs of implementing the program were more effective in making schools adopt the program than information emphasizing the evidence of student benefits of the program. Evidence about the benefits may be the traditional “what works” approach in evidence-based policy making (Baron 2018). The full content of both versions of the email is displayed in Appendix Figure A1.

We randomly assigned schools to one of the two versions of the invitation email.⁷ The invitation experiment lasted 13 days beginning March 3, 2022.

Table A2 in the Appendix presents a balance check of the randomization by comparing differences between the two groups. As evident from the table, there is a minor, yet statistically significant difference in the share of students with high well-being. To see that this imbalance does not substantially affect the results, we use statistical models both with and without covariates, yielding no changes to the results.

We preregistered the hypothesis that “Highlighting the low costs for managers increases take-up more than highlighting benefits for clients.”

Step 2. Since school principals in Denmark receive the predominant share of requests by email, delivering a physical copy of the program may be more effective in getting their attention (Kruikemeier, Lecheler, and Boyer 2018).

In Step 2, we randomly assigned schools that had not yet signed up for the program after 14 days to either a standard email reminder (including a copy of the original invitation) or a

6. We selected another random sample for a replication study, which we return to below.

7. 10 school principals were in charge of more than one school. We cluster randomized at the principal level to avoid contamination between schools with the same principal. Clustering standard errors at the school principal level does not change the statistical significance of the effect estimates.

reminder in the form of a physical copy of the books and information for parents contained in the READit program as well as the same reminder text as in the email.⁸ Schools that had received the student benefit invitation in Step 1 received a text repeating the student benefit message, whereas schools from the transaction cost group in Step 1 received a text repeating the transaction cost message.

Figure A2 shows the bag and its content next to the content of the (email) reminder (in its two versions). The bag contained informative materials about the program, examples of supplementary resources for teachers, and examples of book content. As mentioned, the bag also contained a letter with the same text as in the email-reminder (with transaction cost information in the transaction cost group in Step 1, and student benefit information in the student benefit group in Step 1).

The physical copy was delivered by a postal service. The reminder phase lasted 33 days.

The physical copies of the materials did not contain any new substantial information relative to the first invitation and the email reminder. Yet, even though we do not find any indication of an interaction effect between the content of the email in Step 1 and the reminders in Step 2 (see appendix Table A18), our replication study (see below) used a more simple design to rule out such interaction or spill over effects between the invitation steps.

In the preregistration, we hypothesized that “Sending physical samples increases take-up relative to an email invitation.”⁹

Step 3. In Step 3, we placed all schools that had not yet signed up on a waiting list and randomized at what day we started calling them on the phone. We started the waiting list experiment 47 days after the initial invitation. At this point, no schools had adopted

8. Schools had a 3/4 chance of being assigned to the physical copy group (see Figure 1), in order to increase the overall adoption rate.

9. In the preregistration, we said that we would evaluate the effect of the physical copies two months after we sent them off. However, since no schools signed up after about 35 days since the first invitation, we stopped Step 2, the Reminder phase, and initiated Step 3, the Phone Call phase, which was not preregistered.

the program in the last 8 days, and 210 schools (57% of the sample) had not accepted the invitation.

The phone call phase lasted 23 days. However, our analysis includes a longer time frame because the schools that were last in the call order needed time to consider the invitation. The last school to accept the invitation did so 29 days after the first call was made to any school.

The purpose of the phone calls was to remind the schools of the possibility of participating in the program and address potential questions they may have had. To maintain consistency in communication, we developed a protocol for the two callers responsible for making the calls.¹⁰ The first call was made to the school’s main phone number. If the schools did not respond to our call after three attempts, we ceased further contact.

We could not use a traditional intention-to-treat estimation to assess the effect of phone calls because all schools received a treatment. To address this issue, we structured our data in long format with treatment and adoption status for each school, i , at every day, t , of the contact period starting with phone calls on April 19 (day=0) and ending with the latest school adoption on May 18 (day=29).

Some schools might have decided to adopt the program before the first phone call but did not sign up until after the phone call. In order not to ascribe any such potential adoptions to the phone call, we include a model with a three-day “washout period”, meaning that we exclude school-day observations on the day of the phone call and the following two days (three days in total).

We also present a model that uses the three-day “washout period” and, additionally, includes school-fixed effects to ensure that the school would not have enrolled in the absence of a phone call. The assumption in the school-fixed effects waiting list design is that the school in the days prior to treatment (phone call) is a valid counterfactual for the school in

10. Table A9 in the Appendix shows that there is no correlation between the caller and the sign-up rate.

the days after the treatment. To illustrate the validity of this assumption, we plot the trend line in Figure 2 for the 210 schools before their phone call (dashed lines). As evident from the figure, the schools had a flat trend line until they received treatment in the last phase, which increases confidence in the validity of the estimates.

Step 4. In Step 4, we tested what type of information was of primary interest to the decision-makers *once they paid attention to the invitation*.

If schools wanted to adopt the program, they were asked to visit a website containing information organized in five boxes (see Appendix Figure A4 for a copy of the website). We randomized the order of four of the boxes. We labelled the boxes (i) “Start-up costs in relation to READit” (containing information about transaction costs), (ii) “READit improves student’s reading proficiency” (containing information about the benefits of the program for students), (iii) “This is how the program runs” (containing information about the costs of running the program), and (iv) “READit supports teachers’ parental collaboration” (containing information about the benefits of the program for teachers). A fifth box “Read about READit” was placed on top of the other boxes and was not randomized. The content of the information boxes is not immediately visible; decision-makers must actively select the information they wish to access by clicking a box in the information board, following techniques for studying information processing (Lau and Redlawsk 2001).

We recorded which of the boxes that schools clicked on first as an indication of their primary interest in the four types of information (compared to the non-randomized box with general information about the program). In the preregistration, we registered this as a secondary outcome: “whether the first click on the website concerns information on the transaction costs, the running costs, the benefits for employees or benefits for the clients.”¹¹

11. The only other secondary outcome in the preregistration was whether schools logged on to the website. All results with this outcome variable (instead of adoption of the program) are presented in Appendix section ‘Additional preregistered analyses’ Tables A13–A17.

We simply tabulate the schools' first choice of box to calculate the proportion of first clicks. To assess the differences between the proportions, we calculated 95% confidence intervals for each of the proportions. The confidence intervals are calculated as $\pi \pm 1.96 \frac{\sigma_\pi}{\sqrt{n}}$, where π represents the proportion of first clicks on the box, σ_π is the standard deviation (calculated as $\pi(1 - \pi)$), and $\frac{\sigma_\pi}{\sqrt{n}}$ denotes the estimated standard error.¹²

The analysis includes all schools that made a click on a box, which is 180 schools (some schools did sign up without making such an information search beforehand).

Results

We present our results in line with the four step invitation procedure described in the methods section. Afterwards we present additional analyses to support the interpretation of the results.

Step 1. Invitation: Transaction Costs Information in Invitation Email

We estimate a linear probability model to test the effect of transaction cost framing on adoption of the program:

$$Y_i = \beta_0 + \beta_1 Transaction_i + \mathbf{X}_i \boldsymbol{\beta}_2 + \epsilon_i, \quad (1)$$

where Y_i is a dichotomous variable that captures whether school i has adopted the program (signed up) or not. β_0 captures the adoption rate in the student benefit frame control group. $Transaction_i$ is an indicator of schools in the transaction cost frame group. \mathbf{X}_i is a vector

¹². Despite the randomization of the order of boxes, some imbalance in the order occurred. Figure A5 in the Appendix presents the distribution of the first clicks both with and without accounting for this imbalance. This has no substantial impact on the results.

of covariates included in some model specifications. β_1 expresses the effect of the transaction costs frame, and ϵ_i is an error term. The analysis includes all the invited schools: $N = 368$.

Figure 2 shows how the adoption rate developed from when the invitations were sent out by the experimental conditions in Steps 1-3. The first part of the figure (Step 1 from day 0 to day 13) shows the difference in sign-up rates between the two versions of the email invitation. It shows that 19.5% of the schools receiving the student benefit framing signed up, and 20.8% (an additional 1.3 percentage points) signed up when receiving the transaction cost framing. This difference is not statistically significant. Table A3 in the Appendix shows the formal test of the difference between the two invitations with and without covariates included.

The estimated effect size of 1.3 percentage points corresponds to the average effect size in nudging studies of variations in written correspondence (DellaVigna and Linos 2022). Since it is statistically insignificant, we cannot rule out that there is no effect of the transaction cost framing. However, results from the subsequent steps in our design suggest that the effect may be repressed by a majority of decision-makers' lack of attention to the email.

