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EdWorkingPaper No. 26-1639

Increasing Take-Up of Literacy Tutoring: Evidence from a Randomized Experiment

Misty Gallo
University of Arkansas

Patrick J. Wolf
University of Arkansas

Harry A. Patrinos
University of Arkansas

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VERSION: November 2026

Suggested citation: Gallo, Misty, Patrick J. Wolf, and Harry A. Patrinos. (2026). Increasing Take-Up of Literacy Tutoring: Evidence from a Randomized Experiment. (EdWorkingPaper: 26-1639). Retrieved from Annenberg Institute at Brown University: https://doi.org/10.26300/hx70-rn17

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Misty Gallo
University of Arkansas

Patrick J. Wolf
University of Arkansas

Harry A. Patrinos
University of Arkansas

October 8, 2026

Abstract

Government programs often experience low take-up, limiting their potential positive impact. We examine whether reducing registration frictions can increase participation in a state-financed literacy intervention program. In a randomized field experiment, families received either encouragement and a simplified enrollment process, or these interventions plus text message nudges, relative to a business-as-usual control group. We find that encouragement and simplified registration increased take-up by more than 10 percentage points, while a fuller treatment of encouragement, simplified enrollment, and text nudges increased take-up by 6 percentage points. We also estimate the impact of these treatments on whether families used funding for tutoring and how much they used. We find positive intent-to-treat effects with the fuller treatment showing a strong impact on use and amount of funding used. These low-cost interventions offer a promising strategy for increasing participation in government programs.

JEL: I21; D91; C93

Keywords: literacy, tutoring, take-up, nudge, behavioral economics, encouragement, regulatory friction, parental engagement, choice, implementation

Acknowledgments: We thank Amy Counts and Amy Hindsman of the Arkansas Department of Education for helping us obtain the data needed to conduct this experiment. We appreciate the work of the five district liaisons in implementing the experiment under our direction. We are deeply grateful to Denise Airola for her assistance with data. We thank Accelerate for sponsoring the study and Matthew Steinberg and Tia Kim for their helpful guidance. Any remaining errors are the responsibility of the authors alone. The experiment reported in this study is listed in the OSF Registry (10.17605/OSF.IO/EV2RW).

1. Introduction

Low take-up for social interventions and services remains a pervasive issue plaguing policymakers and implementers. The tendency for social interventions to suffer from low participation has persisted through time and context (OECD, 2024). Because social interventions often aim to mitigate inequalities or barriers affecting the public, policymakers and researchers seek ways to reduce administrative burdens that may discourage vulnerable populations from accessing beneficial programs (Herd & Moynihan, 2018; Stewart & Wolf, 2014). Information asymmetry in navigating the take-up process itself can stymie the benefits of public services (Bell et al., 2022; Burden et al., 2012). This problem is not new; of the 10 million US children who lacked health insurance in 1996, 4.7 million were eligible but unenrolled in Medicaid, prompting the government to engage in a $500 million public information campaign to promote participation (Aizer, 2003). While low take-up is not new, without meaningful efforts to reverse the trend, it may undermine the policy interventions needed to assist the public.

Policymakers want to deliver public intervention benefits to the individuals who need them most. Public service access often comes with high administrative burdens, including learning costs, compliance costs, and psychological costs (Herd & Moynihan, 2018). According to a recently published book, *The Time Tax* (Lowrey, 2026), Americans spend over 12 billion hours completing federal paperwork, often to obtain needed services. This administrative complexity can exacerbate inequality, as complex registration processes and insufficient program information deter participation in social services, especially among the most disadvantaged members of the eligible population; however, intentional design could mitigate this difficulty (Bettinger et al., 2009; Herd et al., 2023). Emergent research focuses on identifying and alleviating administrative burdens that limit access to necessary interventions (Lasky-Fink & Linos, 2024). Barriers that may impede take-up of government programs include learning,

psychological, and compliance costs (Barnes, 2021; Herd & Moynihan, 2018). Reducing the compliance burden by simplifying the registration process may help ease the friction to take-up.

Literacy initiatives have become prominent policy interventions (van der Weijden et al., 2025). Educators and researchers alike emphasize that students who fail to master basic literacy skills by third grade are more apt to struggle in all subjects throughout school (Kortecamp & Peters, 2024). In fact, students who start fourth grade without reading proficiency are four times less likely to graduate from high school than peers with stronger early literacy skills (Fiester, 2013; Hernandez, 2011). Today, the nation's students are showing troubling deficits in the literacy skills needed for adequate reading. In 2024, 40% of U.S. fourth grade students performed below the NAEP Basic level in reading, while 81% of economically disadvantaged fourth graders failed to reach NAEP Proficient (NCES, 2025). Literacy skills have worsened since the COVID-19 pandemic, with early grades showing the troubling deficits most clearly (Georgiou & Parrila, 2023; National Assessment Governing Board, 2025; Shakeel, Gallo, & Wolf, 2026).

Arkansas, like many states, suffers from persistent literacy deficits. In 2023, only 35% of Arkansas students read at grade level (Arkansas Department of Education, 2023). As part of the Arkansas LEARNS[1] Act of 2023, beginning in the 2025-26 school year, third-grade students are expected to meet literacy proficiency standards to be promoted to fourth grade. To help students meet this new requirement, Arkansas incorporated targeted evidence-based literacy tutoring programs (Arkansas Department of Education (ADE), 2025).

The evidence supporting the effect of high-impact tutoring on student learning is strong. According to Nickow et al. (2020) in a meta-analysis of studies concerned with school-based

[1] Literacy, Empowerment, Accountability, Readiness, Networking, and School Safety (LEARNS) is an education act signed into law in 2023 that changed multiple aspects of education in Arkansas. https://learns.ade.arkansas.gov/

tutoring, defined as one-on-one or small group learning interventions, tutoring yields substantial positive impacts on learning outcomes, with an overall pooled effect size estimate of 0.37 standard deviations (SD).

Tutoring targeting low-SES students often shows low take-up and is plagued by attendance lapses, largely attributed to the compliance and transportation burdens it places on families (Kraft & Falken, 2021). Often, the students most in need of tutoring are the least likely to use the program (Heinrich et al., 2010; Reinheimer & McKenzie, 2014). The problem with tutoring by student choice is the assumption that struggling students will recognize the need for help, understand how to find it, and try to get it. Avoiding the prescribed treatment in this instance could lead to long-term negative economic, social, and mobility impacts (DeBaun & Roc, 2013).

In 2024, Arkansas launched the Literacy Tutoring Grant Program (LTGP) to provide outside-the-school (OTS) literacy tutoring to struggling readers to help them achieve reading proficiency and thereby avoid third-grade retention. Students in kindergarten through third grade were eligible for the grant based on their low score on a literacy screener. Their parents were notified of their eligibility through a generic letter. Less than 5% of eligible students registered for the $500 grants, and take-up was uneven across school districts, leaving many vulnerable students unserved. In 2025-26, the second year of the grant's implementation, the state increased each grant to $1500, hoping to attract more students and increase usage.

Because students cannot be compelled to participate in OTS tutoring, policymakers must design programs that are attractive to families. Practitioners should make registration as easy as possible to maximize take-up (Robinson et al., 2025). This paper presents results from a randomized experiment to boost take-up rates of out-of-school tutoring programs by targeting

common barriers to participation. We conducted a field experiment in Arkansas using an encouragement-and-simplified-registration design, with some parents also nudged, to increase tutoring take-up in the second year of the Arkansas LTGP. This study demonstrates the effectiveness of simple, inexpensive methods to increase participation in government intervention programs for families.

We draw on theories from economics, behavioral economics, and sociology to inform our study. Traditional economic theory views individual decisions as reflecting assessments of expected benefits and costs (Arrow, 1974). When given a choice, such as whether to enroll in a government program, individuals weigh expected benefits against expected costs. If the benefits seem likely to outweigh the costs, they take the action rationally. To this standard cost-benefit calculus, behavioral economists add the view that individuals are less rational actors than they are constrained consumers. These constraints include cognitive and attentional limits, as well as external friction barriers, which may lead consumers to make choices driven by irrational impulses (Bhargava & Manoli, 2015; Mullainathan & Thaler, 2001; Stevens, 2015). In the case of the LTGP, the expected benefits include improvements in literacy skills sufficient to avoid third-grade retention and increase school success. The expected costs to the parent are the burden of completing the application, of selecting an approved tutor, and of ensuring that the tutoring service is delivered effectively to the child. People with relatively low social capital who might especially benefit from such programs may be at high risk of reluctance to enroll (Soss, 1999; Stewart & Wolf, 2014). Failure to navigate these administrative burdens may signal lapses in program design rather than a disinterest in participating in the service.

Administrative burden is an "individual's experience of policy implementation as onerous*"* (Burden et al., 2012*).* A growing body of research examines how to reduce

administrative burdens to boost social service take-up (Nielson & Nielson, 2024). A recent meta-analysis by Bendtsen (2026) identifies the impact of lessening administrative burden on both initial application for, and ultimate receipt of, social benefits. Its main findings indicate that public services are accessed more equitably when citizens receive help in overcoming administrative burdens. Such assistance has stronger effects in the application phase than in the take-up phase (Bendtsen, 2026). The analysis concludes that the most effective interventions reduce compliance burdens.

Previous literature offers guidance on how school districts can improve take-up of educational interventions such as tutoring for struggling readers (Deneault, 2023; Gray & Hoyt, 2020; Oreopoulos & Ford, 2019). Two approaches that boost take-up – mandating participation and delivering the tutoring in school during the school day – are unavailable to some districts. For the LTGP in Arkansas, the grant funds voluntary tutoring that must occur outside school hours. Three areas of research are relevant to this experiment: the effect of explicitly describing the benefits of individual tutoring as part of the registration process; the effect of personalized and simplified registration assistance; and the effect of text-based nudges.

