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Book matters: The effect of Cocky's Reading Express on student performance☆

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ABSTRACT

The literature argues that children are more likely to succeed academically if they acquire strong reading skills and a love of reading at a young age. In this paper, I evaluate an early childhood literacy program, Cocky's Reading Express (CRE), to understand how reading events at school and the gifts of books impact learning. Combining the CRE visit records with administrative student data, I find that CRE leads to 0.02–0.03 of a standard deviation increase in statewide English Language Arts test scores among low-income students one year after the visit and find suggestive evidence that CRE improves the math scores for subgroups of students in poverty. In particular, the CRE effect varies based on locality and access to reading materials, with a larger effect on students residing in metropolitan areas or close to public libraries. However, the positive effects on low-income students diminish over time; CRE does not show impacts on the scores of students from better-off families either.

1. Introduction

Early childhood development is essential to an individual's subsequent life outcomes. The literature has documented early childhood intervention as an effective means of improving academic achievement for disadvantaged children and reducing the gap between disadvantaged children and their more advantaged counterparts (*e.g.*, Deming, 2009; Heckman et al., 2010; Magnuson and Duncan, 2016; York et al., 2018)..

This paper assesses the effects of a reading intervention program, Cocky's Reading Express (CRE), on children's academic outcomes. CRE is a literacy outreach program of the University of South Carolina (UofSC) founded by a group of undergraduate students in 2005 with a mission to promote literacy among children in South Carolina. Under this program, UofSC students and the university mascot Cocky visit children in pre-kindergarten (pre-K) through second grade in underserved public schools to read aloud to them and give each child a book of their own.

CRE may improve academic performance via several channels. First, the reading sessions may spark children's interest in books and help them understand the importance of lifelong reading. Young children likely look up to college students from a flagship university and the beloved figure Cocky and thus take their advice to read seriously (Prilosadoso et al., 2021; Ross et al., 1984). The book gifts may remind them of the CRE visit so that their motivation lasts longer. When children get more motivated to read, parents are also more likely to share reading with them (Preece & Levy, 2020). Second, the gifts of books help enhance home literacy environment. The physical proximity of books increases children's exploration of and engagement with books and thus improves their literacy outcomes (Lindsay, 2010; Neuman, 1999). More importantly, the presence of age-appropriate books at home may incentivize parents to initiate and maintain book reading routines (Bus et al., 1995; Kalb & van Ours, 2014; Neuman et al., 2021). Third, CRE hands out leaflets of book recommendations and information on community literacy resources in addition to books. When parents are better informed about quality reading materials and where to access them, they may become more likely to utilize such resources. Last, reading provides information across the curriculum (Grimm, 2008). Hence, CRE may positively impact student performance in subjects other than reading by improving literacy skills (Glenberg et al., 2012; Grimm, 2008; Hubner et al., 2022). CRE may also inspire increased motivation for learning so that participants may perform better in all subjects. Notably, families of low socioeconomic status (SES) typically have fewer books at home than higher-SES families. Low-SES parents are also less likely to share book reading, utilize community literacy resources, and have linguistic interaction with their children. Therefore, CRE may particularly benefit children from disadvantaged family backgrounds.

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I link the CRE visits over the years, which are recorded by school and grade, to the individual-level English Language Arts (ELA) and math scores in statewide tests of elementary students. Using a difference-in-differences approach, I compare the test scores of students visited by CRE to those not visited in the previous school year, controlling for student demographics, including measures of poverty, school characteristics that may affect student performance, and unobserved heterogeneity across grades and schools. Though children do not self-select into the CRE sessions, CRE may choose the schools to visit according to school attributes unobserved to econometricians but relevant to student outcomes. Therefore, I match a visited school-year observation to the "nearest neighbor" in the control group so that a matched pair of schools are geographically close, share common student characteristics, and garner similar educational resources. The only difference is that the CRE visits one school but not the other in that particular year. Then, I add the matched-pair fixed effects to the difference-in-differences model to control for the unobserved heterogeneity across (pairs of) schools. Alternatively, I calculate the difference, or "distance", between a matched pair and restrict the sample to schools with the distance in the bottom quartile. This way, the treated and control groups are balanced in the observables.

The empirical results suggest that CRE visits improve the test scores of elementary school students from low-income households but have little impact on their higher-income peers. Specifically, the ELA scores of low-SES students increase by 0.02 to 0.03 standard deviations one year after a CRE visit; the impact on the math scores of the same demographic group is smaller and only significant in some specifications. Moreover, the effect of CRE on the ELA scores decreases in the subsequent years; the improvement in the math scores takes more time to manifest and is also slightly more persistent than that in ELA.

I next explore the heterogeneous effects of CRE to shed light on the mechanisms of why this program enhances student academic outcomes. First, I find that low-income students in schools where the school library offers fewer books per student enjoy a slightly larger increase in their test scores than their counterparts whose school library has more resources. This finding may suggest that the books gifted to children who are most deprived of reading materials, either at school or at home, make a difference in promoting a scholarly culture at home (Evans et al., 2010).

Second, the effects of CRE vary by the potential access to literacy resources. In particular, the impacts of CRE on disadvantaged children appear more evident in metropolitan areas than in rural areas. Low-SES students also experience more significant test score increases if they live close to a public library. The improvements among these students are even larger if CRE visits a school right before school vacations when students presumably use library services more intensively (Gilpin & Bekkerman, 2020). The CRE events and the gifts of books may have children and their families start a reading routine and utilize extra reading materials they would not otherwise use. Because CRE provides information regarding community literacy resources, students with easier access to such resources, either because they reside in a metropolitan area or close to public libraries, are more likely to take advantage of them and improve their academic performance accordingly. These findings may verify that, while book ownership exerts an important influence in its own right, the increased support at home, either through shared reading activities or efforts to garner additional reading materials, may also account for the child outcomes (Neuman et al., 2021).

This paper has three main contributions to the literature. First, this paper contributes to a large economic literature on early childhood interventions. This literature largely focuses on the impacts of preschool programs (*e.g.*, Magnuson et al., 2007; Deming, 2009; van Huizen and Plantenga, 2018). This paper extends this literature by examining a reading intervention, Cocky's Reading Express, that affects home-based inputs to early childhood development. Although there is no data directly measuring home inputs, the current results may imply that families increase their literacy-promoting activities post-CRE visits and confirm several studies in other fields that demonstrate a positive association between book gifting and parental interest in and frequency of shared book reading, public library use, and the number of children's books at home (de Bondt et al., 2020; Neuman, 1999). Second, the paper adds to a broad multidisciplinary literature that evaluates reading interventions and book distribution programs (*e.g.*, Kim and Quinn, 2013 ; de Bondt et al., 2020; Neuman et al., 2021). Notably, most of these studies are in realms other than economics; they are often based on small samples and fail to address the endogenous self-selection of participants. This paper fills the gap and provides causal estimates using administrative student data. The paper also highlights the role of community literacy resources that may improve the effectiveness of school-based reading interventions. Lastly, relative to other reading interventions, CRE is unique in that it combines reading sessions, book gifting, and information dissemination. To the best of the author's knowledge, no other book giveaway program studied in the literature contains all these elements. The three aspects of CRE may complement each other so that a modest intervention like the CRE can generate economically meaningful academic improvements among low-SES students. The findings also confirm the argument in the existing literature that personal contact is crucial to the efficacy of book giveaway programs (de Bondt et al., 2020).

The findings in this paper have important policy implications. Book giveaway programs are a relatively low-cost strategy to promote early childhood literacy and have been adopted in many countries and served millions of families. Technology advancement may reduce the cost further as electronic books become more common in book distribution (de Bondt et al., 2020), although the influence of e-books on child reading is questionable (Furenes et al., 2021). To close the achievement gap between disadvantaged and advantaged children, which emerges even before they start kindergarten, reducing the disparities in book availability to children becomes more important than ever. The COVID-19 global pandemic has dramatically sped up the digitalization of education, while the adoption of technology in education likely broadens, rather than narrows, these gaps (Gordanier et al., 2022; Vigdor et al., 2014). The closure of schools and public libraries due to the pandemic further limits student access to books and other educational resources, which disproportionately hurts the learning of young and low-SES students (US Department of Education, 2021; Weiland et al., 2021). Book distribution programs may attenuate the problem by providing free and inexpensive books and learning materials to children in low-income communities (Aridi, 2020). Therefore, it is of critical importance to understand which delivery model can efficiently inspire book reading and how organizations that offer books and supports to families can reach those most in need. Analyzing the effects of CRE on children's academic outcomes may thus provide potential answers to these questions. The empirical results in this paper may verify the pivotal role of personal connection in book giveaway programs for both parents and children. As CRE has shown little impact on children from nonpoor families, books given to high-SES families are likely a redundancy of efforts and a waste of resources.[1]

The rest of the paper is organized as follows. Section 2 conducts a review of relevant literature; Section 3 describes the program of Cocky's Reading Express in further detail; Section 4 discusses the data employed in the analysis; Section 5 introduces the empirical strategies; Section 6 presents and discusses the regression results; finally, Section 7 concludes the paper.