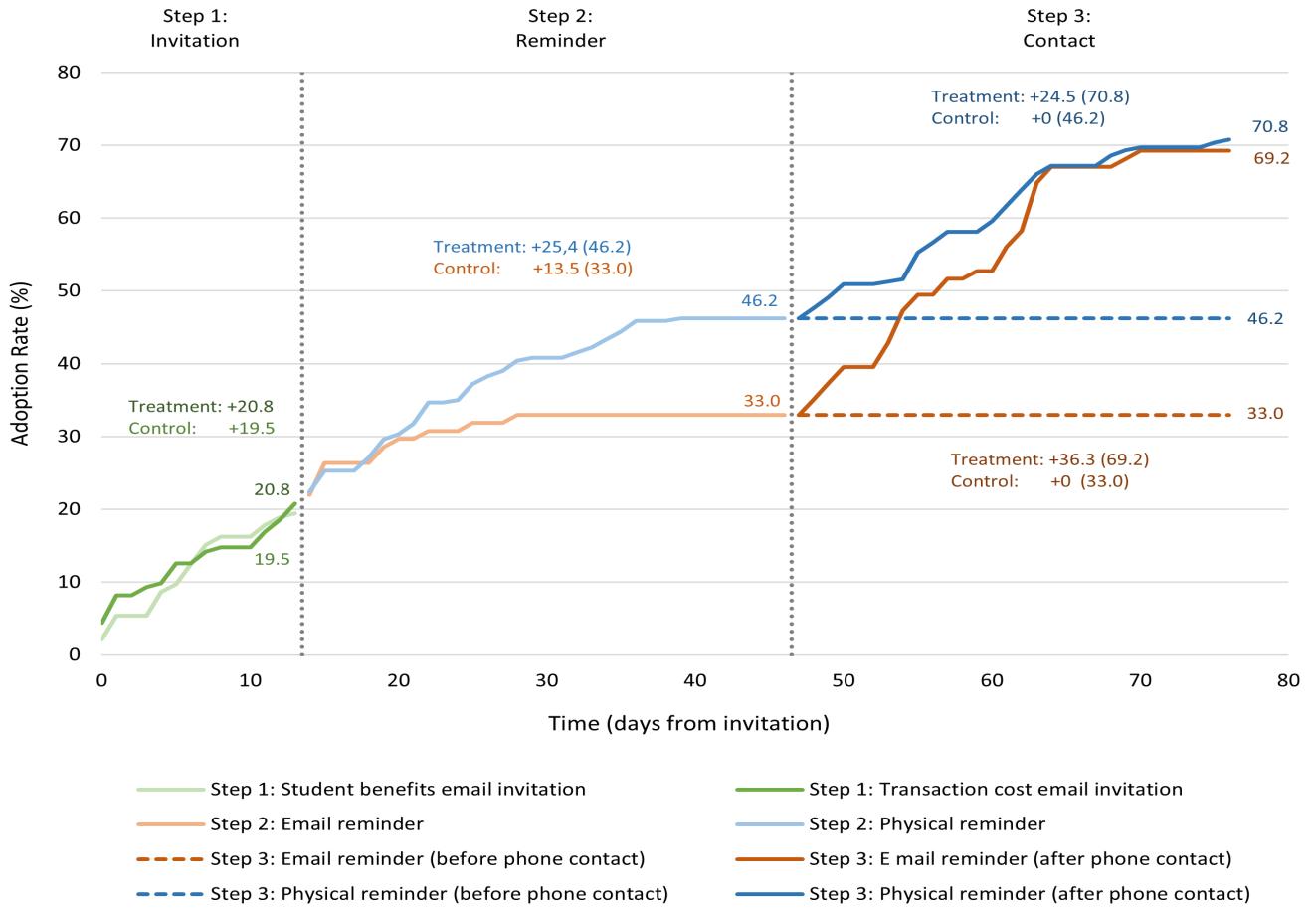
Step 2. Reminder: Physical Sample

As preregistered, we estimate a linear probability model to test the effect of the physical sample in the reminder phase:

$$Y_i = \beta_0 + \beta_1 Physical_i + \mathbf{X}_i \beta_2 + \epsilon_i, \quad (2)$$

where Y_i is a dichotomous variable that captures whether school i have adopted the program or not. β_0 shows the adoption rate in the email reminder group, β_1 expresses the effect of the physical sample, $Physical_i$, relative to the email reminder, \mathbf{X}_i is a vector of covariates included in some model specifications, and ϵ_i is an error term. The analysis includes the invited schools that had not accepted the invitation when the reminder phase began, which

Figure 2: Sign-up rate during Experiments 1-3 by experimental conditions



Note: $N = 368$. The flat dashed lines in Step 3 represent the control groups from the waiting list experiment. The waiting list control group consists of the schools that have not yet received a phone call on any given day. The number of schools in the control group is therefore reduced day by day until $N = 0$.

is $N = 294$.¹³

Table A4 in the Appendix presents a balance check on the groups receiving either email reminders or physical copies in the reminder phase. The table shows that there are no significant differences between the two groups, which increases the confidence in identical

¹³ β_1 can be thought of as the local average treatment effect (LATE) for the ‘compliers’, i.e., schools that did not sign up in Step 1. Since this is a selected sample, we cannot be certain that the effect of physical copies would be the same for ‘non-compliers’, i.e., schools that signed up in Step 1. The effect size from Step 1 on the full sample, can therefore not necessarily be compared directly to the LATE in Step 2.

outcomes in the absences of treatment. However, as a robustness check, we also present an analysis in the Supplementary Information where the covariates are included in the models. This approach does not alter the results.

Step 2 in Figure 2 shows that adoption increased by 13.5 percentage points in the group that received the email reminder. Some of the schools that signed up in the reminder phase might have signed up anyway, so we only cautiously interpret this as a causal effect of the email reminder.

The figure also shows that the physical copy increased adoption by 25.4 percentage points, which is an additional 11.9 percentage points relative to the email reminder ($p < 0.01$), and this has a causal interpretation given the randomization to the two reminder conditions. The formal test of the effect of the physical copy of the invitation is presented in Appendix Table A5.^{14 15}

Even though the physical reminder indirectly contained some additional information relative to the email (such as the quality of the paper and other materials in the physical sample), we believe that this was of little importance to the schools' decision (or lack of decision) to sign up. This interpretation is supported by insights from the subsequent phone calls to schools that had not yet signed up (see Step 3). Only in very few instances did the schools contacted by phone ask for information that was not already available on the web page. Yet,

14. Appendix Table A5 shows an adoption level of 15.3% in the email reminder and an additional 17.6%-points in the physical copy group. These are not the same numbers as in Figure 2 because the figure compares enrolment in each step to the total number of schools, whereas the regression analyses in the appendix table only include the schools that were included in the experiment in Step 2 (those that had not adopted the program in Step 1).

15. In the preregistration, we described that we would make an exploratory test of the interaction between the treatment arms in step 1 and step 2 in a 2x2 factorial design. The results of this analysis is shown in the appendix in Table A18. We find no statistical significant interaction effect. If we perceive the treatments in Step 1 and 2 together as four experimental conditions (Student Benefit/Transaction Cost Invitation X Email/Physical Reminder), we should adjust for multiple hypothesis testing. Table A19 in the appendix uses the procedure proposed by List, Shaikh, and Xu (2019) and compares it to unadjusted p -values, as well as Bonferroni and Holm corrections. The difference between the Student Benefit + Email reminder group and the Transaction Costs + Physical reminder group is 14.9%-points with an unadjusted p -value of .069. With the List, Shaikh, and Xu (2019) correction, the p -value is .154.

there could be other explanations for the effect of physical copies (including some effect of reciprocity or social pressure). Below we address such interpretations in two ways. First, we examine how those who signed up after they received a physical copy implemented the program afterwards. If it was only a gesture of reciprocity or reaction to pressure, they may not have worked as hard on subsequent implementation. Second, we present a replication study, where we only used reminder phone calls (instead of physical reminders) in order to have a more clean test of the reminder effect.

We also note that 7% of the schools logged on to the website in Step 1 or Step 2 but did not adopt the program until the subsequent step when reminded about the program. For these schools, the obstacle to signing up might not have been the initial attention, but rather the necessity of maintaining attention, as they adopted the program after being reminded again. Tables A13–A17 in the Appendix show that login rates to the web pages closely mirror sign up rates, thereby indicating that the majority of schools visiting the website also signed up.

Step 3. Contact: Phone Calls

We leveraged our waiting list design and estimated the treatment effect of receiving a phone call in Step 3 using a within-school before-after design as shown in this equation:

$$Y_{it} = \beta_0 + \beta_1 Phone_{it} + \epsilon_{it}. \quad (3)$$

Y_{it} is a dichotomous variable that captures whether school i has adopted the program or not at day t . β_0 is the constant, β_1 expresses the effect of the phone call, $Phone$, relative to the days before the call, and ϵ_{it} is the error term. We denote this basic model, Model I.