Information campaigns promoting the intervention's benefits can boost take-up, though findings are highly contingent. Program information alone is less effective than when paired with encouragement and outreach directed to families potentially promoting both knowledge and participation (Goodman, 2016; Gurantz et al., 2020; Zhang, 2025). Simplification of registration coupled with an information campaign showed that *information* without *assistance* in the registration process had no significant effect (Barr, Bird, & Castleman, 2017; Barr & Turner, 2018; Bettinger et al., 2009; Hoxby & Turner, 2013).

Behavioral economics is less concerned with changing the mind of unwilling consumers

than with designing choices in ways that are likely to appeal to the consumer and in removing friction which may inhibit these choices. While prior research has focused primarily on how administrative burdens may limit access to public services, less work has examined how those burdens may also reduce a program's effectiveness and efficiency (Heinrich, 2015). Limiting unnecessary administrative burdens may help circumvent negative consequences for society. Levitt et al. (2016) found positive results after incorporating behavioral economics cues, specifically for students who may be slow to adopt interventions, particularly in mitigating three potential disruptors to the enrollment process: the effects of cognitive unease, procrastination, and ego depletion (Fox et al., 2019; Levitt et al., 2016). To encourage literacy tutoring take-up, it is important to limit the cognitive load and time needed for registration.

Text-based nudges have been used with varying levels of success, but to date they have not shown a disincentivizing effect. Text-based nudges are relatively easy to implement and are a low-cost way to target identified student populations. Nudging has been shown to increase take-up of public services in evaluations of employment services, college completion, and financial aid services (Hopkins & Dorion, 2024; Page et al., 2022). Nudges accompanied by information and relationally grounded (that is, from a known and trusted sender) promote student success, particularly for at-risk students (Debrah & Timmins, 2026). The success of text-based nudges has been linked to a theory of mattering; students are more likely to engage when they feel relationally connected to the intervention (Prilleltensky et al., 2020). Research has shown particular success when nudges are more personalized and less automated (Yeung & Nguyen-Hoang, 2020). Of particular interest to us, a recent study evaluating text nudges directed at parents of primary school students in Kenya showed success in both outcomes and parental engagement in school choice (Gray-Lobe et al., 2026). Text-based nudges do not force

participation in programs, but they do serve as reminders to encourage participation. A recent randomized field trial measuring the effectiveness of nudges in increasing participation in an Earned Income Tax Credit (EITC) showed null effects. However, the authors hypothesized that the effect may have been nullified by the difficulty of the encouraged task (Linos et al., 2022). The authors further speculated that nudges to increase take-up of complicated services should be accompanied by higher-touch interventions.

Studies indicate that both extra information and nudges improve program take-up in some, but not all, circumstances (Burnett & Kroeger, 2020; Castleman & Page, 2016; Page et al., 2022). Nudges show more promise when aimed at parents than at students (Lynch, 2024). However, it is unclear whether extra information and nudges, when implemented simultaneously, will boost take-up to desired levels.

To address lagging take-up in the Arkansas literacy tutoring grant context, we designed a field experiment in which we vary the registration incentives for a subset of families in randomly selected groups and evaluate whether this intervention increases registration. Appendix A describes the partnership with ADE and the districts that supported the implementation of this experiment. Our research questions are: *Can encouraging participation while limiting friction from administrative processes increase take-up in literacy interventions? Does the effect of encouragement persist through registration to the next step of pursuing a literacy tutor?* We evaluate three behavioral mechanisms in combination that may improve program take-up: (1) increasing awareness of program efficacy; (2) reducing administrative burdens through simplified registration; and (3) encouraging participation through text-based nudges. We offer the following hypotheses:

1. Increasing awareness of program efficacy while simplifying the registration process

will encourage tutoring registration across the eligible student population.

2. Sending text nudges to families will further encourage tutoring registration across the eligible student population.
3. Families most adversely affected by administrative burden (i.e., students experiencing poverty, English Language Learners, and non-white students) will see the largest positive effect from the aid offered by the experimental interventions.
4. The positive impacts of encouraging participation and reducing administrative burden on registration rates will endure through downstream measures of program take-up, use, and dosage measured by total dollars spent out of the grant.

The randomized experiment tested hypotheses 1-3 by providing all treated students with both an encouragement design and a simplified enrollment design, a controlled and observable registration process, and nudges to one randomized subgroup of treated students. We test hypothesis 4 through an instrumental variable design, instrumenting registration via treatment assignment to find the local average treatment effect (LATE) of grant funding used to hire literacy tutors. We present causal impacts observed for hypotheses 1 and 2. We present descriptive observations for hypothesis 3 and causal impacts for hypothesis 4.

The results from the experiment indicate that the encouragement treatments successfully increased program take-up rates. Both treatment groups demonstrated statistically significant improvements of comparable magnitude on take-up rates, with Treatment 1 increasing program registration by 10.9 percentage points ($p < 0.01$) and Treatment 2 boosting registration by 6.1 percentage points ($p < 0.05$) relative to the control group. Treatment 1 increased registration by 37.1%, and Treatment 2 increased it by 20.7% relative to the control group registration rate of 29%. In focus group conversations with both district personnel

and with families, both sets of stakeholders noted that the eased registration made a positive difference in the experience. Additionally, causal evidence shows that Treatment 2 increased both the likelihood of fund use and the amount of fund use. Applying the framework from Kraft (2021), we conclude that this intervention has a large effect on tutoring take-up and a promising effect on subsequent use of tutoring funds. Importantly, the intervention is easy to scale at minimal cost. We contribute to the literature by demonstrating that a relatively easy-to-implement and low-cost intervention can meaningfully induce families to participate in a literacy intervention, highlighting the importance of program design and ease of adoption.

2. Experimental Strategy

2.1 Experiment Design

The study design is a three-armed, randomized field experiment within strata (Bækgaard et al., 2015). The 38 strata consist of district, prioritization category, and elementary school. Five districts across the state volunteered to participate in the experiment at the ADE's request. The participating districts showed heterogeneity in take-up in the previous year ranging from 9% to 92% registration rates. Appendix Table A3 provides district characteristics including previous year tutoring take-up as well as state contextual rates for comparison.

2.2 Data

The ADE determined which students were eligible to receive the tutoring grant based on baseline scores on a literacy assessment given in the fall of 2025. The 1,200 students deemed eligible in the five partner districts included 412 Priority 1 students and 788 Priority 2 students. The 412 Priority 1 students were third graders classified as at risk of failing to

achieve proficiency on the reading component of the spring 2026 ATLAS exam[2] and therefore of being held back from grade promotion. The 788 Priority 2 students were those in kindergarten through second grade who were classified as at risk based on the same screening procedure as the third-grade students. The fall literacy screening test scores serve as baseline scores for estimating the outcome scores on the spring ATLAS exam. The screener had different highest available scores across the grades. We standardized each score by grade before using the baseline scores in our regression models. Priority 1 students were enrolled in 18 different schools in the five districts. Priority 2 students were enrolled in 20 different schools, with two additional schools joining the original 18.

Within each stratum, we randomized eligible students into one of three conditions. The Control context provided families with substantial recruitment efforts to boost take-up, including help from the non-profit organization Learning Heroes (Appendix B). The intervention offered by Learning Heroes was not randomized and was distinct from the interventions being evaluated by the research team. The research team acted independently from the Learning Heroes team. We describe the technical processes for randomization and form generation in Appendix C. The control condition was business-as-usual, consisting of a generic eligibility letter designed by the ADE (Appendix D) and a standard set of student recruitment activities coordinated by each district. The Treatment 1 condition included all recruitment activities in the business-as-usual context but added a more detailed and encouraging notification letter that highlighted the effectiveness of literacy tutoring and changed the default phrasing from "your child is eligible" to "please confirm your child's participation in the program" (Appendix E). The Treatment 1

[2] The statewide criterion referenced exam in Arkansas schools, ATLAS (Arkansas Teaching, Learning & Assessment System), is administered in the fall as a benchmark and in the spring as a summative measurement https://atlasportal.org/index.html

condition also included the pre-population of a customized webform with the eligible student's information so that parents only needed to go to the linked webform, review the information, confirm their email address, and press the "confirm enrollment" button. Inaction served to decline enrollment. The Treatment 2 condition included all elements of the business-as-usual and Treatment 1 but added up to three follow-up text nudges (Appendix F), one week apart in most cases, to parents who had not yet registered their student. Once students registered, their names were removed from the text nudge list.

The three-armed randomization of students within each of the 38 strata yielded 114 experimental subgroups: 38 Control, 38 Treatment 1, and 38 Treatment 2. The randomization assignments within each stratum and their aggregation are presented in Appendix Table A1. Across all strata, 402 students were assigned to the control condition, 399 to Treatment 1, and 399 to Treatment 2 (Appendix Table A1).

Findings from the quantitative element of the take-up rate study were supplemented with information from the qualitative component involving focus groups. The full text of the questions posed during focus groups is available in Appendix G. The team succeeded in scheduling focus groups in all districts except District B, although our team subsequently had a reflective, informal call with District B leadership near the time of the ATLAS test administration. We held focus groups separately with district personnel and parents of eligible students, in person, in Districts A and E. We held virtual sessions with district personnel in Districts C and D. School personnel included teachers, district administrators, interventionists, and special education facilitators. In total, 42 school personnel and 9 parents participated in the focus groups.