[1] Still, children from high socioeconomic backgrounds may benefit from book giveaway programs in aspects not reflected in their test scores. Future research is warranted to assess these improvements.

2. Literature review

A broad literature in economics evaluates the impacts of early childhood intervention on the academic outcomes of children. The literature finds these interventions effective in improving the academic achievement of disadvantaged children and reducing the gap between disadvantaged children and their more advantaged counterparts. Most of these studies focus on preschool programs, such as Head Start (Deming, 2009; Garces et al., 2002; Kose, 2021), public pre-K programs (Andrews et al., 2012; van Huizen & Plantenga, 2018), and means-tested subsidies that can be used to pay for center-based care (Magnuson et al., 2007). Interventions directly impacting home-based inputs to early childhood development are less studied in this literature, with a few exceptions. For example, York et al. (2018) and Cortes et al. (2021) study a messaging intervention for parents of preschoolers that provides parenting tips and ideas and find improvement in parental engagement and short-term literacy achievement gains; Kearney and Levine (2019) show that the introduction of *Sesame Street*, which aims at reducing the educational deficits experienced by disadvantaged children, improves school performance, particularly for boys. There is also a strand of papers examining the impacts of cash assistance or tax rebates to families on the academic outcomes of children (Bastian & Michelmore, 2018; Dahl & Lochner, 2012). However, how families spend the added income from social benefits is usually not distinguishable.

Books are a crucial input to education, even as technology becomes increasingly prevalent in education. Numerous organizations and advocacy groups have devoted substantial resources to enhancing book access, particularly among disadvantaged children (Annie E. Casey Foundation, 2010). Book giveaway programs, especially the large-scale ones such as Bookstart, Reach Out and Read, and Dolly Parton's Imagination Library,[2] have been widely analyzed in the fields of education, health, psychology, and sociology but have yet to gain more research attention in the economics literature. Many studies show that these programs improve children's home literacy environment and literacy-related behavior and skills, despite the insignificant effect on the number of books at home (*e.g.*, Goldfeld et al., 2011; van Steensel et al., 2011; Ridzi et al., 2017; Skibbe and Foster, 2019). Of the three large programs, Reach Out and Read is found to have the strongest impact on child literacy outcomes, notwithstanding the limited number of books given out per child. Possibly, a vital element of Reach Out and Read is that families receive advice on shared book reading from a pediatrician or nurse practitioner whom parents trust and respect, whereas the other programs do not involve much personal contact (de Bondt et al., 2020). Nevertheless, most relevant research relies on small samples, and their methodological quality varies. Also, participants may self-select into the book gifting programs mentioned above under their program designs, but the existing studies largely fail to address the endogenous selection issue.[3]

3. Background

Cocky's Reading Express (CRE) is a literacy outreach program of the University of South Carolina (UofSC). The program primarily serves students in pre-K through to the second grade, with priority given to students in underserved public schools.[4] Since its creation in 2005, faculty and student volunteers from UofSC have traveled with the Carolina mascot Cocky across South Carolina to read to children. They also give out the books that are read, and every child receives a copy. The program is free as it is funded entirely by grants and gifts. To date, CRE has visited over 200 public schools in all 46 counties of the state and handed out over 100,000 books to children.

CRE conducts three types of events: regular school visits, family literacy nights, and public library readings. Regular school visits are the most common among the three. These events are assembly-style and held while school is in session. A reading event typically lasts for about 45 min. The college students read to the children, and Cocky helps the children understand the importance of lifelong reading. After the event, CRE gives each participating child a goody bag to take home as a reminder of the visit from Cocky and the UofSC students. The bag contains one book (sometimes more than one book depending on funding availability) and several leaflets on recommendations for reading materials. CRE selects age-appropriate, award-winning books covering various life-related topics to spark the reading interests of children, even if the books given away vary by visit.[5] The other two types of events take place at a lower frequency. The family literacy nights are usually held in the evening at a school that CRE visits during the day, and parents and children are both invited. CRE also sometimes reads to children from the general public at public libraries, usually on weekends. These events are also mascot-based, during which children are read to and given books.

In this paper, I focus on regular school visits due to data availability. When deciding the schools to visit, the staff of CRE usually look at high-need counties, such as the counties in the Interstate 95 (I-95) corridor,[6] and choose from the list of Title I schools,[7] prioritizing the ones that have never been visited by CRE or have not been visited for a while. Typically, CRE would wait three to five years to visit the same school so that a different cohort of children could participate in the program. Schools may request a visit from CRE, but CRE can only honor such requests if there are funds available and time in the schedule, prioritizing underserved schools. As a result, visits initiated by school requests make up a tiny portion of all CRE school visits. In a few instances, CRE needs to indicate which schools to visit when applying for a grant. If the funder agrees with the choice of schools, the grant funds are restricted to the selected schools. Therefore, CRE may visit these particular schools more than once within one or two years due to the grant requirements. Sometimes, a donor asks CRE to visit a specific set of schools when making a gift. For example, as a sponsor to CRE, Dominion Energy has requested CRE to visit public schools in their service area. Nevertheless, donor requests are less likely to be related to

[2] Bookstart, established in the United Kingdom in 1992, is the world's first national book gifting program. The program provides two gift packs, one for infants and the other for children aged three to four. Each pack typically includes one or more age-appropriate books and a flyer with reading tips and book suggestions. Reach Out and Read was initiated in Boston in 1989 and has expanded across the United States. The program works with pediatric care providers to distribute books to families at well-child visits from six months old until age five, targeting low-income families but not exclusively. Dolly Parton founded Imagination Library in Tennessee in 1995 and currently operates in the United States, United Kingdom, Canada, Australia, and Ireland. This program sends one free book monthly to children from birth until age five, regardless of family income.

[3] A few papers attempt to account for participants' self-selection into Imagination Library and find the program has limited impacts on student outcomes (Bennett, 2021; Thompson et al., 2017). However, the insignificant estimates may also result from the quality of the data employed.

[4] Underserved schools refer to schools in poor areas that cannot provide adequate education for children. They are typically underfunded and in need of effective educators; they often fail to meet accountability requirements.

[5] Appendix Table A.1 displays some books CRE has handed out to children participating in the events.

[6] The I-95 corridor denotes a strip of poor, rural areas along Interstate 95, the main north-south Interstate Highway on the US East Coast, in South Carolina. The I-95 corridor is one of the most impoverished areas of South Carolina and is known for its disrepair of public education and high unemployment rates.

[7] Title I is a federal education program that provides financial assistance to local educational agencies and schools with high numbers or high percentages of children from low-income families to help ensure that all children meet state academic standards.

Table 1
Summary statistics for CRE visits: 2006–16.

Total number by year				Total number by grade	
School year	No. of visits	Schools visited	Books gifted	Grade visited	No. of visits
2006	22	20	–	3K	1
2007	6	6	–	4K	123
2008	20	20	2,857	K	263
2009	15	14	1,519	1	262
2010	19	19	3,882	2	235
2011	60	54	8,789	3	32
2012	50	42	9,095	4	16
2013	44	42	8,906	5	15
2014	35	21	6,287	6	6
2015	31	23	6,332	7–8	1
2016	32	29	7,868	9–12	1
Total	334	290	55,655	Total	958

Note: School years are denoted by the calender year in which a school year ends. The number of books given out is not recorded or missing for some schools in years 2006–2011. So the annual numbers reported above, as well as the total number in the last row, are smaller than the actual numbers. The total number of visits by grades is calculated at the grade level.

the performance of a particular school, and priority is given to schools with more severe student poverty among the eligible ones.