The third part of Figure 2 shows that sign-up increased by 24.5 percentage points in the group that had received physical copies in Step 2 and by 36.3 percentage points in the email

group that had received an email reminder. The formal test of the average treatment effect of the phone call is presented in Appendix Table A6.¹⁶ After the beginning of the phone call phase, no schools signed up prior to receiving a call, which indicates that the increased adoption rate can be interpreted as a causal effect of the phone calls.

Interestingly, the phone call increased sign-up to an almost identical level for the schools that had received an email reminder (69.2 percent) and the schools that had received a physical copy (70.8 percent) in Step 2. This indicates that the physical copy and the phone calls had the same effect of gaining the attention of the schools, regardless of any subtle additional pieces of information contained in either of the two treatments and regardless of the difference in timing.

Only 23 schools reached by phone call declined the invitation (6% of the full sample). This also indicates that few of those who had not yet signed up before the phone call phase did so based on a deliberate decision.

Among those 30% schools that ended up not adopting the program, we did not manage to get in contact with the relevant decision-maker in 45% of these schools after three attempts. We cannot know whether they would have declined adoption if we had been able to get in contact, yet we suspect that a substantial portion of this group did not participate because of a lack of attention to the invitation.

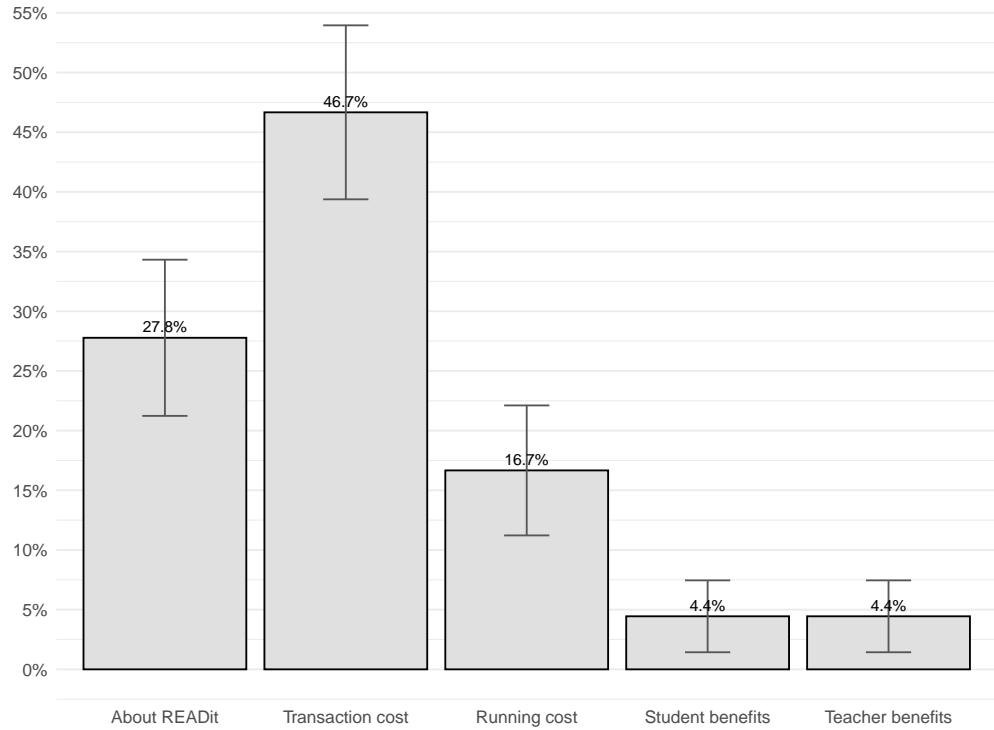
Step 4. Information: Transaction Cost Information on Website

Figure 3 shows that 46.7% of the first clicks were made on the transaction cost box. This is significantly higher than the box with the general information about the READit program (27.8%), even though this box was placed at the top of the website. Boxes on running costs, student benefits and teacher benefits were selected as first option in significantly fewer

16. Again, the average treatment effect of 39 percentage points in model 1 in Table A6 is higher than the numbers in Figure 2 because the analysis includes only the 210 schools that had not signed up before the phone call waiting list experiment began in Step 3.

instances.

Figure 3: First Click on Web Page Information Boxes



Note: Distribution of first clicks on website information boxes (schools=180). Fractions are presented with 95% confidence intervals.

We see this as an indication that conditioned on having gained the attention of the decision-makers, the effects of evidence-based programs are a second-order priority, and the first priority is the immediate costs of implementing the program. This indicates that the null result in the email experiment in Step 1—examining the effect of the transaction cost frame vs. the student benefit frame—may be explained by schools not paying attention to the invitation.

Interpreting the Results

We argue that attention is a first-order barrier to adoption of the evidence-based program, which means that late- and non-adoption was primarily a consequence of passive nonresponse in the framework of Dutz et al. (2021). In this section we exploit additional data to test alternative explanations of the results. We group these alternative explanations in three categories following Al-Ubaydli, List, and Suskind's (2017) three explanations of why interventions may lose their small-scale effectiveness when implemented at scale: (i) effects from small sample studies may be due to a statistical artifact (see also Gelman and Carlin 2014), (ii) individuals selecting into the small scale studies may have higher expected benefits than the average population (related to ideas about randomization bias, Heckman 1992), and (iii) representativeness of the situation, which may relate to differences in the way interventions are implemented (cf. Andersen and Hvidman 2024).

Statistical Inference: A Replication Study

The initial trial included 368 schools. In Step 2 comparing the email reminder and the physical copy, we had 294 schools and an effect size of 17.6%-points (see Table A5), which gives a statistical power of 85%.¹⁷ Still, this is a large effect size, and there is a risk that our results are driven by chance findings.

Another concern with the initial trial might be that information received in the email in Step 1 could interact with the effect of the physical copies and email reminders in Step 2, even if we do not see any indications of this (see Table A18 in the appendix.)

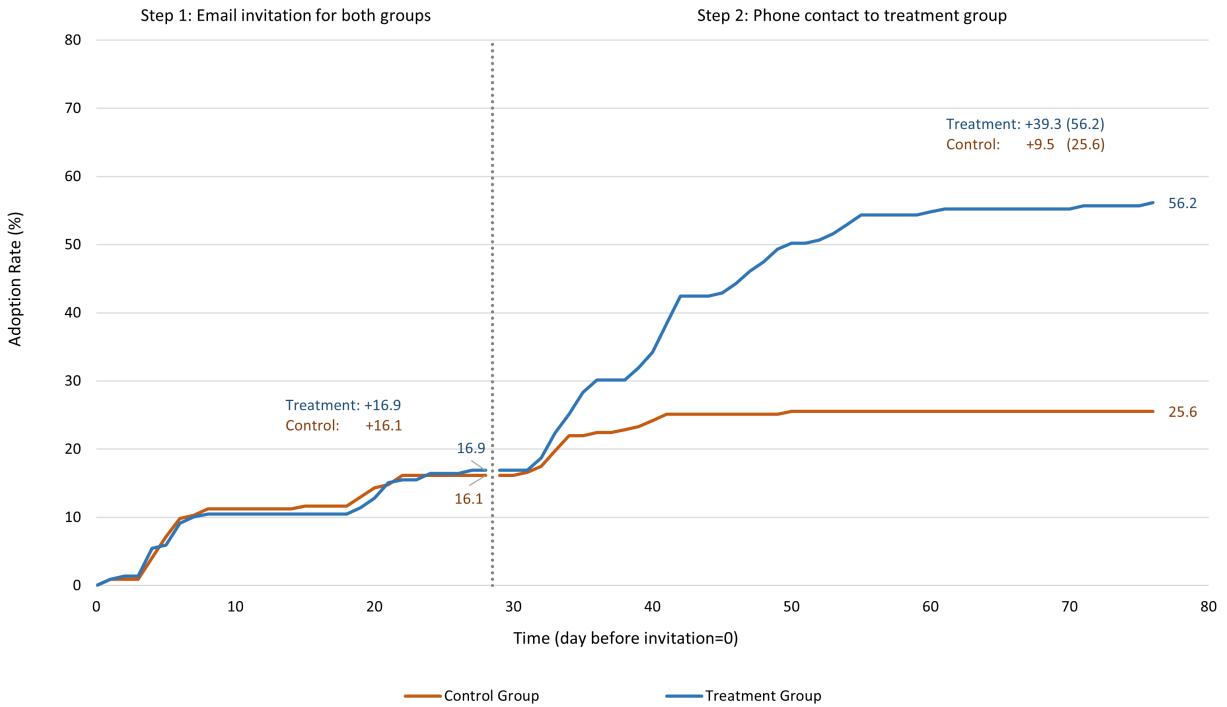
We therefore ran a replication study on another random sample of the full population of all public schools (see Figure 1). We randomly assigned the schools to a control and a treatment group. At baseline all schools received the same initial invitation email. After 28

17. This power calculation uses an adoption rate of 15.3% among the 72 schools in the email reminder group and 32.9% among the 222 schools in the physical copy group and an alpha-level of 5%.

days we contacted schools in the treatment group by phone to ensure that they had noticed the invitation to the program. The control group received an email reminder.