2.3 Baseline Balance Test

The ADE data included student background characteristics and baseline test scores. Random assignment produced experimental groups that were generally well balanced on observed baseline characteristics (Table A2). Of 33 tests across the full sample and the two priority groups, three were statistically significant at the 10 percent level, approximately the number expected by chance. More importantly, a joint test of baseline covariates, controlling for randomization strata fixed effects, does not reject balance across experimental groups [(F=1.44), (p = 0.1582)].

The frequencies in Appendix Table 2 reveal some key features of the student population eligible for literacy intervention and attending public school in the five volunteer districts. Nearly all program-eligible students are also Free/Reduced Lunch eligible. Almost one-third of them qualify for special education services, and an additional 6-7% qualify for Section 504 services due to one or more diagnosed disabilities.[3] We operationalize both 504 and special education status in our analysis. A slight majority of the eligible students are male. About half are African American, with less than one-third white and smaller proportions Hispanic and Asian/Pacific Islander. About 6% are immigrants and 12-13% are English learners. A little over 10% qualify for special services reserved for immigrants and English learners under Title III of the Elementary and Secondary Education Act. Less than 1% are in foster care, and 4% are homeless. We include the statewide averages of these demographic conditions in Appendix Table A3 as a comparison.

[3] As defined in the Arkansas Statewide Handbook, student with a disability refers to a student who qualifies under the Individuals with Disabilities in Education Act (IDEA) for one of 13 qualifying categories. Section 504 status refers to the section of the federal Rehabilitation Act of 1973 that provides an adjacent but distinct measure of student disability to the Individuals with Disabilities Education Act from which we derive our special education status measure. English Language Learner student refers to a student with limited English proficiency, or indicates English is not the student's first language or the language spoken at home. FRL status is assigned to students whose family income is at or below 130% of the federal poverty level (free) or between 130 and 185% of the federal poverty level (reduced).

2.4 Implementing the Study

For our experiment, the five partner districts identified local employee liaisons to facilitate communication with the research team for study implementation. Employee liaisons were district employees who did not serve as instructors of record for students. The research design consisted of a randomized controlled trial of modifications to the registration process and follow-up focus groups with each district to understand the process from the perspective of both parents and school personnel. With the exception of district liaisons, partner districts were not informed of students' assigned experimental conditions. The liaisons in the partner districts were given PDF links to letters informing parents that their student was eligible for a $1,500 literacy tutoring grant.

The registration process began on December 1, 2025, with the Arkansas Department of Education (ADE) identifying the 1,200 students eligible for the program in the partner districts. The research team randomized the eligible students into three conditions: Control, Treatment 1, and Treatment 2. The Control letter included a link and a QR code to the registration form, with all necessary information to register provided in the text of the letter. Under business-as-usual, Control parents had to enter the required information about their child manually. Treatment letters included more parent-friendly language that informed parents of the benefits of the tutoring program. Like the Control letters, the Treatment letters included both a link and a QR code to the registration form, but the treatment registration forms were *prepopulated* with all the necessary information about each student. Treatment parents were merely prompted to confirm the accuracy of that information and then click "submit." Because of the large population of Spanish speakers in the Arkansas public school system (over 8% of the public-school students),

registration forms were available in both English and Spanish (Bureau of Legislative Research, 2022). Letters were also available in both English and Spanish, and districts were able to print the letters and distribute hard copies or send them through email. Priority 1 students were eligible to receive their letters on December 1, and Priority 2 students were eligible to receive theirs the following week. The research team monitored the registration process, communicating new student registrations to the ADE on a weekly basis. Upon registration, the ADE verified eligibility and emailed the families with instructions for selecting a tutor. The research team monitored all technical difficulties that districts had throughout the registration process. The students assigned to Treatment 2 were subsequently sent text nudges by their school district liaison encouraging them to register for the tutoring grant. Text messages were sent to Priority 1 students on December 8, December 15, and December 19, 2025, and to Priority 2 students on December 15, December 19, 2025, and January 12, 2026. The research team provided the liaisons with the exact content and timing of the text nudges (Appendix F). Liaisons had insight into the list of students who received text nudges, but liaisons did not have direct contact with students.

2.5 Outcomes

We were interested in measuring impact on two outcomes: the rates of registration for the literacy tutoring grant and the grant funding used after registration. Funding use would indicate that students not only registered for the grant but took the additional step of attaining a tutor. It is not certain that increased registrations would lead to increased program use, however, as post-registration regulatory friction can produce substantial client attrition between program registration and use (Barnes, 2021).

While we could only control the information and administrative burden different

experimental groups of parents received prior to registering for the program, we hypothesized that easing the path through the gateway of registration would subsequently lead to greater use of the program by students in the treatment groups. Again applying the Kraft (2021) framework, a short-term and low effort response (such as to register for the tutoring grant) is expected to have a larger effect size than an action that required a sequence of decisions and sustained effort (such as selecting a tutor). We measure the treatments' effects on both. Because the experimental interventions were both low-cost and easy to implement, understanding the effect of the encouragement treatments on both program take-up and program use allows the authorities to intervene in a cost-effective way that is easily scalable.

2.6 Empirical Strategy

Randomized controlled trials are the preferred method for estimating the causal impact of educational interventions, but they are not always feasible. We could not restrict the tutoring program only to a random sample of eligible students, nor would that have been ethical. While we were unable to randomize the offer of tutoring, we *were* able to randomize the encouragement to register. Randomized encouragement designs are widely used when researchers cannot directly assign participation but can randomize encouragement to participate (Angrist et al., 1996; Angrist & Imbens, 1994, 1995; Angrist & Pischke, 2009; Imbens, 2014; Imbens & Rubin, 2015; Paloyo, 2016; Powers & Swinton, 1984; West et al., 2008). Our randomized encouragement design allowed us to estimate the causal effect of encouragement, combined with easing the registration burden, on tutoring take-up and use while preserving universal access to tutoring for at-risk students. All eligible students remained free to enroll in tutoring, but families were randomly assigned to receive additional encouragement through enhanced information, simplified registration, and, for one treatment group, text reminders.

This paper estimates the causal effect of two different combinations of encouragement strategies on tutoring registration as our **primary specification**:

$$R_i = \beta_0 + \pi_1 Z_{1i} + \pi_2 Z_{2i} + \lambda_s + \nu_i \quad (1)$$

where $R_i$is tutoring take-up measured as registration for the program, Z_{1i} is randomized encouragement including the booster letter and ease of registration, Z_{2i} is randomized encouragement including the booster letter and ease of registration plus text nudges, and $\lambda_s$denotes randomization-strata fixed effects. As a robustness check, we include X_i, a vector of student baseline characteristics.

We subsequently evaluate the full effect of assignment. To serve as a comparison, we estimate the **naive** model:

$$Y_{is} = \beta + \beta R_{is} + \lambda_s + \varepsilon_{is} \quad (2)$$

where Y_{is} is funding use or amount used. R is not randomly assigned, so $\beta$is an association, not a causal estimate.

The ITT (intention-to-treat) analysis uses random assignment to the encouragement treatments, regardless of whether students subsequently register for or use the tutoring grant. This analysis relied on a **reduced form** equation of:

$$Y_{is} = \beta + \delta_1 Z_{1,is} + \delta_2 Z_{2,is} + \lambda_s + \varepsilon_{is} \quad (3)$$

where $Y_{is}$is the outcome of interest, such as any funding use or amount of funding used; $Z_1$and Z_2indicate assignment to Treatments 1 and 2; and $\lambda_s$denotes randomization-strata fixed effects. Here, $\delta_1$and $\delta_2$are the ITT effects of the two randomized encouragement treatments on Y.

We also estimate an instrumental variables (IV) model in which randomized assignment serves as an instrument for registration. The **first stage** equation is:

$$R_i = \beta_0 + \pi_1 Z_{1i} + \pi_2 Z_{2i} + \lambda_s + \nu_i \qquad (4)$$

which is the same as equation 1. The **second stage** equation is:

$$Y_{is} = \beta + \theta \hat{R}_{is} + \lambda_s + u_{is} \qquad (5)$$

where $\hat{R}_{is}$is predicted registration from the first stage. Under the IV assumptions, θestimates the causal effect of treatment-induced registration on Y for students whose registration behavior was affected by the randomized encouragements.

3. Results

The results from the experiment indicate that the encouragement was successful as the treatments were effective at increasing program take-up rates. Both treatment groups demonstrated statistically significant improvements of notable magnitude on take-up rates, with Treatment 1 increasing program registration by 10.9 percentage points ($p < 0.01$) and Treatment 2 boosting registration by 6.1 percentage points ($p < 0.05$) relative to the control group. Treatment 1 increased registration by 37.1%, and Treatment 2 increased it by 20.7% relative to the control group rate of 29%. In focus group conversations with both district personnel and families, both groups noted that the eased registration made a positive difference in their experience.

3.1 Overall Counts on Student Registration for the LTGP

The registration process concluded on February 27, 2026, with the total allowable funding allocated to registered students. Later registrants were placed on a waitlist. Students on the waitlist were granted funding after the school year concluded. The Control condition in the five partner districts was notably more robust than the rest of the state because Learning Heroes

provided extensive supports. However, the Learning Heroes interventions were part of the business-as-usual context available to all eligible students in the partner districts.

First, we present raw registration rates (Table 1). For context, statewide, 26,612 kindergarten through third-grade students were eligible to enroll in the program, with 5,600 available grants of $1500. Excluding our five partner districts, 25,412 students were eligible for the tutoring grant, and 5,343 registered. This represents a statewide registration rate of 21.0%. In the five partner districts, eligible students were assigned to three groups: Control, Treatment 1, and Treatment 2. Of the 402 students randomly assigned to the Control group, 118 registered for the tutoring grant, yielding a 29.4% registration rate. Of the 399 students assigned to Treatment 1, 162 registered, a 40.6% registration rate. Of the 399 students assigned to Treatment 2, 143 registered, for a 35.8% registration rate. More importantly, within the five districts in our experiment, students were significantly more likely to register for the literacy tutoring grant if they experienced one of the treatments. The higher registration rate in Treatment 1 was statistically significant at $p < 0.01$, and the higher rate in Treatment 2 was statistically significant at $p < 0.05$, compared with the control group's lower registration rate. The higher registration rates in the Treatment 1 and Treatment 2 groups did not differ significantly from each other.