CRE coordinates with schools to determine which grades to visit. Typically, a CRE reading session includes all students in pre-K to the second grade in a school unless a particular grade is not offered at the school or has a scheduling conflict. For instance, if a CRE event is scheduled in the afternoon and the pre-K program is half-day, pre-K students would not participate. Sometimes, CRE reads to grades other than pre-K to second grade. For example, due to funder requests, CRE organized a few reading events for special needs students in higher grades. Schools may also have CRE include additional grades in their program if they have a small enrollment.

For a potential donor, CRE asks for $2,500 to initiate a school visit. The fund covers the cost of transportation (including the bus driver and gas), books, and other printed materials. The faculty and college students who lead these programs are all volunteers. Thanks to the Scholastic Literacy Partnership, CRE can get books for as little as $2 each, and they always work within the budget. Usually, with a grant for the minimum amount, CRE will visit an audience of 300 students at maximum. More significant gifts and grants enable CRE to visit larger schools or conduct multiple school visits to meet the needs of more children.

Table 1 displays some statistics regarding CRE visits from school years 2005–06 through 2015–16. The first four columns present the numbers of CRE visits, schools visited, and books handed out each year.[8] While the program expands over time, the number of school visits fluctuates due to funding availability. The last two columns report the number of visits by grade. Students in pre-K to grade 2 receive the most visits, whereas other grades are sometimes visited.

Notably, CRE has several unique features compared to better-known book giveaway programs that have attracted much research attention, such as Bookstart, Reach Out and Read, and Imagination Library. First, CRE does not simply give books away to children; CRE also organizes mascot-based events to promote reading. Second, CRE targets older children, whereas other book gifting programs focus on children from birth until they start school. While some studies regard earlier book reading as more beneficial to a child's language and cognitive developments (Raikes et al., 2006), pre-K to the second grade is a critical stage when children gradually become independent readers. Third, unlike many other programs, the staff at CRE decide which schools to visit, and the choice primarily depends on student SES in a school. While participation is not mandatory, it is common for all children in relevant grades to join a CRE reading session. Therefore, CRE participants are unlikely to be selected into the program according to their endogenous motivations. Lastly, large-scale book giveaway programs can have much heterogeneity across locations and time. For instance, when giving away books, some health care providers in Reach Out and Read may provide parents with a long and detailed consultation, while others may give minimal emphasis on the importance of book reading. Heterogeneity is less likely to be an issue with CRE visits. These reading events are always led by a group of UofSC student volunteers and have the same format, even though the books read and handed out can differ.

4. Data

I employ administrative data from the South Carolina Department of Education (SC DOE) and the South Carolina Department of Social Services (SC DSS) to acquire information on student backgrounds and academic performance. Specifically, I use panel data from 2007–08 to 2015–16 from the SC DOE. (I denote a school year using the year in which the school year ends henceforward.) The end-of-year scores of state standardized tests for English Language Arts (ELA) and math are used to measure student academic outcomes. In South Carolina, students take statewide math and ELA tests beginning in the third grade. As noted before, CRE may visit grades higher than the second grade for a reason. Therefore, to avoid endogeneity-related issues, I focus on elementary school students in traditional schools[9] and assess their test scores in grades 3 through 5 in the primary analysis.[10] The DOE data also contain information on student demographics, including gender, race, and ethnicity. More importantly, the dataset includes the registration information of all public school students. Even if students in grades lower than the third do not take statewide tests, information on the schools these students attend is available. The DSS data allow me to observe if a student's household participates in the Supplemental Nutrition Assistance Program (SNAP) or Temporary Assistance for Needy Families (TANF) in a year. Hence, I can distinguish students living in poverty.

Because student academic performance is highly dependent on the inputs to education at the school level, I gather school-level characteristics from annual school report cards (also produced by the SC DOE) and the Common Core Data from the National Center for Education Statistics. These characteristics include total enrollment, the student-teacher ratio in core subjects, the share of teachers with advanced degrees, expenditures per pupil, average teacher salary, whether a school provides a school-wide Title I program, whether a school is a charter school, a magnet school, or other non-traditional schools, and the locality of a school. I exclude special needs public schools from the analyses and link the student-level data to the CRE visit records based on the school year, school, and grade.

Table 2 displays the descriptive statistics of the data. The sample contains 1,404,593 observations from 616,089 students in 858 schools. Almost 40% of the students in the sample receive either SNAP or TANF during the period of study. Over half of the students are white, and

[8] The South Carolina Center for Community Literacy of the UofSC generously provides the records of CRE school visits. The records contain information on the date of each CRE visit, the school visited, the grades read to, and the number of books handed out. Unfortunately, the number of books was not documented in 2006 and was missing for some schools from 2007 through 2011. Hence, the yearly total number is smaller than the actual numbers.

[9] Elementary education in South Carolina ranges from grade 1 to grade 5. Most elementary schools also include five-year-old kindergarten (5K or K), while some offer pre-K programs. Traditional schools refer to the schools that offer conventional education. Examples of non-traditional schools include magnet, charter, and virtual schools.

[10] SC DOE and SC DSS also provided data on students in grades 6–8. Therefore, I include both elementary and middle school students in the sample as a robustness check.

Table 2
Summary statistics.

Variable	Mean	Std. Dev.	Min	Max
Panel A: Individual Characteristics				
ELA Score (Normalized)	0.000219	1.000	−7.207	13.38
Math Score (Normalized)	0.00261	0.999	−6.726	13.18
Benefit Recipient	0.392	0.488	0	1
Female	0.490	0.500	0	1
Race and Ethnicity				
White	0.535	0.499	0	1
Black	0.364	0.481	0	1
Hispanic	0.0719	0.258	0	1
Asian and Pacific Islander	0.0182	0.134	0	1
American Indian	0.00417	0.0645	0	1
Multiracial	0.00757	0.0867	0	1
No. of Observations				1,404,593
No. of Students				616,089
No. of Years				9
Panel B: School Characteristics				
CRE (=1)	0.0405	0.197	0	1
No. of CRE Visits	0.0467	0.272	0	8
Books Given Out per Student	0.118	0.647	0	10.83
Total Enrollments	96.10	54.99	2	593
% Students on Benefits	0.449	0.193	0.00581	1
% Black	0.420	0.280	0.00907	1
% Hispanic	0.0786	0.0851	0.00600	1
Student-Teacher Ratio	19.28	3.673	0.300	39.50
% Teachers w/ Advanced Degrees	60.60	11.80	0	100
Expenditure per Pupil	6.060	1.918	0	64.95
Average Teacher Salary (1000 82-84 USD)	0.0357	0.00288	0.0142	0.0468
Title I School	0.821	0.384	0	1
Not Visited in Past 3 Years	0.905	0.293	0	1
I-95 Corridor County	0.219	0.413	0	1
Dominion Service Area	0.234	0.423	0	1
School Type				
Elementary School	0.890	0.313	0	1
Elementary & Middle School	0.0422	0.201	0	1
Middle School	0.0677	0.251	0	1
No. of Observations				5,908
No. of Schools				858
No. of Years				9

black students make up about 36% of the student population. Over 80% of the schools conduct a Title I program during the analysis period; about 22% are in I-95 corridor counties, and 23% are inside areas served by Dominion Energy. Approximately 4% of the school-year observations have received CRE visits.

5. Empirical strategy

5.1. Regression model

To assess the effect of CRE on student academic performance, I estimate a difference-in-differences specification using a two-way fixed effects (TWFE) model. Specifically, I estimate the following equation:

$$Y_{igst} = \beta_1 CRE_{gst-1} + X'_{it}\beta_2 + Z'_{st}\beta_3 + \alpha_s + \delta_{gt} + \varepsilon_{igst}. \tag{1}$$

Y_{igst} stands for the test score in a specific subject for student i in grade g of school s in year t. CRE_{gst-1} is a measure of CRE visits one year ago, such as a binary indicator for whether CRE visits a school-grade in a year, the total number of visits to the school-grade in that year, and the number of books given away per student. These measures are all at the school-grade level, as CRE does not document the information of individual students who participate in the events. I link a student's current test scores to the status of CRE visits in the year before for two reasons. First, elementary school students start to take statewide tests of math and ELA in the third grade, and CRE primarily targets students in four-year-old kindergarten (4K) to the second grade. Therefore, examining the lagged effect results in a larger and less selective treatment group. Second, if CRE visits lead to changes in reading behaviors, these changes may take time to be reflected in test scores.