Figure 4 presents the results of the replication study. We observe no systematic difference between the two groups at baseline before the phone call intervention started. Adoption rate at this point was 16.1% and 16.9%. After initiating the phone calls and sending the email reminder to the control group, adoption rate increased to 25.6% in the control group and 56.2% in the treatment group, yielding an effect of 30.6%-points. (For balance between the experimental conditions at baseline and a formal test of the difference, see Appendix Tables A10 and A11.)

Figure 4: Sign-up rate in Replication Study



Note: $N = 442$.

The adoption rate was lower in the replication study than in the initial trial.¹⁸ Yet, the

18. We expect this to be explained by invitation to another research study done by another research insti-

phone call still more than doubled the adoption rate.

The replication study results suggest that the large effect of reminding schools in the first trial, was not just a statistical coincidence. How much should the initial study and the replication study change our priors about the effect of getting the attention of the decision-makers? This depends on the priors, and the power of the studies. Rogers and Lasky-Fink (2023, 54, 59, 63) report three studies in which a concise email roughly doubled the response rates relative to wordy emails. Such results may increase our priors that getting readers' attention may actual double the impact on their immediate reactions, even if effects on subsequent behavior (in on study this was tested as campaign donations), were smaller.

Table 1 shows the chances that it is true that reminders increase adoption rate as much as found in the initial study and the replication study, i.e., the post-study probabilities (PSP) that the hypothesis is true depending on the priors and the number of studies confirming the hypothesis. We assume a power of 85% (based on Step 2 in the initial study, see above), and alpha-level of 5%, and we use the formula presented in Moonasinghe, Khoury, and Janssens (2007) (see also Maniadis, Tufano, and List 2014).¹⁹

tution in Denmark in a period just before our invitations came out.

19. To evaluate the post study probability (PSP), we use this formula:

$$PSP = \frac{Prior * TruePositives}{Prior * TruePositives + FalsePositives},$$

where

$$TruePositives = \sum_{i=r}^n \binom{n}{i} (1 - \beta)^i (1 - (1 - \beta))^{(n-i)},$$

and

$$FalsePositives = \sum_{i=r}^n \binom{n}{i} (\alpha)^i (1 - \alpha)^{(n-i)}.$$

$1 - \beta$ is the power of the study (one minus the Type II error rate), α is the Type I error rate, n the total number of studies, and r is the number of significant results confirming the hypothesis.

Table 1: Post-study probability that hypothesis is true

Prior	# Studies confirming the hypothesis		
	0	1	2
0.01	0.0099	0.0911	0.7429
0.10	0.0909	0.5006	0.9666
0.20	0.1667	0.6672	0.9830

Note: Cell entries show the post-study probability that a hypothesis is true conditioned on the prior and the number of studies confirming the hypothesis. The calculations assume power of 85% and alpha-level of 5%.

If none of the two studies had confirmed the hypothesis, the PSP would be a little smaller than the prior. If just one study confirmed the hypothesis and with a prior of 0.01, the PSP would be only 0.09. However, with both studies confirming the hypothesis, the PSP becomes 0.74. If we are willing to assume a prior of 0.10, then the PSP would increase from 0.50 with one confirming study to 0.97 with two confirming studies.

In sum, the replication study indicates that initial results were not a chance finding or driven by some interaction between the transaction cost information and reminders.

Population Representativeness and Randomization Bias

Another interpretation of the results would suggest that the reason that some schools do not adopt the program, or only adopt it after several reminders, is that these schools simply should not expect to have the same benefit of the program as the early adopters. This would be active non-participation due to costs exceeding benefits in the framework of Dutz et al. (2021). This is similar to the idea of randomization bias suggesting that those who opt into research projects are those with the highest expected benefits (Heckman 1992).

To examine this explanation we leverage the fact that we have administrative data on student composition for all groups (early, late and non adopters). We estimate their expected benefits based on the evidence about the program, which was published at the time of the

intervention. The evidence showed how much the effects of the intervention were larger for students with immigrant background and mothers with low education (Andersen and Nielsen 2016). This information was summarized at the READit website, where schools could read about the program. The information was also to some extent present in the initial student benefit invitation email (we return to this below). Even if schools have not read this information, let alone knew about the scientific article presenting the exact effect estimates, using the evidence from the article may be the best way of estimating what effects schools with different student compositions should expect from participating in the program. We therefore multiply the interaction coefficients from the published study on the student composition to examine the expected effects from participating for early, late, and non-adopters, respectively.

The results are displayed in Table 2. There is little evidence that early adopters had significantly higher expected effects than later adopters and non-adopters. Relative to early adopters, late adopters have slightly lower expected effects ($\beta = -.0017(0.0019)$), yet the difference is small and statistically insignificant (model 1). The average effect size was 0.128 standard deviations, so with 95% confidence intervals, late adopters would at least have expected benefits of $(0.128 - 0.00173 - 1.96 * 0.0019)$ 0.123. The similar is true for non-adopters ($\beta = -.0007(0.0021)$). Models 2 and 3 nuance these findings by dividing late adopters into groups requiring different types of reminders and phone contact. Yet, the conclusion remains the same. They have almost the same expected benefit as early adopters. (Even if the email reminder group has a lower effect (of $\beta = -.007$) which is significant at the 10 % alpha level.)

The risk of randomization bias based on expected benefits may be largest among schools that were informed about the student benefits in the original invitation, since this version of the invitation included information that effects were larger for students with different ethnic background than Danish (see Figure A1). To examine this, we subset the data to the schools that received information about student benefits. Appendix Table A12 confirms that also in this subsample, predicted effects are very similar across early-, late-, and non-adopters.

Table 2: Differences in Expected Effects of the Intervention by Early-, Late-, and Non-Adopters

	(1) Expected effect	(2) Expected effect	(3) Expected effect
Late adoption	-0.0017 (0.0019)		
No adoption	-0.0007 (0.0021)		
Late adoption			
- Reminder		-0.0037 (0.0023)	
- Phone contact		-0.0002 (0.0022)	
No adoption		-0.0007 (0.0021)	
Late adoption			-0.0079 [†] (0.0046)
- Email reminder			-0.0030 (0.0024)
- Physical reminder			0.0001 (0.0030)
- Email reminder (after phone contact)			-0.0003 (0.0024)
- Physical reminder (after phone contact)			-0.0007 (0.0021)
No adoption			
Constant (mean of early adopters)	0.128** (0.0016)	0.128** (0.0016)	0.128** (0.0016)
Observations	368	368	368
Adjusted R^2	-0.003	0.001	-0.000

Note: OLS regression of the expected benefits on indicators for late and no adoption. The constant represents the mean for early adopters and is the expected standardized effect after 7 months with no interaction effects from the original study for this group of schools. The first column compares early (reference group), late, and non-adopters. The second compares early, reminder, phone contact, and non adopters. The third column divides the group into different types of reminders and non-adopters. [†]p < .1, * p < .05, ** p < .01, *** p < .001.

In sum, we do not find indications that differences in adoption can be explained by differences in the expected benefits of the program.

A third explanation may be that late adopter schools agree to adopt (for instance due to some effect of reciprocity or social pressure; Gerber, Green, and Larimer 2008; Malmendier and Schmidt 2017), but do not actually adopt the program, in the sense that they implement the program to a smaller extent than early adopters. In that case difference in adoption is not caused by lack of attention but differences in willingness to implement the program. Therefore we study how much schools succeeded in engaging parents.

To examine this, we compare the implementation of early and late adopters. We consider three outcomes in Table 3: The number of students enrolled, the percentage of parents who visited the reading platform at least once, and the percentage who opened an e-book on the platform. We apply OLS-models of implementation at the student level with clustered standard errors at the school level.