Table 1. Registration Rates for the Experimental Groups

Condition	Registration Rate (%)
Control	29.4%
Treatment 1	40.6%
Treatment 2	35.8%
p-value	0.004
Pearson chi^2	11.19

Comparison	Difference	Standard Error	P-Value
T1 v Control	11.25	0.034	0.001
T2 v Control	6.49	0.033	0.050
T1 v T2	-4.76	0.034	0.167

Note: p-values for the lower panel calculated from two-way pairwise Z scores.

To generate more precise estimates of the magnitude and statistical significance of the treatment impacts, we estimate the impact of the randomized treatment conditions on the probability of registering for the LTGP according to Equation 1, controlling for the fixed effects of randomization strata.

In the simplest model presented in Table 2, random assignment to Treatment 1, consisting of the encouragement letter coupled with the ease of registration due to the pre-populated customized webform, increased the probability of registration by 10.9 percentage points. That gain is on top of a control group registration rate of 29.4 percentage, described above, thus representing an estimated increase of 37.1% in the probability of registering for the program if a student experienced the conditions of Treatment 1. The regression-adjusted Treatment 1 impact is statistically significant at our high-test level of $p<0.01$.

Table 2. Impact of Encouragement on Registration

	(1)	(2)
Treatment 1	0.109***	0.108***
	(0.033)	(0.033)
Treatment 2	0.0608**	0.0534**
	(0.024)	(0.023)
Covariates	NO	YES
Constant	0.296***	0.620***
	(0.016)	(0.076)
Observations	1,200	1,198
R-squared	0.315	0.347

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
Both models include controls for randomization strata. In models including covariates, controls for female, nonwhite, FRL, SPED, 504, EL, and baseline test scores were included. The marginal effects from the Logit model were nearly identical to the coefficients from the LPM, so we present the more easily interpretable LPM results as our preferred.

Random assignment to Treatment 2, consisting of the encouragement letter, the ease of

registration due to the pre-populated customized webform, and three text nudges, increased the probability of registration by 6.1 percentage points, all else equal. Given the control group registration rate of 29.4 percentage points, the regression-adjusted Treatment 2 impact represents a 20.7% increase in the probability of registering for the program. The Treatment 2 impact is statistically significant at the $p < 0.05$ level. An F test rejects the null that the treatment impacts are jointly equal to zero. Column 2 shows the impact of treatment assignment on registration, controlling for baseline covariates. The results are largely consistent in both magnitude and significance. We present the remaining models, which include baseline covariates as robustness checks, in the appendix.

Randomized encouragement increases the take-up of the literacy tutoring grant by 6.1-10.9 percentage points relative to the control. This improvement comes at minimal cost. The intervention required reformatting communication to families and, for the nudges, additional text messages to parents. To determine the feasibility of replicating and scaling the intervention, policymakers must consider cost. For this intervention, which required a redesign of the registration process, the cost could be understood as $0. Abdul Latif Jameel Poverty Action Lab (2025) advises that a cost analysis should include an understanding of the counterfactual–in this case the cost of the Control intervention. The cost difference between the Control and Treatment registration processes was negligible. Considering not only effect size but also context and cost is a helpful diagnostic, and in this case, it should encourage policymakers and researchers to look for ways to scale simplified registration to encourage take-up for needed and effective literacy interventions (Kraft, 2020).

3.2 Student Use of Tutoring Services via the LTGP through Instrumental Variable Design

We next examine whether the effects of the encouragement treatments extended beyond

registration to actual use of tutoring services. We measure tutoring use through invoices submitted to the ADE to understand families' later actions after they registered for tutoring.

Registration did not automatically result in tutoring. Once ADE verified registered students as eligible, families had access to the tutoring grant funds ($1,500). Families could choose in-person, online, group, or school-based after-school tutoring. But families had additional steps after registration. These post-registration steps created potential barriers to program use that the experimental treatments did not directly mitigate. Registrants attrited by almost 50%. Lowering the registration threshold (through simplified registration) may reduce buy-in among registrants, lowering the likelihood of use (Dykstra et al., 2026). Attrition may also reflect administrative friction. To examine if registration led to use in predictable ways, we first present descriptive funding amounts and rates below, followed by an empirical analysis.

Families and tutors could submit invoices through June 30, 2026, but we focused on tutoring completed before the ATLAS literacy examination at the end of April. To include only funds spent preparing for the ATLAS literacy test, we totaled invoices submitted on or before June 3. This cutoff may have omitted some late-submission invoices for services completed during the testing window, but there is no reason to believe there is a systematic pattern of omissions.

Students in our five partner districts who registered for the program were more likely to use it and, on average, used more money than registered students in other districts across the state (Appendix Table A4). Of the 423 registered students in the five partner districts, 226 (53.4 percent) used any funds, while statewide, excluding the partner districts, 2,497 (46.7 percent) of the 5,343 registered students used any funds. Although $8.01 million was allocated for tutoring use in Arkansas (excluding the 5 partner districts), $2.61 million was used statewide, or 32.6% of

the allocation. In the districts that participated in our experiment, $636,000 was available, and $265,400 was used, or 41.7% of the allocation. We also observe the total amount used by students in each assigned group: 65 Control group students spent $71,240, 76 students in Treatment group 1 spent $87,144, and 85 Treatment 2 students spent $107,015 (Appendix Table A5).

The intention-to-treat estimates capture the effect of randomized encouragement on tutoring use, regardless of whether students registered for the program. However, we are also interested in whether registration itself increased subsequent use of tutoring. Because registration is a behavioral choice, a simple comparison of registrants and nonregistrants may be biased by unobserved differences between the two groups.

We estimate the causal effect of receipt of tutoring for compliers – registration for the LTGP. Registration is the behavioral response prompted by the first-stage encouragement. Instrumental Variables (IV) allows us to estimate the local average treatment effect (LATE) of induced registration. We use treatment assignment as an instrument for registration, with a two-stage least squares model.

For ease of interpretation and comparability, Table 3 presents results for whether students used *any* LTGP funds (a binary variable). In our naïve model (Column 1), the effect of registration on funding use is a large and statistically significant 41.5pp ($p < 0.01$). This LPM model is similar to our descriptive analysis, comparing registration directly with funding use without adjustment. Because registration is not randomly assigned, this estimate should be interpreted as an association rather than a causal effect (Equation 2).

We next estimate the intention-to-treat effect of randomized assignment on funding use using an LPM (Column 2). Treatment 1 increased the probability of using any funds, but the

estimate is not statistically significant. Treatment 2 increased the probability of using any funds by 4.6 percentage points (p<0.05). These reduced-form estimates capture the causal effect of the randomized encouragements on subsequent tutoring use (Equation 3).

The first stage confirms that both treatments which incorporated encouragement increased registration. The effect (discussed above) is a significant ($p < 0.01$) 10.9pp increase for Treatment 1 and a significant ($p < 0.05$) 6.1pp increase for Treatment 2 (Equation 4). To complete our analysis, we then instrument for registration with random assignment, finding that for compliers, that is, for students whose decision to register for tutoring was *changed* by their randomized assignment, registration increased the likelihood of using tutoring funds by a strong 30.7 pp ($p < 0.10$) (Equation 5). Appendix Table A6 presents the full results for the naïve, reduced form, first stage, and IV models, including covariates.

Table 3. Treatment Effects on Registration and Grant Utilization

	(1) Naive	(2) Reduced form ITT	(3) First Stage IV	(4) IV/2SLS
Treatment 1		0.026	0.109***	
		(0.016)	(0.033)	
Treatment 2		0.046**	0.061**	
		(0.022)	(0.024)	
Registration	0.415***			0.307*
	(0.086)			(0.167)
1st Stage F statistic				6.60
Constant	0.042	0.164***	0.296***	0.080
	(0.030)	(0.011)	(0.016)	(0.055)
Observations	1,200	1,200	1,200	1,200
R-squared	0.570	0.395	0.315	0.349

Robust standard errors in parentheses; Models include controls for randomization strata. *** p<0.01, ** p<0.05, * p<0.1

The first stage F statistic is 6.60, below the conventional Staiger-Stock (1997) and Stock-Yogo (2005) levels. The Kleibergen-Paap assessment finds the instrument is not

underidentified (p = 0.018), confirming its relevance. Given this, we report Anderson-Rubin Wald weak IV robust inference adjustments. The Anderson-Rubin Wald robust inference adjustment is 2.64 and marginally statistically significant (p = .097), and the Stock-Wright statistic is significant at conventional levels (p = 0.038). For our purposes, we determine that our instrument is identified but weak, and the weak inference adjustment was only marginally significant. Therefore, we interpret these results cautiously.

Finally, we examine the amount of tutoring use. Students had access to $1500 through the grant, but families differed in how much funding they used. We use a similar strategy for this funding outcome as for the first, but instead of measuring the impact on any funding use (a binary variable), we measure the impact on the amount of funding use (a continuous variable from $0 to $1500). Table 4 presents the full results.