X_{it} is a vector of individual characteristics, including gender, race, and student poverty measured by whether student i receives SNAP or TANF benefits in year t. Z_{st} contains school characteristics that may affect student performance. These characteristics include student-teacher ratio in core subjects, the share of teachers with advanced degrees, expenditures per pupil, average teacher salary, the share of students on benefits in a school, and whether the school provides a school-wide Title I program. Since one cannot rule out the possibility that the characteristics of a school evolve endogenously with CRE visits, I also test a specification without controlling for Z_{st}.[11] The school fixed effect, α_s, and the grade-by-year fixed effect, δ_{gt}, capture the discrepancies across schools, grades, and over time which are not accounted for by individual characteristics or time-variant school characteristics. I control for school fixed effects in the main specification because CRE chooses which schools to visit, and there is no information on whether a specific student participates in a CRE event. Nevertheless, I test two alternative specifications: one controls for individual fixed effects instead of school fixed effects; the other replaces the school fixed effects and grade-by-year fixed effects with school-by-grade fixed effects and year fixed effects.[12] ε_{igst} is an idiosyncratic error.

11 Results are similar with and without controlling for time-variant school characteristics. The results without school characteristic controls are available from the author upon request.

12 The estimates are shown in Appendix Table A.2 and resemble the baseline results.

Table 3
CRE visits and school characteristics.

Variables	1(CRE Visit)				
	(1)	(2)	(3)	(4)	(5)
Total Enrollments	−0.026	−0.024	−0.034	0.015	−0.128**
	(0.020)	(0.022)	(0.026)	(0.051)	(0.056)
% Students on Benefits	0.009	0.035	0.042*	0.116	0.166***
	(0.023)	(0.023)	(0.025)	(0.084)	(0.063)
% Black	0.052***	0.014	0.003	0.008	−0.018
	(0.015)	(0.016)	(0.018)	(0.061)	(0.042)
% Hispanic	0.010	0.001	0.064**	−0.008	0.009
	(0.025)	(0.027)	(0.031)	(0.074)	(0.063)
Student-Teacher Ratio	0.000	0.000	0.001*	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
% Teachers w/ Advanced Degrees	0.000	0.000	0.000	−0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Expenditures per Pupil	0.002	0.001	0.002	−0.002	−0.004
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)
Average Teacher Salary	0.001	0.000	−0.000	−0.003	−0.003
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
1(Title I School)	0.006	0.007	0.004	−0.003	0.001
	(0.006)	(0.006)	(0.006)	(0.007)	(0.008)
1(Non-traditional School)	−0.015*	−0.012	−0.003	−0.010	−0.003
	(0.008)	(0.008)	(0.009)	(0.015)	(0.017)
1(Not Visited in Past 3 Years)	−0.044***	−0.034**	−0.000	0.126***	0.089***
	(0.015)	(0.014)	(0.012)	(0.016)	(0.020)
1(Elementary & Middle School)	0.020	0.021	0.004		−0.007
	(0.026)	(0.026)	(0.019)		(0.031)
1(Middle School)	−0.032***	−0.033***	−0.034***		−0.022
	(0.005)	(0.005)	(0.005)		(0.017)
Fixed Effects		DMA	District	School	Principal
F(All Fixed Effects = 0)		9.159***	2.940***	47.71***	36,163***
Observations	7,421	7,421	7,421	7,445	7,421
R-squared	0.033	0.042	0.083	0.204	0.285

Note: *** p< 0.01, ** p<0.05, * p<0.1. Dependent variable is a binary indicator for whether CRE visits a school in a year. Robust standard errors in parentheses are clustered at the school level. All specifications control for year fixed effects.

Notably, the treated group includes students in grades 3 to 5 who participated in the CRE programs in the previous year (*i.e.*, in grades 2 to 4), about 80% of whom are third graders. The control group consists of students in schools not visited in the previous year, regardless of the visit status of other years, and students in schools visited the year prior but not in the grades attending the event.[13] The effect of CRE, β_1, is identified by comparing the cross-grade-year variation in the test scores of a school that receives the treatment in a certain year and grade and the variation of a school that does not receive the treatment in the same year and grade. I estimate Eq. (1) using OLS and cluster standard errors by school and grade as the treatment varies at that level.[14] In some specifications, I also allow the effect of CRE to differ across student socioeconomic status by adding an interaction between CRE_{gst-1} and a binary indicator for student i's SNAP or TANF receipt status to the regression.

5.2. Identification

A primary concern is whether CRE's school choice is endogenous to unobserved school traits that impact student performance. I have taken several steps to address this issue.

I start with regressing a binary indicator for CRE visits on a series of school characteristics measured in the year prior to the visit to understand what type of schools are more likely to receive a CRE visit. Table 3 presents the results estimated using the OLS. The sample includes all the elementary and middle schools in South Carolina from 2008 to 2016. The first column includes only the observed school characteristics. Columns 2 to 4 introduce market area (DMA) fixed effects,[15] school district fixed effects, and school fixed effects to the regression, respectively. The last column controls for principal fixed effects, as personal connections may play an important role in setting up these visits besides the features of a school. Year fixed effects are included in all the specifications.

The estimates suggest that CRE is more likely to go to schools with more severe student poverty. There is also evidence that CRE pays more frequent visits to smaller and traditional schools with a higher share of minority students and a larger pupil-teacher ratio –schools that likely have fewer resources. However, the corresponding coefficients are not significant in all the specifications. As children in pre-K to the second grade are the primary targets of CRE, middle schools receive significantly fewer visits than elementary schools or schools with an elementary section. Notably, while the likelihood of visits in the current year appears lower for schools that were not visited in the past three years in Columns 1 and 2, the likelihood is estimated to be higher when controlling for school fixed effects or principal fixed effects in Columns 4 and 5. These estimates suggest that CRE focuses on a particular set of schools. Hence, schools outside this set, or those not visited before, would be less likely to receive a CRE visit. Within the set of schools that possibly meet the selection criteria of CRE, CRE prioritizes those that have not been seen for a while. The F-statistics for various fixed effects included in Columns 2–5 imply significant heterogeneity in CRE visits across DMAs, districts, schools, and principals.

[13] As the control group includes various subgroups of students, I also test a triple difference specification. More discussions are in Section 6.2.1.

[14] As an alternative, I test specifications that cluster the standard errors at the school level to account for the nesting of students in schools. Also, it is usually a school, rather than a grade, that decides with CRE to hold a reading event. Moreover, I test clustering the standard errors at the student level, given the repeated observations of students over the years. The results are very similar and available upon request.

[15] A designated market area (DMA) is a region that receives the same (or similar) television and radio station offerings. A DMA can coincide or overlap with one or more metropolitan areas.

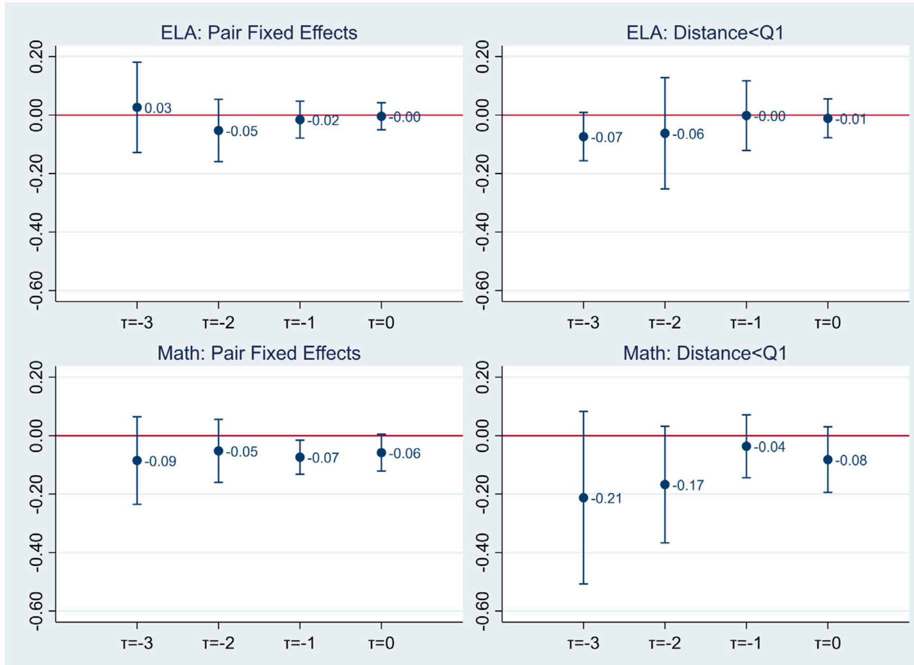

Fig. 1. Pre-existing trends in test scores.
Note: Bars stand for the 95% confidence intervals. The analysis is conducted at the school-grade-year level. The outcome is ELA scores in the top panels and math scores in the bottom ones.