For all three outcomes we see substantively small and statistically insignificant results. At early adopting schools 69.9 % of the children are enrolled on average. Late adopters enrolled 69.0 % (0.9 % fewer) of the parents, yet the difference is not statistically significant. These rates of enrollment corresponds to 4,242 students in early adoption schools received the book bags and access to the website. Additionally 10,456 students received bags and access to the website on the late-adopter schools due to the various reminders. A next step in the line of implementation is the percentage of children who accessed the reading platform. 29.9 % accessed the platform in the early adoption group, while the late adopters were 0.5 % less likely to visit the platform. Finally, 14.9 % opened an e-book on the platform in the early adoption group, whereas late adopters were 0.6 % less likely to do so. Similarly to the number of enrolled children and the percentage opening the first page, this difference is also statistically insignificant.

These estimates are somewhat imprecise and we cannot rule out some lower levels of

implementation (for instance, with 95 % confidence intervals, enrollment rate may be 7.5 % points lower among late than early adopters). However, we find no clear indications that those who only adopted the program after being reminded about the invitation, had lower levels of implementation afterwards.

Table 3: Implementation for Early and Late Adopters

	Enrolled (1)	Accessed website (2)	Opened Book (3)
Late adopter	-0.009 (0.043)	-0.005 (0.022)	-0.006 (0.014)
Constant (mean of early adopters)	0.699*** (0.038)	0.299*** (0.020)	0.149*** (0.012)
Observations	21,240	21,240	21,240
Adjusted R^2	0.000	0.000	0.000

Note: OLS regressions of the outcomes (enrolled, accessed website, opened book) on an indicator for late adopters. The sample is only early and late adopters (since non-adopters did not implement the program at all). The constant represents the mean level of implementation among the early adopters. †p < .1, * p < .05, ** p < .01, *** p < .001.

Discussion

In two field-experimental studies we demonstrated how capturing decision-makers attention leads to at least doubling the adoption of evidence-based programs. Furthermore, we showed that the findings could hardly be attributed to randomization bias or differences in subsequent implementation behavior. How do these results reconcile with previous studies of evidence adoption?

In a study of 132 soccer ball producers in Pakistan, Atkin et al. (2017) found that the adoption rate of a new technology that had clear net benefits was about 11% after initial contact (corresponding to our early adopters) raising to 14% when late responders—who only responded after additional attempts to get their attention—were included. The main expla-

nation of the low take up among those who did pay attention was what we call transaction costs, namely opposition from the employees in shifting to the new technology. In a second experiment, Atkin et al. (2017) found that this opposition could be overcome after changing the payment scheme. The authors note:

“A natural question is why the firms themselves did not adjust their payment schemes to incentivize their employees to adopt the technology. Our model suggests two possible explanations. The first is that owners simply did not realize that such an alternative payment scheme was possible or desirable, just as the technical innovation had not occurred to them. The second is that there was a transaction cost involved in changing payment schemes that exceeded the expected benefits in this case. In the end, the two hypotheses have similar observable implications and are difficult to distinguish empirically” (1159).

Our results are consistent with these findings and provide a possible answer to the question of why the firms did not adjust the payment schemes. We show that attention is a first-order barrier to adoption of evidence-based programs, while transaction costs seem to be of secondary importance.

Our results are also consistent with a study of 28 textile plants in India by Bloom and colleagues (Bloom et al. 2013; Bloom et al. 2020). In their initial trial they informed plants (within firms) about effective management practices. The control group only received information about the practices, whereas the treatment group received consultancy which—in our interpretation—reduced the transaction costs of implementing the practices. The control group increased their adoption of the practices from about 26% at baseline to 38%. The consultancy treatment group increase adoption from about 26% to 63%, demonstrating the importance of the consultancy treatment (Bloom et al. 2013). Interestingly, when returning to the same firms nine years later, other plants within the treated firms had adopted management practices at the same level as the treated plants. And in the control group where

some plants received information, other plants within the same firm, had adopted the same level of practices. Yet there were no indication of spill-overs from treated firms to non-treated firms. These results are also consistent with the notion that attention within firms ensures that best practices diffuse from one plant to another plant, but that there is little attention to what happens in other firms.

In the study of evidence adoption by US cities, DellaVigna, Kim, and Linos (2024) conclude that “organizational inertia,” (the costs of deciding upon and prioritizing new initiates) determines evidence adoption, and not the strength of the evidence: “In cases with preexisting communication, there is a routine process and staffing in place to send the communication, so the first step is not a hurdle, and altering the wording to adopt an effective innovation is relatively straightforward, leading to high adoption. In cases with a new communication set up for the experiment, instead, there is no automatic pathway to send it again, leading to low adoption” (2754). This is in line with the traditional organizational change, inertia, and resistance to change literature (Oreg, Vakola, and Armenakis 2011; Hannan and Freeman 1984; Kotter 1995) that also point to the challenge to overcome the initial costs of adoption.

DellaVigna, Kim, and Linos (2024) is a study on 30 cities that had already participated in a randomized trial testing new forms of communication. In that sense they already payed attention to the innovations. In that situation, the costs of making employees implement the new procedure—what we call the transaction costs—are dominating. Studies on nudging often argue that the costs are negligible because they may pertain to as little as changing a few words. However, the *transaction* costs of adopting a new type of communication, a new letter, for instance, may be substantial and explain the limited adoption.

Garcia-Hombrados et al. (2024) sent emails to 13,709 mayors and local council members in Spain. About 38% of the emails were opened and about 6% clicked on the link within the email. The authors find that treatment increased improvement of the municipalities’ Wikipedia pages from 2.55% in the control group (receiving no information) to 4.23% when

the evidence information came from politically aligned sources. This is a rare example of a study with no self-selection into the study, and it demonstrates the importance of political alignment for policymakers (which may be different from the school principals in our study). At the same time, it also indirectly shows that about 62% never open the email and about 94% do not click on the link to receive the full information. We think this is consistent with our results indicating that attention is a first-order barrier for adoption in a full population of organizations.

It should be emphasized that even if we argue that attention is a first-order and necessary condition for evidence adoption, it is not a sufficient condition. We study a case in which there is evidence that benefits exceeds the costs, because there exists scientific evidence of the benefits of the program, and there are negligible running costs of the program thanks to a donation financing the costs of the book bags as well as the online platform. In cases with less clear benefit-cost ratios, active nonparticipation may be more dominant (cf. Dutz et al. 2021). In a study of the adoption of fertilizer technology in Uganda, Bold et al. (2017) find that 30% of nutrient is missing in fertilizer bought at the local market, which explains why adoption is low in this case. Relatedly, Bergman, Lasky-Fink, and Rogers (2020) find that school principals and other education professionals largely underestimate the effect of strategic defaults on parent participation in a program. Informing them about the effectiveness increases their willingness to pay.²⁰ If the benefit/cost ratio is low, uncertain, unknown or even misconceived, we would of course not expect attention (and transaction costs) to be sufficient explanations for adoption.

We also note that even if transaction costs was clicked on first among those considering signing up for our program, this was done in a context where they already had received some information about the program. Decision makers probably need some basic level of

20. We note, that the Bergman, Lasky-Fink, and Rogers (2020) study is made on a selective sample of workshop participants at Harvard and with a response rate of 43%, which means that it is less informative about any first-order barriers to adoption for those who do not participate at such workshops.

information about a program before they start considering the transaction costs.

Conclusion

In sum, our results suggest that attention is an overlooked barrier to adoption of evidence-based programs. Once decision-makers pay attention (and have some basic information), the immediate transaction costs of implementing the program is of more interest than information about the evidence for benefits and costs of running the program. These results do not imply, that attention and transaction costs are the only relevant factors for adoption of evidence-based programs. Decision-makers may have other reasons to decide whether to adopt or not. These reasons, however, all seem to depend on attention in the first place.

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Appendix

Random Sample of all Public Schools

Table A1: Balance between sample and population schools

	Population	Sample	Difference
School size (#students)	435.2	425.5	-9.7
Students pr. class	20.3	20.2	-0.1
Classes with a subject teacher	0.88	0.88	0.00
GPA	7.83	7.82	-0.01
Students with high well being	0.91	0.91	0.00
Student absenteeism	0.049	0.051	0.002*
Schools	718	368	

Note: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates from bi-variate OLS models. Number of observations may vary due to missing data.

Step 1. Invitation

Figure A1: Invitations

Evidence on student benefits

You are hereby invited to participate in the reading initiative READit for 1st and 2nd grade. READit provides schools and teachers with tools to strengthen collaboration with parents on reading, ensuring that all children have the best conditions for becoming proficient and happy readers.

With READit, students - and their parents - receive a bag containing physical books along with access to an IT platform with e-books. On this platform, parents can also find guidance on how they - regardless of their own reading abilities - can help their children learn to read with a focus on cultivating an enthusiasm for reading and engaging in discussions about the books they read together.

READit builds on experience from previous projects, and is a further development of the reading program READ – together about reading. Several research studies have shown that READ improves students' reading proficiency. The reading proficiency were particularly improved for students whose parents, prior to participation in READ, did not believe that they could help their child become better at reading. In addition, the project had particularly good results among students with a different ethnic background than Danish.