In our naïve model, registration is associated with $451 of funding use ($p < 0.01$). Again, this is an association only and should be understood as a comparison of registration with dollars of funding spent (Column 1). In our ITT model (Column 2), we estimate the effect of randomized assignment on the amount of dollars spent. Treatment 1 shows an increase of $38 in tutoring, but the effect is not statistically significant. Treatment 2 shows an increase of $84 ($p < 0.01$) and is statistically significant. This reduced form equation represents the causal effect of randomized treatment assignment on the amount of tutoring dollars spent. In our final step, we instrument treatment assignment for registration and find that, for compliers (students whose registration status was affected by treatment assignment), grant dollars used increased by $456 ($p < 0.1$) (Column 4). Table A7 presents the full results for these models with covariate controls.

Again, we note the first-stage F statistic (6.60). We also applied weak-instrument robust inference adjustments to this outcome. The Anderson-Rubin Wald test performed better in

this model, and the Stock-Wright statistic was stronger as well ($p = 0.02$). We conclude that the instrument performs more reliably in predicting the amount of funding used. We continue to interpret these with caution, but we note the consistency of the regression adjusted results with the descriptive tables (Tables A4 and A5).

Table 4. Treatment Effects on Registration and Amount of Grant Utilization

	(1) Naive	(2) Reduced form ITT	(3) First Stage IV	(4) IV/2SLS
Treatment 1		38.03	0.109***	
		(23.09)	(0.0325)	
Treatment 2		84.22***	0.0608**	
		(25.02)	(0.0237)	
Registration	450.9***			456.1*
	(115.9)			(249.3)
1st Stage F statistic				6.60
Constant	62.23	180.5***	0.296***	60.39
	(40.84)	(14.05)	(0.0161)	(81.05)
Observations	1,200	1,200	1,200	1,200
R-squared	0.568	0.442	0.315	0.340

Robust standard errors in parentheses; Models include controls for randomization strata *** p<0.01, ** p<0.05, * p<0.1

Simplified registration coupled with encouragement (through both a parent-friendly letter and a sequence of text messages) showed a positive effect on both registration and on the funding use outcomes. We see a noteworthy increase in funding amounts and in the likelihood of using funding. In this scenario, funding use means families contacting and using a tutor to support their child's literacy growth.

3.3 Heterogeneous Treatment Impacts

We present Logit results of our preferred specification in Table A8. The results were

consistent with the easier-to-interpret LPM results. Next, in an exploratory analysis, we examined whether the treatment impact on program take-up varies by student characteristics (Appendix Table A9). We see that when interacting treatment assignments with baseline student characteristics, EL students experienced a greater effect from Treatment 1, by 11 percentage points, compared to non-EL students, with the differential impact marginally significant. Neither treatment had significantly differential impacts on students by sex, race, or special education status. We examined these subgroup results in an exploratory way, and we are unable to draw causal conclusions.

The impacts of the treatments on registration rates did not vary significantly across the five volunteer districts, as indicated by the fully-interacted model results in Appendix Table A10. A joint F-test measuring differences across districts in treatment effect yielded a $p = 0.843$. The treatment effects in District A serve as the omitted reference category, yielding a general effect of a 10.4 percentage-point increase from Treatment 1 and a non-significant increase of 3.8 percentage points from Treatment 2.

Examining the raw, unadjusted data on funding use, White students in both treatment groups had a higher likelihood of using funds than the control group: 15.7% for Treatment 1 and 13.8% for Treatment 2. However, while the likelihood of using funds increased only slightly for Nonwhite Treatment 1 students relative to Control (2.9%), that relative increase jumps to 66.2% when comparing Nonwhite Treatment 2 to the Control condition. The pattern of increase does not extend to either FRL or SPED students. Finally, we examined the amount used by registered students across demographic subgroups. Compared with White Control students, White Treatment 1 students saw a relative increase in funding use (12.4%), as did White Treatment 2 students (15.5%). Compared to Nonwhite Control students, Treatment 1 showed a 33.0%

increase, but Nonwhite Treatment 2 students showed a substantial 181.2% relative increase (Appendix Table A11). These results, while interesting, are not causal. Further research should investigate the potential to increase intervention take-up among marginalized students whose families may be most affected by administrative burdens.

3.4 Qualitative Results from Focus Groups

As part of our efforts to understand the registration process, our research team conducted a series of focus groups with the partner districts. We were successful at scheduling meetings with four of the five districts, although two of the meetings were online. During the meetings, we met with staff, faculty, and families. The research team structured the meeting using the framework developed by Krueger (2000), with the questions available in Appendix G. We used Zoom cloud audio transcription to record the conversations, but all respondents were fully anonymized. We received participants' permission to include their responses for research purposes. Staff members and families at each district were generally positive about their district's efforts at literacy intervention but noted that time constrains teachers' ability to meet students' needs. Faculty and staff members noted their support for the tutoring grant, but they additionally spoke of the apparent gaps students show, specifically in reading skills. Staff and families also noted that many students had a variety of specific needs, including dyslexia, autism, and ADHD, which impede literacy skill attainment. From one district employee:

There are so many gaps that we're trying to fill, and not enough resources.

Many districts made substantial efforts to boost participation in tutoring. These efforts affected control and treatment students equally and were outside the research team's purview. However, when asked about the initial registration process, both staff members and families noted its ease. From a parent (of a student assigned to the treatment group):

It just said, click back the little dot. They didn't even have… they didn't have to fill out a bunch.

However, parents and staff members expressed frustration over the subsequent step of funding through the program administration website. From a district employee:

I didn't know if we could have shorter steps, or more simple steps, or something just more parent-friendly, where it's just, you know, they understand, okay.

Many staff members expressed frustration that the program administration step included considerable lags in funding. Most concerning, some staff reported that families would abandon the process while waiting for the administrator to fund their accounts. One employee noted:

So the steps became too much for them, and then it was a discrepancy between the pay and the person, whoever they chose. So it became a little bit confusing to them if there was no person to assist.

Another staff member agreed, saying:

There are grandpa… great-grandparents that are raising their children. So, technology is an issue for them. And they'll tell you, baby, I have no idea what to do with that. I mean, I see it, I want the help. But I don't know how to do this, and so, you know, we'll say, okay, well, you know, come on, we'll help you get all the information, you know, that you need, and get you signed up and all of that. Well, I mean, do I have to have Wi-Fi? Do I gotta have… no, nope, you don't have to have those things. Just come on and we'll help you take care of it. But… (staff member shrugged).

Unfortunately, once parents registered for the tutoring grant, they were told to expect a follow-up email from the program administration vendor. The follow-up emails took as long as two weeks, and they often ended up in parents' spam folders. According to a staff member:

It was a problem with that email and getting the next one, you know, confirming so he could get the [vendor] email. There seemed to be some disconnect.

Upon receipt of the vendor email, parents frequently had difficulty navigating the steps of selecting a tutor, confirming service, and uploading invoices. District personnel and parents repeatedly expressed frustration with the difficulties after registration. One parent succinctly said, *"So, it was… it was just very overwhelming."*

These findings from our focus groups provide important context for interpreting the take-up and usage data from the ADE. Families who experienced less friction in the registration process reported difficulty with the subsequent steps to pursue tutoring. This difficulty may have led families to abandon their search for tutoring services, missing the opportunity afforded by the LTGP grant, at least according to families and district personnel in our focus groups.

3.5 Challenges and Limitations

The team focused on two primary areas of concern. The first related to timing and the second related to fidelity of implementation. We hoped to conclude text nudge delivery before the winter holiday break, but the roll-out timing pushed the launch to conclude in January, and a severe winter storm further interrupted it. Although the text nudges were intended to direct the students to their registration forms, that was not possible. Instead, nudges two and three simply reminded parents to use the form sent home to register. This may have artificially attenuated the impact of text nudges.

In addition, as with many RCTs, we had reservations about implementation fidelity. We monitored registrations to guard against spillovers. Three Treatment 2 students registered using the Control form, and five students, three who were Treatment 1 and two who were Control, registered directly with the program administrator, bypassing the registration forms that we

created and monitored for the experiment. Parents of two Control students and one Treatment 1 student attended focus groups and registered for the program on site after the focus group. We coded each of these students with their original treatment assignment, regardless of their subsequent actions. A total of 22 students registered after the program initially reached capacity and were placed on the waitlist. Nine of them are Control, six are Treatment 1, and seven are Treatment 2. We include those later registrants in the counts presented here. Because of the limited number of spillover registrations and the heterogeneity among waitlist registrants, we are confident that treatment status was maintained with fidelity.

4. Conclusion

Arkansas, joining many states, offers intervention services for vulnerable students. Registration to access these services remains disappointingly low. Is this due to disinterest from the client (the family) or poor provider design? While a positive idea in theory, families did not show eagerness to register for a grant that would give them funds for literacy tutoring in the program's inaugural year. Our research team opted for a low-cost, easily scalable intervention—simplified registration, parent-friendly language, and text nudges—to encourage registration. In a randomized field experiment, we tested the following hypotheses:

1. *Increasing awareness of program efficacy while simplifying the registration process will encourage tutoring registration across the eligible student population*. We saw an increase in raw registration counts for students who experienced both treatment conditions. These raw increases were confirmed by regression models, including those that adjusted for demographic differences and did not differ significantly across districts.
2. *Sending text nudges to families will further encourage tutoring registration across the*

eligible student population.. Students in Treatment 2 showed a higher registration rate than the Control group, but the impact was observationally smaller than in Treatment 1. However, students who received the text nudges were demonstrably more likely to use funding post-registration. While we are interested in the apparent impact on downstream use, we hesitate to speculate on why this result occurred. We anticipate future research to understand if the effect of encouragement, including text nudges, may have more persistent impacts on behavior.