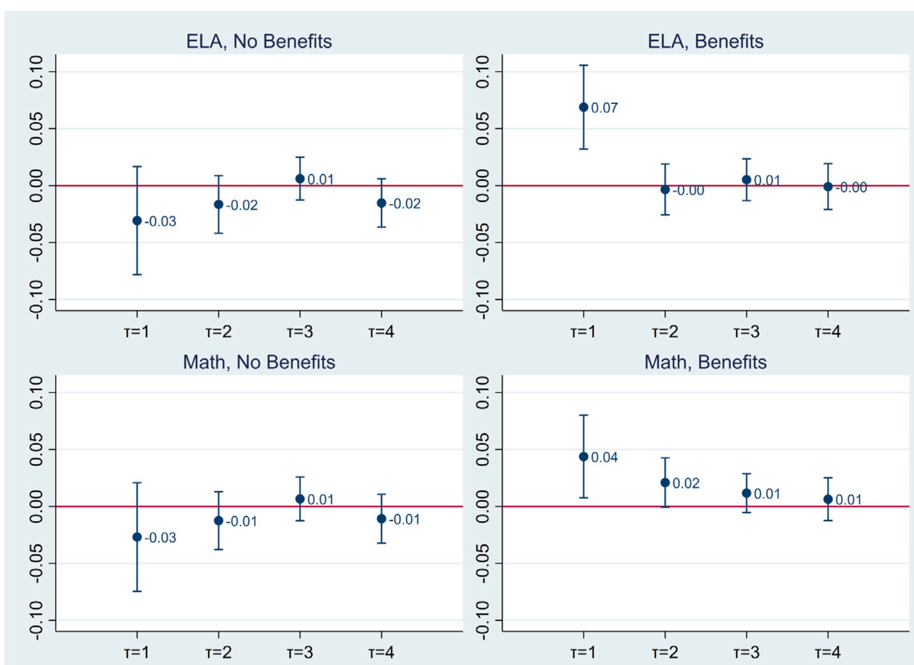

Fig. 2. Long-run effect of CRE: Pair fixed effects.
Note: Bars stand for the 95% confidence intervals. The outcome is ELA scores in the top panels and math scores in the bottom ones. Robust standard errors in parentheses are clustered at the school-grade level. All specifications control for gender, benefit recipient status, race, student-teacher ratio, the share of teachers with advanced degrees, expenditure per pupil, average teacher salary, the share of students on benefit in a school, whether the school is a Title I school, grade-by-year fixed effects, and school fixed effects. The estimation sample includes 675,092 observations.

Next, given the essential difference between CRE-visited schools and those not visited, I employ the approach of nearest-neighbor matching. In particular, I match a treated school to the "nearest neighbor" school in the control group. The matching is based on the total enrollments, the share of students on social benefits, the racial and ethnic composition of the student body (*i.e.*, the share of blacks and the share of Hispanics), the inputs to education (*i.e.*, the student-teacher ratio, the share of teachers with advanced degrees, expenditures per student, and whether the school provides the Title I program), the urbanicity of the school, school level, whether the school is traditional, the DMA of the school, and year. That is, I am comparing students in two schools of the same type and level that are geographically close to each other,

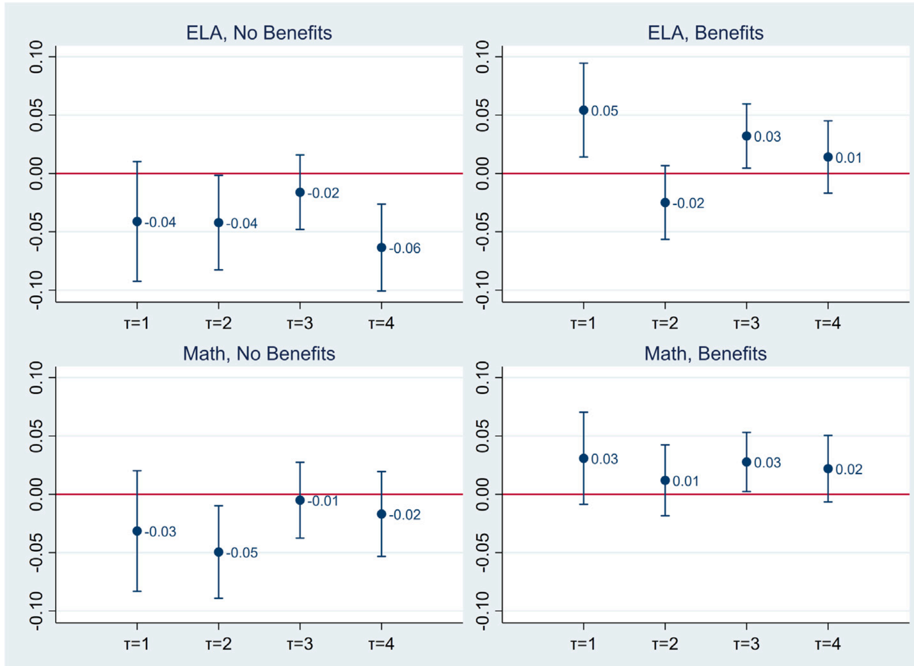

Fig. 3. Long-run effect of CRE: Distance<Q1.
Note: Bars stand for the 95% confidence intervals. The outcome is ELA scores in the top panels and math scores in the bottom ones. Robust standard errors in parentheses are clustered at the school-grade level. All specifications control for gender, benefit recipient status, race, student-teacher ratio, the share of teachers with advanced degrees, expenditure per pupil, average teacher salary, the share of students on benefit in a school, whether the school is a Title I school, grade-by-year fixed effects, and school fixed effects. The estimation sample includes 223,677 observations.

have comparable student demographics, and garner a similar amount of resources. The only difference is that the CRE visited one school in the previous year but not the other. Hence, any difference in the changes in student test scores between the two schools is likely due to the CRE visit.[16]

After performing the nearest-neighbor matching, I consider two methods to balance the treated and control groups. First, I add matched-pair fixed effects to Eq. (1). The pair fixed effects should absorb any unobserved common determinants for test scores of a pair of a treated school and a control school. Second, I derive the nearest-matching distance between a matched pair of schools. Then I trim the sample to include only the schools with a distance below a certain threshold so that the treated and control schools remaining in the sample are more comparable. I use the lower quartile as the cutoff in the primary analysis and test alternative cutoffs as a robustness check.[17] Admittedly, the sample trimming may also limit the external validity of the analysis, as only schools with specific features are evaluated.[18]

Finally, I check the pre-existing trends in test scores of the treated and control students. Fig. 1 depicts the estimates: the point coefficients capture the difference in the scores between the two groups in the periods leading to a CRE visit and the year of the visit; the bars represent the 95% confidence interval.[19] The upper row looks at the ELA scores and the bottom math scores. The left two panels examine the full sample controlling for the matched-pair fixed effects; the right two are estimated based on the trimmed sample where the nearest-matching distance is in the bottom quartile. These figures suggest that the treated and control students likely experience parallel trends in the test scores before a CRE visit. I also examine whether the pre-trends in student demographics and school resources differ for the two groups. As shown in Appendix Figs. A.1 and A.2, there are no persistent differences between the treatment and control groups in these factors either. These findings may help alleviate the concern of endogenous school selection.[20]

6. Empirical results

6.1. Baseline results

I start with investigating the effect of CRE on student English Language Arts (ELA) test scores and math test scores based on Eq. (1). The regression results are presented in Table 4. The outcome is the normalized ELA test score in Panel A (normalized by grade and year) and the normalized math score in Panel B. A binary indicator for whether CRE visits a school-grade in the past year is defined as the treatment. For each outcome, I estimate the regressions on the whole sample first, add the matched-pair fixed effects next, and restrict the sample to schools

[16] Appendix A.1 provides more technical details of nearest-neighbor matching.