You can learn more about READit and sign up on this page <https://readit.au.dk/registration>. Thanks to support from the Novo Nordisk Foundation, we can offer READit for free to your school. However, since we are unable to provide READit to all schools, you need to log in with your unilogin to complete the registration.

Note: Italics are used here to highlight the difference between the two emails. The original text was not in italics.

Transaction costs

You are hereby invited to participate in the reading initiative READit for 1st and 2nd grade. READit provides schools and teachers with tools to strengthen collaboration with parents on reading, ensuring that all children have the best conditions for becoming proficient and happy readers.

With READit, students - and their parents - receive a bag containing physical books along with access to an IT platform with e-books. On this platform, parents can also find guidance on how they - regardless of their own reading abilities - can help their children learn to read with a focus on cultivating an enthusiasm for reading and engaging in discussions about the books they read together.

READit builds on experience from previous projects, and it is important to us that it is easy for you to start the program. Therefore, READit has been developed to support the work that is already taking place at the school. We make sure that the teachers can easily get answers to their questions, and we, for example, make letter templates available so the individual teacher does not have to spend extra time on preparation.

You can learn more about READit and sign up on this page <https://readit.au.dk/registration>. Thanks to support from the Novo Nordisk Foundation, we can offer READit for free to your school. However, since we are unable to provide READit to all schools, you need to log in with your unilogin to complete the registration.

Table A2: Balance in the invitation phase

	Student benefit email	Transaction costs email	Difference
School size (#students)	440.30	410.34	-29.97
Students pr. class	20.22	20.22	-0.01
Classes with a subject teacher	0.88	0.88	0.00
GPA	7.78	7.85	0.07
Students with high well being	0.92	0.91	-0.01+
Student absenteeism	0.05	0.05	0.00
Schools	185	183	-2

Note: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates from bi-variate OLS models. Number of observations may vary due to missing data.

Table A3: Adoption rate Step 1: Invitation email

	Model I	Model II
Invitation email: Transactional cost	0.013 (0.046)	0.007 (0.045)
School size (#students)	-	-0.0001 (0.0001)
Students pr. class	-	0.014 (0.01)
Classes with a subject teacher	-	0.388 (0.242)
GPA	-	-0.048 (0.046)
Students with high well being	-	0.092 (0.488)
Student absenteeism	-	-2.51 (1.24)
Constant	0.195*** (0.034)	-0.011 (0.586)
Covariates included	No	Yes
Schools	368	368

Note: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates from OLS models with cluster-robust standard errors at the randomization level (school principal). P-values have been adjusted using the Bonferroni correction for multiple hypothesis testing. Observations with missing data is set to sample average and included with missing data indicators (not reported in the table).

Step 2. Reminder

Figure A2: Reminder



Note: The physical reminder letter was consistent with the email reminder (in the column to the right) in both content and structure, including two variations: one specifying the transaction cost and another emphasizing student benefits.

E-mail

In early March, you received an invitation to participate in the reading initiative READit for 1st and 2nd grades. We are now following up on that invitation. Registration is still open, and you still have the opportunity to enroll.

With READit, students—and their parents—receive a bag containing physical books along with access to an IT platform with e-books. On this platform, parents can also find guidance on how they—regardless of their own reading abilities—can help their children learn to read with a focus on cultivating an enthusiasm for reading and engaging in discussions about the books they read together.

[Student benefits version:] READit builds on experience from previous projects, and is a further development of the reading program READ – together about reading. Several research studies have shown that READ improves students' reading proficiency. The reading proficiency were particularly improved for students whose parents, prior to participation in READ, did not believe that they could help their child become better at reading. In addition, the project had particularly good results among students with a different ethnic background than Danish.

[Transaction cost version:] READit builds on experience from previous projects, and it is important to us that it is easy for you to start the program. Therefore, READit has been developed to support the work that is already taking place at the school. We make sure that the teachers can easily get answers to their questions, and we, for example, make letter templates available so the individual teacher does not have to spend extra time on preparation.

You can learn more about READit and sign up on this page <https://readit.au.dk/registration>. Thanks to support from the Novo Nordisk Foundation, we can offer READit for free to your school. However, since we are unable to provide READit to all schools, you need to log in with your unilogin to complete the registration.

Table A4: Balance in the reminder phase

	E-mail reminder	Physical reminder	Difference
School size (#students)	411.51	442.80	31.29
Students pr. class	19.96	20.31	0.34
Classes with subject teacher	0.86	0.88	0.01
GPA	7.82	7.83	0.01
Students with high well being	0.91	0.91	0.00
Student absenteeism	0.05	0.05	0.00
Schools	72	222	.

Note: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates from bi-variate OLS models. Number of observations may vary due to missing data.

Table A5: Adoption rate Step 2: Reminder

	Model I	Model II
Reminder: Physical copy	0.176** (0.053)	0.164** (0.055)
School size (#students)	-	-0.00005 (0.0002)
Students pr. class	-	0.006 (0.012)
Classes with a subject teacher	-	-0.054 (0.289)
GPA	-	-0.015 (0.044)
Students with high well being	-	0.948 (0.517)
Student absenteeism	-	-3.248 (1.661)
Constant	0.153** (0.043)	-0.476 (0.644)
Covariates included	No	Yes
Schools	294	294

Note: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates from OLS models with cluster-robust standard errors at the randomization level (school principal). P-values have been adjusted using the Bonferroni correction for multiple hypothesis testing. Observations with missing data is set to sample average and included with missing data indicators (not reported in the table).

Step 3. Contact

Figure A3 shows various excerpts from the script that was used to conduct the telephone follow-up. As indicated, the script included, among other things, a call introduction, description of the project's content, timeline, etc., as well as a questionnaire in which the conversations were recorded.

Figure A3: Phone Call: Excerpt from phone call script

Introduction:

"I'm calling from Aarhus University. I'm calling because we have sent an invitation to your school to participate in the reading initiative READit."

Project Description:

"In brief, READit is a reading initiative for children in 1st and 2nd grades that supports reading at home, enabling parents to better assist their children in becoming happy and proficient readers. [...]."

Languages:

"The following languages will be available in translation: English, Arabic, Farsi, Pashto, Somali, Turkish, Romanian, Vietnamese, Polish, and Ukrainian. [...]."

Time Line:

"March: The school registers here on this page

May: School and teachers receive information about READit.

August: Teachers receive materials and access to the READit platform. [...]."

Questionnaire in connection with the phone call:

"If it is not convenient right now: May I call back later?" Note the time (remember to add to the calendar). "[...]."

Question 1

"Have you seen the invitation?"

1. Yes

2. No

[If the person has not seen the invitation]: *Question 2a*

"May I send the invitation to you? [...]."

Table A6: Adoption rate Step 3: Contact (both reminder groups)

	Model I	Model II	Model III
Contact: Phone call	0.39*** (0.013)	0.431*** (0.013)	0.403*** (0.008)
Constant	0.000 (0.011)	0.000 (0.011)	0.02** (0.007)
Washout days excluded	No	Yes	Yes
School fixed effects	No	No	Yes
Schools	210	210	210

Note: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates from OLS models. P-values have been adjusted using the Bonferroni correction for multiple hypothesis testing.

Table A7: Adoption rate Step 3: Contact (E-mail reminder group)

	Model I	Model II	Model III
Contact: Phone call	0.449*** (0.025)	0.496*** (0.025)	0.472*** (0.016)
Constant	0.000 (0.022)	0.000 (0.021)	0.017 (0.014)
Washout days excluded	No	Yes	Yes
School fixed effects	No	No	Yes
Schools	61	61	61

Note: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates from OLS models.

Table A8: Adoption rate Step 3: Contact (Physical reminder group)

	Model I	Model II	Model III
Contact: Phone call	0.365*** (0.015)	0.404*** (0.015)	0.377*** (0.010)
Constant	0.000 (0.013)	0.000 (0.013)	0.019* (0.008)
Washout days excluded	No	Yes	Yes
School fixed effects	No	No	Yes
Schools	149	149	149

Note: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates from OLS models.