3. *Families most adversely affected by administrative burden (i.e., students experiencing poverty, English Language Learners, and non-white students) will see the largest positive effect from the aid offered by the experimental interventions*. Although Treatment students in more vulnerable population groups showed increases compared to the control group, the results in our under-powered subgroup analyses were not statistically significant and should therefore be interpreted with caution. Future research should focus more directly on the impact of encouragement design and reductions in administrative burdens on specific population groups.
4. *Reducing administrative burden coupled with encouragement increases the likelihood of accessing tutoring as initially measured by funding accessed.* Using the full empirical design, we found that students who received text nudges in addition to the other encouragements were more likely to use any funds and used more funds than those in the control group. Future research should consider whether reducing administrative burdens alongside more high-touch forms of encouragement may lead to higher participation in available interventions.

Students who do not master literacy proficiency by third grade face challenges

throughout their school years. Policymakers allocating funds, such as the LTGP's $8.4 million, look for ways to make the policy more effective. More specifically, can administrative burdens limit policy usefulness by impeding participation in social interventions? If so, we anticipate future research will quantify the opportunities lost if we fail to ease administrative burdens to build capacity (Heinrich, 2015; Strunk, McEachin & Westover, 2014). Redesigning registration forms to include pre-populated fields with positive, client-friendly language is a simple, low-cost change policymakers should consider. The novel aspect of this intervention is its near-zero cost. Redesigning the registration forms to pre-populate required information requires only time investment to improve the user experience.

Future research will use the registration experiment as an instrument to measure the impact on ATLAS end-of-year literacy test scores. This year, third graders who do not demonstrate reading proficiency on the ATLAS examination face mandatory retention. The system should optimize ways to promote ease of take-up to avoid exacerbating registration asymmetry. Failing to incorporate simple changes to a burdensome registration process may cause more than low take-up; it could contribute to student learning loss and worsen inequality for students.

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Appendix A: The Randomization

Five districts across the state agreed to partner with the Arkansas Department of Education in a study of how to increase program take-up and, ultimately, the impact of the tutoring program. The five participating districts (anonymized here) had varying levels of participation in the previous year. District A is a large urban district serving approximately 7,203 students. In the most recent year, 30 percent of eligible students enrolled in the literacy tutoring grant, an increase from 10 percent the year before. District E is a large district serving approximately 3,660 students. About 13 percent of eligible students in District E enrolled in the literacy tutoring grant program. District C, a rural district, has an enrollment of approximately 4,128 students. In District C, 62 percent of students eligible to enroll in LTGP in the most recent year enrolled in the program. District D schools serve approximately 1,948 students in a rural setting and had the highest enrollment rate in the most current year, with 92 percent of eligible students enrolled in the literacy tutoring grant. District B is a small rural district with approximately 1,151 students. A mere 9 percent of eligible students in District B enrolled in LTGP in the most recent year. These discrepancies between partner districts mirror the heterogeneity in take-up across the state. The rates were similarly idiosyncratic and seemed a result of district efforts rather than student characteristics.

Appendix Table A1. Randomization of Eligible Students

		Priority 1			***Priority 2***		
District	*School*	*Control*	*Treatment 1*	*Treatment 2*	*Control*	*Treatment 1*	*Treatment 2*
	School 1	14	14	13	20	20	21
	School 2	12	13	10	22	24	24
	School 3	3	2	2	9	9	8
	School 4	8	5	7	8	9	9
	School 5	5	5	6	9	9	9
	School 6	5	5	6	12	12	11
	School 7	9	10	9	18	18	17
	School 8	6	6	5	15	13	15
	School 9	12	15	12	28	27	27
District A Total (N=642)		**74**	**75**	**70**	**141**	**141**	**141**
	District B	10	6	9	15	15	15
District B Total	**(N=70)**	**10**	**6**	**9**	**15**	**15**	**15**
	School 10				18	18	18
	School 11	9	8	8			
	School 12				14	14	16
	School 13	9	9	9			
District C Total (N=150)		**18**	**17**	**17**	**32**	**32**	**34**
	School 14	13	18	14	19	18	18
District D Total (N=100)		**13**	**18**	**14**	**19**	**18**	**18**
	School 15	5	5	6	13	14	15
	School 16	6	5	7	12	11	12
	School 17	5	5	5	8	8	9
	School 18	6	5	6	15	15	13
	School 19	2	2	1	8	7	7
District E Total (N=238)		**24**	**22**	**25**	**56**	**55**	**56**
Grand Total (N=1200)		**139**	**138**	**135**	**263**	**261**	**264**

Appendix Table A2. Covariate Balance Across Experimental Conditions

Variable	Control	Treatment 1	Treatment 2	Prob.	F Test P Value	P Value Priority 1	P Value Priority 2
Free/Reduced Lunch	0.96	0.94	0.92	0.08	0.06	0.78	0.03
Special Education	0.31	0.29	0.31	0.82	0.82	0.92	0.78
504 Status	0.04	0.08	0.07	0.15	0.12	0.04	0.91
Female	0.45	0.45	0.47	0.92	0.92	0.84	0.94
Race				0.76	0.70	0.98	0.38
Asian/Pacific Islander	0.08	0.08	0.09				
African American	0.49	0.51	0.50				
Hispanic	0.14	0.12	0.15				
White	0.29	0.29	0.26				
Race – Non-white	0.71	0.71	0.74	0.49	0.49	0.99	0.15
Immigrant	0.07	0.05	0.05	0.21	0.25	0.41	0.11
English Learner	0.13	0.10	0.14	0.20	0.21	0.69	0.19
Title III Status	0.11	0.10	0.12	0.67	0.37	0.75	0.77
Foster Care	0.00	0.01	0.01	0.60	0.71	0.23	0.23
Homeless	0.03	0.04	0.04	0.73	0.70	0.79	0.12

Note: Probabilities based on Pearson's Chi-Squared net of randomization strata fixed effects for the overall F tests and school fixed effects for the separate F tests by priority group.

Appendix Table A3. District Characteristics

District Name	District Enrollment	Gifted/ Talented	Special Education	Homeless	English Language Learner	Free and Reduced Lunch	White	Hispanic	Black	Non-white	Previous year take-up rate
District A	7,203	0.08	0.14	0.07	0.10	0.74	0.20	0.15	0.58	0.80	0.30
District B	1,151	0.13	0.14	0.01	0.04	0.78	0.57	0.11	0.27	0.43	0.09
District C	4,128	0.05	0.16	0.00	0.05	0.42	0.83	0.09	0.03	0.17	0.62
District D	1,948	0.04	0.19	0.02	0.02	0.53	0.90	0.06	0.02	0.10	0.92
District E	3,660	0.08	0.12	0.02	0.03	0.71	0.31	0.06	0.58	0.69	0.13
Total of 5 Districts	18,090	0.08	0.15	0.02	0.05	0.64	0.56	0.09	0.29	0.44	
Statewide	474,337	0.09	0.16	0.05	0.05	0.65	0.67	0.10	0.18	0.33	0.05

Appendix Table A4 Raw Funding Use by Registration Status[4]

	Statewide	Parter Districts	Average Amount Used by…
Eligible	25,412	1,200	$221.2
Registered	5,343	423[5]	$625.9
Users	2,497	226	$1,174.3
Total Amount Allocated	$8,014,500	$634,500	
Used	$2,608,833	$265,400	

Appendix Table A5 Raw Amounts Used by Assignment Status

Treatment status	Amount used	Number of users	Average amount used per user
0	$71,240	65	$1,096
1	$87,144	76	$1,147
2	$107,015	85	$1,259

Appendix Table A6 Treatment Effects on Registration and Grant Utilization

	(1) Naive	(2) Reduced form ITT	(3) First Stage IV	(4) IV/2SLS
Treatment 1		0.0197	0.108***	
		(0.0148)	(0.0331)	
Treatment 2		0.0362	0.0534**	
		(0.0216)	(0.0234)	
Covariants Included	*YES*	*YES*	*YES*	*YES*
Registered	0.392***			0.127
	(0.0813)			(0.139)
Constant	0.268***	0.513***	0.620***	0.740***
	(0.0683)	(0.0717)	(0.0757)	(0.166)
Observations	1,198	1,198	1,198	1,198
R-squared	0.586	0.435	0.347	0.424

Strata fixed effects and controls for gender, race, FRL, SPED, 504, EL status, and baseline test. Robust standard errors were included. *** p<0.01, ** p<0.05, * p<0.1

[4]Total allocation number may vary slightly due to students moving from waitlist status.