[17] Results of these tests are in Appendix Table A.3 and reassuring.

[18] To evaluate to what extent the nearest-neighbor matching method improves the comparability of the treated and control schools, I rerun the regressions in Table 3 controlling for the matched-pair fixed effects and rerun these regressions on a trimmed sample with the nearest-neighbor distance in the bottom quartile.Appendix Table A.4 presents the regression results. Though middle schools (which are excluded from the primary analysis) and schools not visited in the past three years continue to show a differential propensity to be visited than other schools, few other coefficients are statistically significant, suggesting the method's effectiveness.

[19] I only consider the periods leading to the first CRE visit to a student when no one is ever exposed. Indeed, among the students who have ever participated in the CRE program, 99.7% did it once and the rest twice.

[20] Because CRE does not show an impact on students from better-off families, I check the treated-control difference in the test scores during the periods leading to a CRE visit of students on social benefits only. Appendix Fig. A.3 displays the estimated pre-existing trends for this demographic group. Again, there is no statistically significant difference in the ELA or math scores between the treated and untreated students.

Table 4
CRE visits and student performance.

	Full sample		Pair fixed effects		Distance <Q1	
Panel A: DV = ELA Score	(1)	(2)	(3)	(4)	(5)	(6)
CRE_{t-1}	−0.000	−0.011	0.006	−0.011	0.012	−0.003
	(0.008)	(0.011)	(0.008)	(0.011)	(0.009)	(0.013)
CRE_{t-1}×Benefit		0.022		0.033**		0.029*
		(0.015)		(0.015)		(0.016)
Benefit	−0.362***	−0.362 ***	−0.364***	−0.365 ***	−0.353***	−0.354***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Observations	1,304,532	1,304,532	1,123,673	1,123,673	365,015	365,015
R-squared	0.210	0.210	0.211	0.211	0.195	0.195
Effect on Students w/ Benefits		0.011		0.022**		0.027**
		(0.010)		(0.011)		(0.012)
Panel B: DV = Math Score	(1)	(2)	(3)	(4)	(5)	(6)
CRE_{t-1}	−0.011	−0.021*	0.000	−0.011	0.009	0.002
	(0.008)	(0.011)	(0.008)	(0.011)	(0.009)	(0.013)
CRE_{t-1}×Benefit		0.019		0.022		0.013
		(0.015)		(0.015)		(0.016)
Benefit	−0.352***	−0.352 ***	−0.356***	−0.356 ***	−0.339***	−0.340***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Observations	1,304,532	1,304,532	1,123,673	1,123,673	365,015	365,015
R-squared	0.213	0.213	0.213	0.213	0.203	0.203
Effect on Students w/ Benefits		−0.002		0.012		0.016
		(0.010)		(0.010)		(0.012)

Note: *** p< 0.01, ** p<0.05, * p<0.1. Dependent variable is normalized ELA scores in Panel A and math scores in Panel B. Robust standard errors in parentheses are clustered at the school-grade level. All specifications control for gender, benefit recipient status, race, student-teacher ratio, the share of teachers with advanced degrees, expenditure per pupil, average teacher salary, the share of students on benefit in a school, whether the school is a Title I school, grade-by-year fixed effects, and school fixed effects. The last two rows of each panel report the estimated total effect on students with benefits and the standard errors.

with a nearest-neighbor distance in the bottom quartile last. I estimate the overall effect of CRE on test scores in odd-numbered columns and allow the effect to differ by student social benefit receipt status in even-numbered columns. Because the interaction between the CRE measure and the binary indicator for benefits captures the difference in the CRE effect between students with and without benefits, I calculate the total effects of CRE on students receiving SNAP or TANF and report them with the standard errors in the last two rows of each panel. I also test specifications that use the number of books gifted per student and the total number of CRE visits in the prior year to measure the treatment intensity. Appendix Table A.5 displays the estimates. Notably, because CRE usually gives each participant one book in a reading event and rarely pays multiple visits to a school in a single year, the first measure is highly correlated with the other two (with a correlation coefficient of 0.83 and 0.96, respectively).

The estimates in the odd columns imply that CRE visits do not significantly impact student test scores overall. However, when distinguishing the effects across household incomes, I find that CRE exerts a significantly more positive effect on the ELA score of students from households receiving TANF or SNAP than those not on social benefits. Indeed, CRE leads to a statistically significant increase in the ELA score of students on benefits when the pair fixed effects are controlled for (Column 4) and in the trimmed sample (Column 6). Specifically, low-income students visited by CRE enjoy 0.02–0.03 standard deviation higher ELA scores in the following year than those not visited. Considering students gain an average of 0.60 standard deviations on nationally normed standardized reading tests between the spring of the second grade and that of the third grade (Lipsey et al., 2012), the CRE-associated test score increase accounts for about a 5% improvement over the annual gain otherwise expected for a second grader.[21] Hence, the effect of CRE is economically meaningful in an elementary school context.

The estimates for math scores, as shown in Panel B, exhibit a similar pattern as those for ELA scores in Panel A. However, the former are consistently smaller than the latter and not statistically significant. Notably, the number of visits shows a significant effect on the math scores of low-SES students in Appendix Table A.5: one additional CRE visit is related to a 0.01–0.02 standard deviation increase in the math score, an impact of a similar magnitude as that on the ELA score. While assemblies may promote motivation more effectively than book gifts, fully distinguishing the effects of reading sessions and book gifts is difficult, given the high correlations between the two measures.

Moreover, CRE visits do not significantly impact non-benefit-receiving students for either subject. Students from better-off households may already have access to books at home, and their parents may know the importance of reading before the CRE events. Therefore, the school visit from CRE does not necessarily alter these students' reading behaviors at school or home. In contrast, students who live in poverty may have few to no books at home prior to CRE. Therefore, the books they get from CRE can make a noticeable difference in their home literacy environment. The book ownership and the information delivered by Cocky and the college students at the reading event may encourage these children to read more often and incentivize their parents to read to them. Such changes may eventually translate to improvements in test scores.

6.2. Robustness checks

6.2.1. Triple difference results

An alternative model to estimate the effect of CRE is a triple difference specification. CRE selects schools to visit. All students in pre-K to grade 2 in a chosen school would be included in the program unless the school makes special arrangements. Accordingly, I restrict the treatment group to schools where only pre-K through to the second grade attended the assemblies. I consider an extra comparison group of students in grades 4 and 5 of a visited school (who were in grades 3 and 4 during the year of exposure) and estimate the following equation :

$$Y_{igst} = \gamma_1 CRE\_School_{st-1} + \gamma_2 CRE\_School_{st-1} \times 1\,(Grade3)_g + X'_{it}\gamma_3 + Z'_{st}\gamma_4 + \alpha_s + \delta_{gt} + \varepsilon_{igst}. \quad (2)$$

[21] The percentage increase may be higher for low-income students as they experience lower-than-average learning gains.

Table 5
Triple difference specification.

	ELA			Math		
	Full sample	Pair fixed Effects	Distance < Q1	Full sample	Pair fixed effects	Distance <Q1
	(1)	(2)	(3)	(4)	(5)	(6)
CRE School$_{t-1}$	−0.003	−0.000	0.008	−0.018***	−0.014***	−0.003
	(0.003)	(0.003)	(0.005)	(0.003)	(0.003)	(0.005)
CRE School$_{t-1}$×1(Grade 3)	−0.010	−0.011	−0.005	−0.012	−0.005	0.004
	(0.011)	(0.011)	(0.013)	(0.011)	(0.011)	(0.013)
CRE School$_{t-1}$×Benefit	−0.362 ***	0.002	−0.008	−0.356***	0.012***	0.005
	(0.002)	(0.004)	(0.006)	(0.002)	(0.004)	(0.006)
CRE School$_{t-1}$×1(Grade 3)×Benefit	−0.000	0.031**	0.032 *	0.011***	0.015	0.011
	(0.003)	(0.015)	(0.016)	(0.003)	(0.015)	(0.016)
Benefit	0.022	−0.365***	−0.349***	0.012	−0.361 ***	−0.343***
	(0.015)	(0.002)	(0.005)	(0.015)	(0.002)	(0.005)
Observations	1,126,786	953,433	282,737	1,126,786	953,433	282,737
R-squared	0.210	0.211	0.195	0.213	0.213	0.203
Effect on Students w/o Benefits	−0.013	−0.011	0.003	−0.030***	−0.019*	0.000
	(0.011)	(0.011)	(0.013)	(0.011)	(0.012)	(0.013)
Effect on Students w/ Benefits	0.010	0.023**	0.028**	−0.007	0.007	0.017
	(0.010)	(0.011)	(0.012)	(0.010)	(0.011)	(0.012)

Note: *** p< 0.01, ** p<0.05, * p<0.1. Dependent variable is normalized ELA scores in Columns 1–3 and math scores in Columns 4–6. Robust standard errors in parentheses are clustered at the school-grade level. All specifications control for gender, benefit recipient status, race, student-teacher ratio, the share of teachers with advanced degrees, expenditure per pupil, average teacher salary, the share of students on benefit in a school, and whether the school is a Title I school. The last four rows report the estimated total effect on students without and with benefits and the standard errors, respectively.