Table A9: Correlation between assigned caller and likelihood of adoption

Model I	
Caller 2	0.034 (0.07)
Constant	0.466*** (0.047)
Schools	210

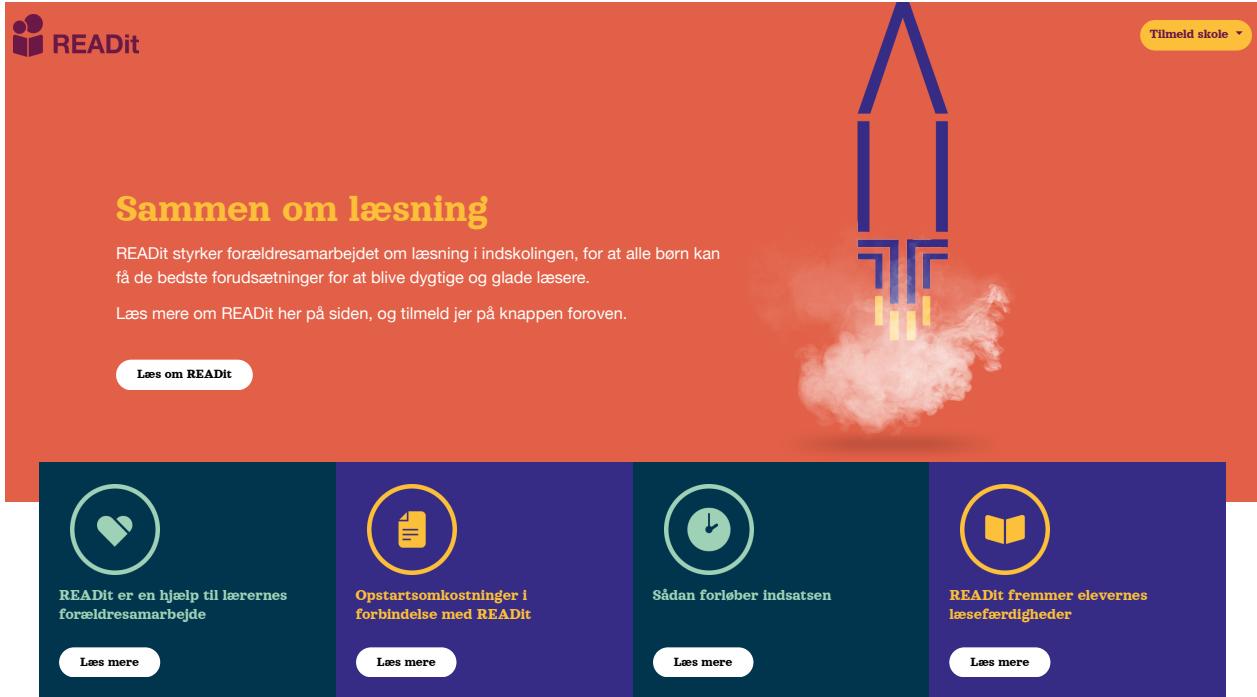
Note: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Two callers, Caller 1 and Caller 2, were assigned to call schools. Caller 1 is the reference category. Estimates from OLS model (robust standard errors in parentheses).

Step 4. Information

Figure A4 shows the website where schools could sign up for the program.

Figure A4: Signup website

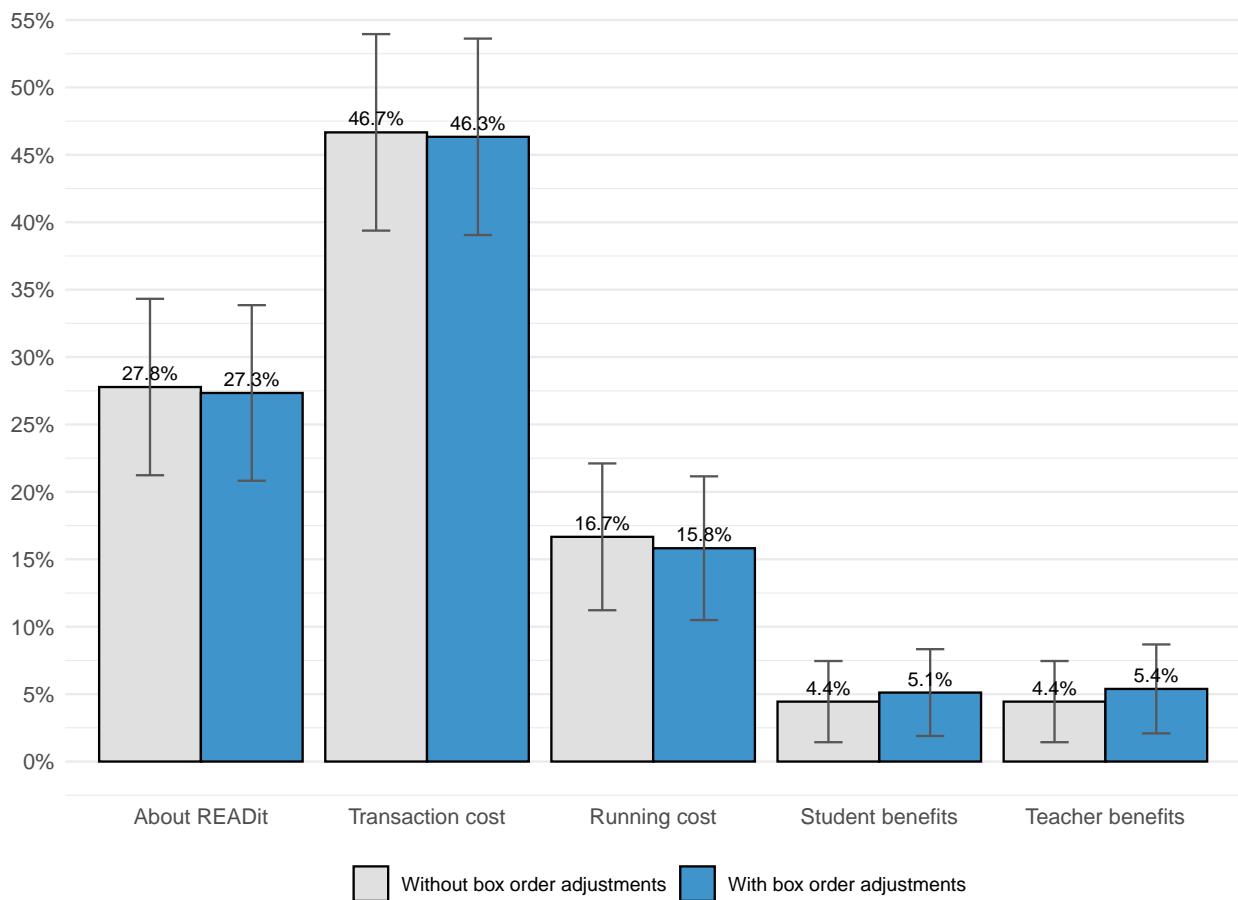


Note: The signup website was in Danish. The four boxes at the bottom presents information about (i) transaction costs, (ii) evidence on student benefits, (iii) running costs (during the program), and (iv) benefits for the teachers. The order of the boxes was randomized. Above the boxes, general information about the program was provided. People at the website could click on any of the white buttons. We registered these clicks.

Figure A5 includes schools that clicked on at least one of the information boxes presented in Figure A4. The bars in Figure A5 represent the share of schools clicking first on the designated box. To account for any order effects of the boxes' placement on the website, the order of box presentation on the website was randomized when presented to schools. Given that one of the five boxes (the box with general information about READit) was always placed on the top of the website, randomization of the other four boxes would in expectation produce a uniform distribution, with each of the four boxes appearing first in 25% of the presentations. In our specific randomization, though, the transaction cost information box appeared first in 31% of the presentations, 27% for the running cost box, while both the student benefits and teacher benefits boxes appeared first in 21% of the presentations.

To account for this imbalance, we have calculated the share of first clicks in each of the versions of the presentations and weighted each presentation equally. The share of first clicks after this adjustment are presented in blue bars. The not-adjusted share of first clicks are presented in the grey bars (identical to Figure 3). Accounting for the unbalanced distribution in the ordering does not substantially alter the results.

Figure A5: First Click on Web Page Information Boxes adjusted for box order



Note: Distribution of first clicks on website information boxes (schools=180). Fractions are presented with 95% confidence intervals, calculated as $\pi \pm 1.96 \frac{\sigma_\pi}{\sqrt{n}}$, where π represents the fraction of first clicks on the box, and $\frac{\sigma_\pi}{\sqrt{n}}$ denotes the estimated standard error. Grey bars without box order adjustments are identical to Figure 3.

Replication Study

Invitation Email

Dear school principal and reading coordinator at [school name],

You are hereby invited to participate in READit – Together in Reading! READit will take place during the spring of 2024 and is a reading initiative for 1st and 2nd grade that supports reading at home.

Through READit, parents will gain knowledge and practical guidance on how to create enjoyable reading moments with their children through dialogic reading. The initiative aims to help make reading a good habit and assist children in becoming happy and skilled readers.

What do the students receive?

With READit, the students receive a bag containing four books, a reading log, a bookmark, and a parent guide, as well as access to our website: the READit portal. The READit portal includes e-books and advice on how parents—regardless of their own reading skills—can support their child’s reading through dialogue about the books they read together. The students will be able to keep the physical materials afterward.