Appendix Table A7 Treatment Effects on Registration and Amount of Grant Utilization

	(1) Naive	(2) Reduced form ITT	(3) First Stage IV	(4) IV/2SLS
Treatment 1		27.26	0.108***	
		(22.49)	(0.0331)	
Treatment 2		71.07***	0.0534**	
		(22.40)	(0.0234)	
Covariants Included	*YES*	*YES*	*YES*	*YES*
Registration	423.5***			180.0
	(108.4)			(214.6)
Constant	411.0***	659.4***	0.620***	966.7***
	(92.09)	(95.21)	(0.0757)	(258.3)
Observations	1,198	1,198	1,198	1,198
R-squared	0.587	0.480	0.347	0.447

Strata fixed effects and controls for gender, race, FRL, SPED, 504, EL status, and baseline test. Robust standard errors were included. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table A8 Impacts of the Treatment Conditions on Registration for the LTGP

	Logit No Covariates	Logit With Covariates
Treatment 1	0.112***	0.099***
	(0.0320)	(0.034)
Treatment 2	0.0649**	0.049*
	(0.0273)	(0.026)
Female		-0.045 *
		(0.026)
Nonwhite		-0.336***
		(0.043)
FRL		-0.321***
		(0.071)
SPED		0.070**
		(0.029)
504		0.087**
		(0.043)
EL		0.184***
		(0.034)
Baseline Test Score		0.010
		(0.012)
Observations	1,200	1,198

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
Logit results present marginal effects

Appendix Table A9 Impacts of the Treatments on Take-up of the LTGP Interacted with Covariates

Variable	Female	EL	Nonwhite	SPED
Treatment 1	0.107**	0.107***	0.138**	0.093***
	(0.043)	(0.030)	(0.061)	(0.034)
Treatment 2	0.080*	0.064**	0.082	0.055*
	(0.042)	(0.029)	(0.061)	(0.032)
Coefficient of variable	0.0034	0.110*	-0.168**	-0.025
	(0.040)	(0.053)	(0.072)	(0.045)
Interaction with Treatment 1	0.005	0.110*	-0.043	-0.054
	(0.059)	(0.065)	(0.067)	(0.063)
Interaction with Treatment 2	-0.036	0.062	-0.026	0.019
	(0.056)	(0.102)	(0.056)	(0.048)
Constant	0.294***	0.281***	0.426***	0.304***
SE	(0.030)	(0.021)	(0.050)	(0.023)
R-squared	0.316	0.321	0.331	0.316
Observations	1,200	1,200	1,200	1,200

Robust standard errors in parenthesis. Strata level fixed effects are absorbed. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table A10 District-Specific Treatment Effects and Heterogeneity

District	Treatment 1	*p*-value	Treatment 2	*p*-value
District A	0.104***	0.003	0.038	0.249
District B	0.174	0.203	0.012	0.934
District C	0.068	0.461	0.147	0.084
District D	0.105	0.255	0.094	0.320
District E	0.128	0.073	0.031	0.651

Note: Effects generated via the Lincom command in Stata from a regression including treatment-by-district interactions.. P values based on t-tests with absorbed strata fixed effects and controls for gender, race, FRL, SPED, 504, EL status, and baseline test. Robust standard errors were included.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table A11 Funding Use by Discrete Student Characteristic among Eligible Students

	White	Nonwhite	Non-FRL	FRL	Non SPED	SPED
Likelihood of any use:						
0	.484	.068	.625	.142	.129	.236
1	.560	.070	.720	.158	.163	.265
2	.551	.113	.968	.147	.179	.28
Amount of use ($):						
0	618.57	48.07	870.31	148.49	146.00	248.01
1	695.25	63.95	899.6	176.88	187.13	306.62
2	714.61	135.21	1272.58	179.52	223.58	354.04

Control group is indicated by 0, Treatment 1 is indicated by 1, and Treatment 2 is indicated by 2
Likelihood is derived with a logit equation; marginal effects are reported. Amount of use is derived from an OLS equation.

Appendix B: Learning Heroes Support

The state partnered with the nonprofit, Learning Heroes (LH), to provide statewide support in building capacity for the Literacy Tutoring Grant Program. LH describes their role as follows: Learning Heroes is a national nonprofit that works to bring together families and educators as partners in K-12 education. We approach our work through an interconnected strategy rooted in research on the home-school relationship, actionable communications for families and educators, and tools and professional learning experiences to advance family engagement strategies that directly impact student learning and well-being. Concept Building the capacity of district teams to engage families is a key step in driving enrollment for the Literacy Tutoring Grant Program (LTGP). As such, we're excited about the opportunity to lead a Community of Practice with five Arkansas pilot districts in order to help strengthen the LTGP while also improving their family engagement practice overall. Providing comprehensive support to the cohort of district teams, the Community of Practice will include: 1. Communications Tools We will co-create a set of materials and marketing tools that district and school leaders can use to help parents understand the value of literacy tutoring for their child. The communications resources will reflect an asset-based, proactive message to families and support collaboration with the district around every level of conversation--from district leaders to principals, principals to teachers, and principals and teachers to families. The specific materials and tools to be customized will be determined in partnership with the team and informed by a set of research-informed messaging.

Appendix C: Technical Process

The research team randomized students into treatment assignment groups using the random number generator function in Excel (Microsoft 365). All data shared by the Arkansas Department of Education was secured in a shared Google Drive, owned and maintained by the ADE. We therefore generated letters, populated registration forms, generated individualized QR codes, and collected responses in the shared and secure Google Drive. We merged the list of eligible students from the provided Excel sheet to a Google sheet, and we then used the Extensions Add-On Script function on Google Sheets to create individualized letters with QR codes and blank registration forms for the Control students, as well as individualized letters with QR codes with populated registration forms for the treatment students. We employed the following prompt into OpenAI, ChatGPT 5.0 for guidance in coding with the Google extension. *"write a code that will both individualize letters using a list of names on a google sheet. Use the text of the letter found on (RESEARCHERS INSERTED GOOGLE DOC LINK). Where there are asterisks, insert the data from the sheet. Each letter should include a QR code to direct to a Google form. Responses should be collected in (INSERTED FOLDER NAME)"* for the Control students.

"write *a code that will both individualize letters using a list of names on a google sheet. Use the text of the letter found on (RESEARCHERS INSERTED GOOGLE DOC LINK). Where there are asterisks, insert the data from the sheet. Each letter should include a QR code to direct to a Google form. Responses should be collected in (INSERTED FOLDER NAME)"* (for Treatment students).

All coding work was done under the supervision of the research team. No student data was loaded into ChatGPT. We provide below a sample of the Google Script used. We retracted all

identifying information including names of files and links to both files and forms.

```
function createLettersBySchool_StartAt43() {
  const sheet = SpreadsheetApp.getActiveSpreadsheet().getSheetByName("Test
district");
  if (!sheet) throw new Error('Sheet "1TREATMENT2" not found!');

  const templateId = 'RETRACTED FOR SECURITY';
  const mainFolderId = 'RETRACTED FOR SECURITY';
  const baseFormUrl = 'https://docs.google.com/forms/d/e/1FAIpFAKE_FORM/viewform';

  const mainFolder = DriveApp.getFolderById(mainFolderId);
  const lastRow = sheet.getLastRow();
  if (lastRow < 1) return Logger.log("No data to process from row 1.");

  const startRow = 1;
  const data = sheet.getRange(startRow, 1, lastRow - startRow + 1, 7).getValues();
// Adjust columns if needed

  data.forEach((row, i) => {
    const [district, school, studentName, studentID, parentEmail, parentContact,
status] = row;
    const rowNumber = i + startRow;

    if (!studentName || status === "Done") return;

    try {
      const parentEmailSafe = parentEmail?.toString().trim() || "Missing";
      const parentContactSafe = parentContact?.toString().trim() || "Missing";

      const prefillUrl =
        `${baseFormUrl}?entry.RETRACTED=${encodeURIComponent(studentName)}` +
        `&entry.RETRACTED=${encodeURIComponent(studentID)}` +
        `&entry.RETRACTED=${encodeURIComponent("")}` + // Grade blank
        `&entry.RETRACTED=${encodeURIComponent(parentEmailSafe)}` +
        `&entry.RETRACTED=${encodeURIComponent(parentContactSafe)}` +
        `&entry.RETRACTED=Yes`;

      const schoolFolder = getOrCreateSubfolder(mainFolder, school);

      const copy =
DriveApp.getFileById(templateId).makeCopy(`${studentName}_Letter`, schoolFolder);
      const doc = DocumentApp.openById(copy.getId());
      const body = doc.getBody();
```

```
      body.replaceText('<<ParentName>>', '');
      body.replaceText('<<StudentName>>', studentName);
      body.replaceText('<<StudentID>>', studentID.toString());
      body.replaceText('<<Grade>>', '');
      body.replaceText('<<School>>', school);
      body.replaceText('<<ParentContact>>', parentContactSafe);
      body.replaceText('<<ParentEmail>>', parentEmailSafe);

      const qrBlob = UrlFetchApp.fetch("https://quickchart.io/qr?size=150&text=" +
encodeURIComponent(prefillUrl)).getBlob();
      replacePlaceholderWithImage(body, '<<QRCode_English>>', qrBlob);
      replacePlaceholderWithImage(body, '<<QRCode_Spanish>>', qrBlob);

      replacePlaceholderWithLink(body, '<<FormLink_English>>', prefillUrl, 'Click
here');
      replacePlaceholderWithLink(body, '<<FormLink_Spanish>>', prefillUrl, 'Haga
clic aquí');

      doc.saveAndClose();

      copy.setSharing(DriveApp.Access.ANYONE_WITH_LINK, DriveApp.Permission.VIEW);
      const letterUrl = copy.getUrl();

      const pdfBlob = copy.getAs(MimeType.PDF);
      pdfBlob.setName(`PDF – ${studentName}.pdf`);
      const pdfFile = schoolFolder.createFile(pdfBlob);
      pdfFile.setSharing(DriveApp.Access.ANYONE_WITH_LINK,
DriveApp.Permission.VIEW);
      const pdfUrl = pdfFile.getUrl();

      // Update sheet
      sheet.getRange(rowNumber, 8).setValue(prefillUrl);
      sheet.getRange(rowNumber, 9).setValue(letterUrl);
      sheet.getRange(rowNumber, 10).setValue(pdfUrl);
      sheet.getRange(rowNumber, 11).setValue("Done");

    } catch (err) {
      Logger.log(`Error processing row ${rowNumber} (${studentName}): ${err}`);
      sheet.getRange(rowNumber, 11).setValue("Error");
    }
  });

  Logger.log(`☑ Finished processing rows starting at 43.`);
}
```

```
// Helper functions
function getOrCreateSubfolder(parentFolder, folderName) {
  const folders = parentFolder.getFoldersByName(folderName);
  return folders.hasNext() ? folders.next() :
parentFolder.createFolder(folderName);
}

function replacePlaceholderWithImage(body, placeholder, imageBlob) {
  body.getParagraphs().forEach(p => {
    if (p.getText().includes(placeholder)) {
      p.setText(p.getText().replace(placeholder, ""));
      const img = p.appendInlineImage(imageBlob);
      img.setWidth(150);
      img.setHeight(150);
    }
  });
}

function replacePlaceholderWithLink(body, placeholder, url, displayText) {
  body.getParagraphs().forEach(p => {
    if (p.getText().includes(placeholder)) {
      const start = p.getText().indexOf(placeholder);
      const end = start + placeholder.length - 1;
      const te = p.editAsText();
      te.deleteText(start, end);
      te.insertText(start, displayText);
      te.setLinkUrl(start, start + displayText.length - 1, url);
    }
  });
}
```