Here, CRE_School_{st-1} indicates whether CRE visits school s in year $t-1$, and $1\,(Grade3)_g$ is a binary indicator which equals one is student i is in grade 3 and zero otherwise in year t. All the other variables have the same definition as in Eq. (1). Presumably, when CRE visits a school, only the students participating in the program are impacted, but others in the same schools are not. Therefore, γ_1 may capture the bias resulting from the endogenous selection of schools, and γ_2 reflects the effect of CRE on student outcomes.

Table 5 presents the regression results, distinguishing the differential effects of CRE on students according to their welfare status. Columns 1–3 examine the ELA test scores and Columns 4–6 the math scores. For each outcome, the first specification looks at all the schools, the second controls for the matched-pair fixed effects, and the last probes a sample trimmed based on the nearest-neighbor distance. Despite a smaller sample size, the estimates in Table 5 resemble those in Table 4. CRE demonstrates a positive and significant effect on the reading scores of students who receive social benefits in Columns 2–3.

6.2.2. Variation in treatment timing

A further concern with the current model is that the standard TWFE estimators are potentially biased given the staggered timing of treatment and varying treatment effects (Baker et al., 2022; de Chaisemartin & D'Haultfoeuille, 2022). Several recent papers (*e.g.*, Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; and Sun and Abraham, 2021) have proposed methods that handle such bias. Accordingly, I employ the estimator proposed by Callaway and Sant'Anna (2021) (CS estimator) to assess the potential bias in the baseline estimates. The CS estimator uses a "clean" control group consisting of only never treated and not-yet-treated observations. Compared to the others, the CS estimator has the advantage of allowing for covariates.

Because the CS estimator does not allow interaction effects of treatments, I divide the sample based on student benefit receipt status and compute the CS estimator for the two subgroups separately. As before, I consider three specifications: one for the entire sample, one that controls for matched-pair fixed effects in addition, and one for the trimmed sample with the nearest-neighbor distance in the bottom quartile. Table 6 presents the estimated average treatment effect on treated (ATT).

Consistent with Table 4, CRE does not exhibit a significantly positive impact on the test scores of nonpoor students. The ATT of CRE on the ELA and math scores among students who receive SNAP or TANF is positive in all specifications. The estimates are marginally significant in Columns 3 to 5. It is also noticeable that the CS estimates are generally larger in magnitude than the standard TWFE estimates in Table 4. If the one-time CRE event leads to a lasting effect on low-SES students, the standard TWFE estimator likely underestimates the treatment effect because the treated observations are used as the control in the post-treatment periods (*i.e.*, when the treatment turns off).

6.2.3. Other robustness checks

I conduct several additional sensitivity tests to verify the robustness of the baseline results. First, to address the concern that CRE visits lead to a compositional change in the student body, I replicate the regressions in Table 4, replacing school fixed effects with student fixed effects. I also test specifications that control for school-by-grade fixed effects and year fixed effects instead of school fixed effects and grade-by-year fixed effects. Appendix Table A.2 presents the results. Second, I consider two alternative thresholds to trim the sample: the median and the 10th percentile of the nearest-neighbor distance. I rerun the estimations in Table 4 and report the results in Appendix Table A.3. Third, I test two alternative sample criteria and display the results in Appendix Table A.6. (a) Because CRE visits grades higher than the fourth on multiple occasions, I expand the sample to include middle school students. (b) In rare cases, CRE visits the same school or even the same cohort of students multiple times within a year or two due to specific grant criteria or funder requests. To address the concern that such requirements are endogenous to student performance, I exclude the schools visited multiple times in a year from the sample. The estimates from all the robustness checks confirm the baseline results that CRE visits lead to significant improvement in the test scores of low-SES students.

6.3. Long-term effects

In this section, I employ two approaches to investigate whether CRE has a lasting impact on student academic performance. First, I estimate Eq. (1) when adding the second, third, and fourth lags of the CRE treatment to the regression function. Fig. 2 displays the estimation results from the full sample controlling for the matched-pair fixed effects, and Fig. 3 shows the estimates from the trimmed sample. The outcome is the ELA score in the top panels and the math score at the bottom in both figures. The two left panels depict the estimated effect

Table 6
Callaway and Sant'Anna (2021) DID estimates.

	ELA			Math		
	Full sample	Pair fixed effects	Distance <Q1	Full sample	Pair fixed effects	Distance <Q1
Panel A: Students w/o Benefits	(1)	(2)	(3)	(4)	(5)	(6)
CRE_{t-1}	0.018	−0.033	−0.125***	0.075	−0.027	0.036
	(0.059)	(0.058)	(0.027)	(0.101)	(0.058)	(0.025)
Panel B: Students w/ Benefits	(1)	(2)	(3)	(4)	(5)	(6)
CRE_{t-1}	0.030	0.046	0.053*	0.123 *	0.184*	0.059
	(0.061)	(0.130)	(0.032)	(0.074)	(0.104)	(0.037)

Note: *** p< 0.01, ** p<0.05, * p<0.1. Dependent variable is normalized ELA scores in Columns 1-3 and math scores in Columns 4-6. Panel A examines students not on social benefits and Panel B students on benefits. The estimates are computed by the csdid Stata command, which is based on the Callaway and Sant'Anna (2021) estimator. All specifications control for gender, benefit recipient status, race, student-teacher ratio, the share of teachers with advanced degrees, expenditure per pupil, average teacher salary, the share of students on benefit in a school, whether the school is a Title I school, grade-by-year fixed effects, and school fixed effects.

of CRE on students who do not receive SNAP or TANF, and the right two depict those on students with benefits. All the regressions control for student demographics, school characteristics, grade-by-year fixed effects, and school fixed effects. The bars stand for the 95% confidence intervals. Notably, the estimation sample necessarily loses observations with every lag term added.

Both figures show that CRE significantly increases the ELA score of students who receive social benefits in the following year by 0.05–0.07 standard deviations. The increase in the math score of this demographic group (0.04 standard deviations) is also statistically significant when pair fixed effects are controlled for. In Fig. 2, the effect of CRE on test scores is insignificant and close to zero after the first year for ELA; the effect on math scores is marginally significant in the second year post-visit (0.02 standard deviation) but insignificant afterward. Fig. 3 tells a slightly different story: besides an overall declining trend, CRE is associated with a statistically significant 0.03 standard deviation increase in ELA scores three years after the visit; the magnitude of the math score increase stays between 0.01 to 0.03 standard deviations over the years and is significant at the 5% level three years after the visit. Again, there is little evidence that CRE improves the performance of nonpoor students. Indeed, CRE appears to lower the test scores of this demographic group in the trimmed sample in a few cases. For example, CRE is estimated to reduce the ELA scores of non-benefit-receiving students by 0.06 standard deviations four years after the visit and reduce their math scores by 0.05 standard deviations two years after the visit in Fig. 3. Notably, controlling for the pair fixed effects and trimming the sample based on the nearest-neighbor distance balance the treated and control groups differentially (as shown in Figs. 1 and A.1–A.3). The nuanced differences between Figs. 2 and 3 may result from noise in the estimation.