What does it take to participate?

Thanks to support from Novo Nordisk Foundation, we are able to offer READit to your school for free. It is important to us that working with this initiative is easy for you, which is why we have developed READit to support the work that is already taking place at the school.

What does the research say?

READit is a research project that builds upon READ – Together in Reading. Several research studies have shown that READ improves students’ reading skills, particularly among students with a non-Danish ethnic background.

You can read more about the project and sign up here: [LINK]. You can log in to the website with Unilogin. We kindly request your registration no later than April 14, 2023.

On behalf of the research group behind READit, [Researchers] TrygFonden’s Centre for Child Research

Department of Political Science
Aarhus University

P.S. READit has been developed in collaboration with our advisory group, consisting of the Danish Union of Teachers, the Association of School Leaders, School and Parents, the Ministry of Education, Aarhus Municipality, and Norddjurs Municipality.

Reminder Email (for control group)

Dear school principal and reading coordinator at [school name],

At the end of March, you received an invitation to participate in the READit reading initiative for 1st and 2nd grades. We are following up on that invitation. Registration is still open, so you still have the opportunity to sign up.

READit will take place during the spring of 2024 and is a reading initiative for 1st and 2nd grades that supports reading at home. Through READit, parents gain knowledge and practical guidance on how to create enjoyable reading moments with their children through dialogic reading. The initiative aims to help make reading a positive habit and assist children in becoming happy and proficient readers.

What do the students receive? With READit, the students receive a bag containing four books, a reading log, a bookmark, and a parent guide, as well as access to our website: the READit portal. The READit portal includes e-books and advice on how parents—regardless of their own reading skills—can support their child's reading through dialogue about the books they read together. The students will be able to keep the physical materials afterward.

What does it take to participate? Thanks to support from Novo Nordisk Foundation, we are able to offer READit to your school for free. It is important to us that working with this initiative is easy for you, which is why we have developed READit to support the work that is already taking place at the school.

What does the research say? READit is a research project that builds upon READ – Together in Reading. Several research studies have shown that READ improves students' reading skills, particularly among students with a non-Danish ethnic background.

You can read more about the project and sign up here: [LINK]. You can log in to the website with Unilogin. We kindly request your registration no later than May 5, 2023.

On behalf of the research group behind READit, [Researchers] TrygFonden's Centre for Child Research
Department of Political Science
Aarhus University

P.S. READit has been developed in collaboration with our advisory group, consisting of the Danish Union of Teachers, the Association of School Leaders, School and Parents, the Ministry of Education, Aarhus Municipality, and Norddjurs Municipality.

Balance between treatment and control schools

Table A10: Balance in the invitation phase

	Control	Treatment	Difference
School size (#students)	434.1	443.8	9.7
Students pr. class	20.03	20.19	0.15
Classes with a subject teacher	0.87	0.86	-0.003
GPA	7.12	7.11	-0.02
Students with high well-being	0.87	0.88	0.006
Student absenteeism	0.07	0.07	0.000
Schools	223	219	-4

Note: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates from bi-variate OLS models. Number of observations may vary due to missing data.

Treatment effect on adoption

Table A11: Adoption rate: Trial 2

	Model I	Model II
Implementation Support	0.306*** (0.046)	0.301*** (0.046)
School size (#students)	-	0.00004 (0.0002)
Students pr. class	-	0.006 (0.01)
Classes with a subject teacher	-	-0.227 (0.327)
GPA	-	-0.081 (0.044)
Students with high well being	-	0.882* (0.428)
Student absenteeism	-	3.636* (1.758)
Constant	0.256*** (0.03)	-0.166 (0.536)
Covariates included	No	Yes
Schools	442	442

Note: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates from OLS models with cluster-robust standard errors at institution group level. Observations with missing data is set to sample average and included with missing data indicators (not reported in the table).

Supplementary Analysis of Randomization Bias

Table A12: Randomization bias in Schools Receiving Information about Student Benefits pronounced for Immigrant Students

	Predicted Effect
Early adoption (Reference category)	-
Late adoption	-0.00198 (0.0028)
No adoption	0.00069 (0.0031)
Constant	0.128*** (0.002)
Observations	185
Adjusted R^2	-0.004

Note: The constant represents the standardized predicted effect with no interaction effects. + p < .1, * p < .05, ** p < .01, *** p < .001.

Additional preregistered analyses

Table A13: Website Attention Rate Step 1: Invitation email

	Model I	Model II
Invitation email: Transactional cost	0.019 (0.046)	0.019 (0.047)
School size (#students)	-	-0.00002 (0.0002)
Students pr. class	-	0.011 (0.011)
Classes with a subject teacher	-	0.415 (0.291)
GPA	-	-0.033 (0.043)
Students with high well being	-	-0.027 (0.565)
Student absenteeism	-	-2.593 (1.675)
Constant	0.259*** (0.033)	0.076 (0.658)
Covariates included	No	Yes
Schools	368	368

Note: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates from OLS models (robust standard errors in parentheses). Observations with missing data is set to sample average and included with missing data indicators (not reported in the table).

Table A14: Website Attention Rate Step 2: Reminder

	Model I	Model II
Reminder: Physical copy	0.141* (0.066)	0.117+ (0.067)
School size (#students)	-	0.00007 (0.0002)
Students pr. class	-	0.001 (0.013)
Classes with a subject teacher	-	0.025 (0.351)
GPA	-	0.006 (0.052)
Students with high well being	-	0.201 (0.684)
Student absenteeism	-	-3.093 (1.935)
Constant	0.292*** (0.057)	0.155 (0.792)
Covariates included	No	Yes
Schools	294	294

Note: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates from OLS models (robust standard errors in parentheses). Observations with missing data is set to sample average and included with missing data indicators (not reported in the table).

Table A15: Web-page Attention rate Step 3: Contact (both reminder groups)

	Model I	Model II	Model III
Contact: Phone call	0.265*** (0.014)	0.293*** (0.014)	0.339*** (0.008)
Constant	0.209*** (0.012)	0.209*** (0.012)	0.176*** (0.007)
Washout days excluded	No	Yes	Yes
School fixed effects	No	No	Yes
Schools	210	210	210

Note: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates from OLS models (robust standard errors in parentheses).

Table A16: Web-page Attention rate Step 3: Contact (E-mail reminder group)

	Model I	Model II	Model III
Contact: Phone call	0.284*** (0.027)	0.314*** (0.028)	0.444*** (0.016)
Constant	0.245*** (0.024)	0.245*** (0.024)	0.149*** (0.013)
Washout days excluded	No	Yes	Yes
School fixed effects	No	No	Yes
Schools	61	61	61

Note: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates from OLS models (robust standard errors in parentheses).

Table A17: Web-page Attention rate Step 3: Contact (Physical reminder group)

	Model I	Model II	Model III
Contact: Phone call	0.255*** (0.017)	0.283*** (0.017)	0.299*** (0.009)
Constant	0.196*** (0.014)	0.196*** (0.014)	0.184*** (0.008)
Washout days excluded	No	Yes	Yes
School fixed effects	No	No	Yes
Schools	149	149	149

Note: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates from OLS models (robust standard errors in parentheses).

Table A18: Adoption rate: Interaction model

	Model I	Model II	Model III
Invitation email: Transaction cost	0.022 (0.052)	0.007 (0.104)	0.03 (0.105)
Reminder: Physical copy	0.131* (0.06)	0.122 (0.082)	0.127 (0.082)
READit Approach:			
Transaction cost*Physical copy	-	0.02 (0.12)	-0.003 (0.121)
School size (#students)	-	-	-0.0001 (0.0002)
Students pr. class	-	-	0.014 (0.012)
Classes with a subject teacher	-	-	0.213 (0.319)
GPA	-	-	-0.044 (0.047)
Students with high well being	-	-	0.807 (0.619)
Student absenteeism	-	-	-4.757* (1.84)
Constant	0.32*** (0.057)	0.327*** (0.071)	-0.291 (0.724)
Covariates included	No	No	Yes
Schools	368	368	368

Note: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Estimates from OLS models (robust standard errors in parentheses).

Table A19: Adoption rate: Multiple Hypothesis Testing

	<i>p</i> -values				
	DI	Unadjusted	List et al.	Bonferroni	Holm
Student benefit + email reminder vs.					
- Transaction cost + email reminder	.007	.943	.943	1	.943
- Student benefit + physical copy	.121	.134	.227	.403	.269
- Transaction cost + physical copy	.149	.069	.154	.207	.207

Note: DI refers to difference in means. Estimates from List, Shaikh, and Xu (2019) using the mhtexp package in Stata.