Appendix D: Control Group Letter in English and Spanish[6]

Dear Parent/Guardian,
Based on_TEST_'s 2025-2026 beginning-of-year K-3 literacy screener assessment scores, your student is eligible for the Arkansas Literacy Tutoring Grant Program. The Arkansas Literacy Tutoring Grant Program provides families with $1500 to use for state-approved online or in-person literacy tutoring services and/or evidence-based digital literacy applications or software programs.
If you would like your child to take advantage of this opportunity, please apply at FORM, or scan the QR code below. Applications will open today and will close on June 1, 2026, with funds needing to be expended by June 30, 2026.
Applications will be reviewed on a first-come, first-served basis with initial approvals communicated via the email provided on the application within 14 days of receipt. Once approved, you will be able to access funds to purchase literacy tutoring services from a list of division-approved providers.
If you have questions about your child's eligibility or the Arkansas Literacy Tutoring Grant Program, please reach out to ade.tutoring@ade.arkansas.gov. For more information, please visit Division of Elementary and Secondary Education - Offices - Learning Services - Tutoring Services (arkansas.gov).
Information needed for Registration:
District Name: TEST
School Name: TEST
Student Name: TEST
Student ID Number: TEST



[QR Code] or visit: https://forms.gle/REDACTED

[6] Highlighted text was personalized and provided for families

Estimado/a padre/madre/tutor(a):
Basado en los resultados de la evaluación de alfabetización de inicio de año 2025-2026 para los grados K-3 de **[nombre del estudiante]**, su estudiante es elegible para el Programa de Becas de Tutoría de Alfabetización de Arkansas. El Programa de Becas de Tutoría de Alfabetización de Arkansas ofrece a las familias $1500 para utilizar en servicios de tutoría de alfabetización, en línea o presenciales, aprobados por el estado, y/o en aplicaciones o programas de software de alfabetización basados en evidencia.
Si desea que su hijo aproveche esta oportunidad, por favor presente su solicitud en FORM o escanee el código QR que aparece abajo. Las solicitudes abrirán hoy y cerrarán el 1 de junio de 2026, y los fondos deberán utilizarse antes del 30 de junio de 2026.
Las solicitudes se revisarán en el orden en que se reciban, y las aprobaciones iniciales se comunicarán al correo electrónico proporcionado en la solicitud dentro de los 14 días posteriores a su recepción. Una vez aprobada, usted podrá acceder a los fondos para adquirir servicios de tutoría de alfabetización de una lista de proveedores aprobados por la división.
Si tiene preguntas sobre la elegibilidad de su hijo o sobre el Programa de Becas de Tutoría de Alfabetización de Arkansas, por favor comuníquese con ade.tutoring@ade.arkansas.gov. Para obtener más información, visite **Division of Elementary and Secondary Education - Offices - Learning Services - Tutoring Services (arkansas.gov)**.
Información necesaria para el registro:
Nombre del distrito: TEST
Nombre de la escuela: TEST
Nombre del estudiante: TEST
Número de identificación del estudiante: TEST

or visit: https://forms.gle/REDACTED

Appendix E: Treatment Group (Both 1 & 2) Letter in English and Spanish

Dear Parent or Guardian,
Your student,(TEST (ID: 1324589), **is eligible** for the Arkansas Literacy Tutoring Grant Program. The Arkansas Literacy Tutoring Grant Program provides families with $1,500 that can be used on state-approved online or in-person literacy tutoring services and/or evidence-based digital literacy applications or software programs.

This is great news for your student! Research confirms that tutors help students improve their reading skills. Improvements in your child's reading ability will support their future learning. The Arkansas Literacy Tutoring Grant Program is designed to help them accomplish that goal.

Please confirm your child's participation in the program by following this link FORM or scanning the QR code below. Applications will open today. Applications will be reviewed on a first-come first-serve basis with initial approvals communicated via the email provided on the application within 14 days of receiving the application. Once that confirmation is processed, you will be able to access funds to purchase literacy tutoring services from a list of division-approved providers.

You can choose the tutor that you think is best for your child. You can also choose if the tutoring is provided by a computer program, by a person online, or by a person at your home, your child's school, or a community center. Staff at the Arkansas Department of Education can help you make these choices, but they are yours to make based on what is best for your child.

If you have questions about your child's eligibility or the Arkansas Literacy Tutoring Grant Program, please reach out to REDACTED@ade.arkansas.gov. For more information, please visit Division of Elementary and Secondary Education - Offices - Special Projects - Tutoring Services (arkansas.gov).

Please ensure your contact information and email are correct in the form. Check the box to confirm both email addresses before submitting.

Thank you,
TEST for grade 1
To register, please scan the QR code below and complete the form:



Estimado/a Padre, madre o tutor(a)

Su estudiante, TEST (ID: 1324589), es elegible para el **Programa de Becas de Tutoría de Alfabetización de Arkansas**. El Programa de Becas de Tutoría de Alfabetización de Arkansas proporciona a las familias **$1,500 dólares** que pueden utilizarse en servicios de tutoría de alfabetización, en línea o presenciales, aprobados por el estado y/o en aplicaciones o programas de software de alfabetización digital basados en evidencia.

¡Esta es una excelente noticia para su estudiante! Las investigaciones confirman que los tutores ayudan a los estudiantes a mejorar sus habilidades de lectura. Las mejoras en la capacidad lectora de su hijo/a apoyarán su aprendizaje futuro. El Programa de Becas de Tutoría de Alfabetización de Arkansas está diseñado para ayudarlos a alcanzar ese objetivo.

Por favor, confirme la participación de su hijo/a en el programa siguiendo este enlace o escaneando el código QR que aparece a continuación FORM. Las solicitudes se abrirán hoy. Las solicitudes se revisarán por orden de llegada, y las aprobaciones iniciales se comunicarán por medio del correo electrónico proporcionado en la solicitud dentro de los 14 días posteriores a la recepción de la misma. Una vez que se procese esa confirmación, usted podrá acceder a los fondos para adquirir servicios de tutoría de alfabetización de una lista de proveedores aprobados por la división.

Usted puede elegir al tutor que considere mejor para su hijo/a. También puede elegir si la tutoría se ofrece mediante un programa de computadora, por una persona en línea o por una persona en su hogar, en la escuela de su hijo/a o en un centro comunitario. El personal del Departamento de Educación de Arkansas puede ayudarle a tomar estas decisiones, pero la elección es suya, según lo que sea mejor para su hijo/a.

Si tiene preguntas sobre la elegibilidad de su hijo/a o sobre el Programa de Becas de Tutoría de Alfabetización de Arkansas, comuníquese a **REDACTED@ade.arkansas.gov**. Para obtener más información, visite **Division of Elementary and Secondary Education - Offices - Special Projects - Tutoring Services (arkansas.gov)**.

Asegúrese de que su información de contacto y su correo electrónico sean correctos en el formulario. Marque la casilla para confirmar ambas direcciones de correo electrónico antes de enviar.

Gracias,

TEST para el grado 1

Para registrarse, por favor escanee el código QR que aparece a continuación y complete el

formulario:

Appendix F: Text of Treatment 2 Nudges

Hi, this is (name of district employee) from (name of district) Your child is eligible for the Literacy Tutoring Grant Program. For your child to participate, you must confirm their enrollment. To confirm their enrollment, please use the registration form sent home with your child. If you need any help, your campus Instructional Specialist can help. We look forward to partnering with you for your child!

Appendix G: Focus Group Questions

The following example questions will be used in the focus group for family members:

1. (Opening question) What are your favorite parts of the school your child attends? Tell us about the parent/teacher/student relationships. What help is available to your student when he or she is struggling to pass a concept or unit?

2. (Transition questions) How do you learn information from your school? Do you feel that the information you learn is too much? Too little? How did you learn about the literacy tutoring opportunity?

3. (Key questions) Upon learning about the literacy tutoring program, what did you understand to be the primary benefits of the program? If you had questions, did you know where to find those answers? Were the steps involved in registration clear? In what ways were they not clear? Were there barriers to enrollment that you had to get over? How did the payment platform work for your family? Did you find it easy to choose a tutor and/or pay the tutor?

4. (Ending questions) Is there anything else we should know about the process of enrolling either as a tutor or as a student receiving tutoring services? Is there anything else that other parents may feel strongly about related to this program?

The following example questions will be used in the focus group for school personnel:

1. (Opening question) What are your favorite parts of the school and the district where you work? Tell us about the parent/teacher/student relationships. What help is available to students when he or she is struggling to pass a concept or unit?

2. (Transition questions) How do you learn information from your school? Do you feel that the information you learn is too much? Too little? How did you learn about the literacy tutoring opportunity?

3. (Key questions) Upon learning about the literacy tutoring program, what did you understand to be the primary benefits of the program? If you had questions, did you know where to find those answers? How did your administrator determine who would/should participate as a tutor? How did your administrator track participation for payment of tutors? Did you find the process of participating easy? In what ways?

4. (Ending questions) Is there anything else we should know about the process of enrolling either as a tutor or as a student receiving tutoring services? Is there anything else that other parents or tutors may feel strongly about related to this program?