In considering the analysis above, two caveats are in order. First, although there are four treatments, nearest-neighbor matching can only be performed based on a single, specified treatment. Same as the primary analysis, I conduct the matching based on the CRE visit status of the previous year and then generate matched pairs and trim the sample correspondingly. Therefore, the yearly observations of a treated and a control school within a matched pair may become less analogous the longer the lag, especially if the schools experience dramatic changes over the years.[22] The estimated coefficients for the longer lags may be biased as a result. Second, the population of treated students differs with the lag length. For instance, to identify the one-year lagged effect of CRE, the treatment group includes mainly children exposed as second graders; for the two-year lagged effect, most of the group was treated in the first or second grade. Therefore, the estimated long-term effects may reflect the compositional changes of the treatment group or the heterogeneous treatment effects based on the age when treated.

To address these issues, I consider a second approach and introduce the lagged CRE treatment to Eq. (1) one by one, restricting the sample to the cohort who were second graders in the year of exposure.[23] I perform nearest-neighbor matching for each lag length based on that specific lagged treatment, derive the matched pairs, and trim the sample. Table 7 reports the regression results for the first to the fourth lag. These estimates generally conform to those displayed in Figs. 2 and 3. Specifically, the estimated one-year lagged effects on third graders resemble those estimated using all elementary grades in Table 4. However, only the effect on the ELA scores of poor students is marginally significant in Column 2. The lack of variation in a smaller sample may be an explanation. The effect on the ELA scores of low-SES students stays positive but is no longer statistically significant beyond the first post-visit year.

Despite CRE's insignificant impact on the math scores of benefit-receiving students one year after the visit, both the two-year and three-year lagged effects are positive and significant (0.03–0.04 standard deviation). The four-year lagged effect is positive and marginally significant in the trimmed sample but is negative and insignificant when the matched-pair fixed effects are controlled for. The delayed improvement in math relative to ELA outcome may imply that CRE leads to an immediate change in child reading behaviors, and their improved literacy skills later result in better math performance (Glenberg et al., 2012; Grimm, 2008; Hubner et al., 2022).

It is also worth noting that the four-year lagged CRE effects on nonpoor students are negative and significant. Low-SES students may put more effort into reading post-CRE and thus acquire more attention from teachers, which would otherwise be given to nonpoor students. However, the negative estimates may also result from bias. CRE visits are not independent across years. Since CRE tries to avoid reading to the same group of students multiple times, a CRE visit to one school implies a smaller chance of visiting the same school in the following three to five years. Also, a longer lag implies more observations being dismissed from the sample. Because CRE visited fewer schools in their earlier years, the sample size reduces even more after the nearest-neighbor matching procedures. The significantly smaller sample may no longer represent all (underserved) schools in South Carolina and may not compare to the samples for lags for different lengths.

To sum up, there is no strong evidence that CRE results in lasting improvements in the academic performance of low-income students. CRE has the most potent impact on ELA test scores in the year after the visit but becomes less significant in the subsequent years. Relative to the effect on ELA scores, that on math scores takes more time to manifest and seems slightly more persistent.

[22] As a robustness check, I nearest-neighbor match the schools according to the treatment status four years prior and run the same regressions in Figs. 2 and 3. The results are reassuring and available from the author upon request.

[23] In the baseline analysis in Table 4, about 80% of the treated group were exposed in the second grade. Accordingly, I limit the sample to third graders when assessing the one-year lagged effect, fourth graders for the two-year lagged effect, fifth graders for the three-year lagged effect, and sixth graders for the four-year lagged effect.

Table 7
Long-run effect: Second graders when visited.

	ELA		Math	
	Pair fixed effects	Distance <Q1	Pair fixed effects	Distance <Q1
Panel A: One-Year Lag (3rd Graders)	(1)	(2)	(3)	(4)
CRE_{t-1}	−0.015	0.007	−0.011	0.008
	(0.013)	(0.016)	(0.013)	(0.016)
CRE_{t-1}×Benefit	0.033*	0.017	0.027	0.015
	(0.017)	(0.019)	(0.017)	(0.019)
Benefit	−0.368***	−0.351***	−0.357***	−0.340***
	(0.003)	(0.006)	(0.003)	(0.006)
Observations	376,420	123,490	376,420	123,490
R-squared	0.214	0.201	0.211	0.199
Effect on Students w/ Benefits	0.018	0.025*	0.016	0.024
	(0.012)	(0.014)	(0.012)	(0.014)
Panel B: Two-Year Lag (4th Graders)	(1)	(2)	(3)	(4)
CRE_{t-2}	−0.020	−0.008	−0.019	0.001
	(0.015)	(0.018)	(0.015)	(0.018)
CRE_{t-2}×Benefit	0.029	0.015	0.055***	0.040*
	(0.019)	(0.021)	(0.019)	(0.021)
Benefit	−0.370***	−0.365***	−0.358***	−0.339***
	(0.004)	(0.007)	(0.004)	(0.006)
Observations	308,898	98,336	308,898	98,336
R-squared	0.219	0.207	0.233	0.233
Effect on Students w/ Benefits	0.010	0.007	0.036***	0.041***
	(0.014)	(0.016)	(0.013)	(0.015)
Panel C: Three-Year Lag (5th Graders)	(1)	(2)	(3)	(4)
CRE_{t-3}	−0.005	0.005	−0.028*	−0.023
	(0.016)	(0.020)	(0.016)	(0.019)
CRE_{t-3}×Benefit	0.028	0.010	0.072***	0.055**
	(0.021)	(0.024)	(0.020)	(0.023)
Benefit	−0.370***	−0.362***	−0.369***	−0.352***
	(0.004)	(0.007)	(0.004)	(0.007)
Observations	252,687	80,156	252,687	80,156
R-squared	0.215	0.209	0.222	0.221
Effect on Students w/ Benefits	0.023	0.015	0.044***	0.032*
	(0.016)	(0.019)	(0.015)	(0.017)
Panel D: Four-Year Lag (6th Graders)	(1)	(2)	(3)	(4)
CRE_{t-4}	−0.066***	−0.088*	−0.101***	−0.049
	(0.020)	(0.050)	(0.020)	(0.051)
CRE_{t-4}×Benefit	0.080 ***	0.149**	0.090***	0.128**
	(0.026)	(0.060)	(0.026)	(0.059)
Benefit	−0.390***	−0.315***	−0.387***	−0.302***
	(0.005)	(0.019)	(0.005)	(0.018)
Observations	189,926	10,475	189,926	10,475
R-squared	0.228	0.190	0.236	0.213
Effect on Students w/ Benefits	0.013	0.061	−0.011	0.079 *
	(0.019)	(0.047)	(0.018)	(0.044)

Note: *** p< 0.01, ** p<0.05, * p <0.1. Dependent variable is normalized ELA scores in Columns 1-2 and math scores in Columns 3-4. Robust standard errors in parentheses are clustered at the school-grade level. All specifications control for gender, benefit recipient status, race, student-teacher ratio, the share of teachers with advanced degrees, expenditure per pupil, average teacher salary, the share of students on benefit in a school, whether the school is a Title I school, grade-by-year fixed effects, and school fixed effects. The last two rows of each panel report the estimated total effect on students with benefits and the standard errors.

6.4. Heterogeneous effects

To further probe the mechanisms of how CRE improves student performance, I inspect the heterogeneous effects of CRE on student academic performance based on resources In this section. I first examine the effect of CRE by school locality, which may imply differential access to community literacy resources. Then I investigate how the effect varies according to the available materials at one's school library and the proximity to a public library.

6.4.1. Metropolitan status

One possible source of differential treatment effects of CRE may be the locality of students. Compared to rural students, metropolitan residents may have more opportunities for cultural events and community-based literacy programs (Cartwright & Allen, 2002; Miller et al., 2019); they may also have access to a larger collection of books through public libraries, schools, and communities. Therefore, students in rural versus urban schools may benefit from CRE events differently.

Accordingly, I define students who go to schools within Greenville-Spartanburg, Columbia, Charleston, and the outskirts of Charlotte, the state's largest metropolitan areas, as in metro areas and define students elsewhere as in non-metro areas. About 80% of students are classified into the former group. To capture the heterogeneous effects of CRE in metro and non-metro areas on students from different socioeconomic backgrounds, I interact a binary indicator for metro areas with the CRE measure, the benefit recipient dummy, as well as their product. I introduce these interactions to Eq. (1). Table 8 presents the estimation results.

The first two columns examine the CRE impact on ELA test scores and the latter two math scores. The main effect of CRE reflects its impact on nonpoor students who do not reside in metropolitan areas. The last several rows report the calculated total effects on the other three subgroups (*i.e.*, nonpoor students in metro areas, poor students in

Table 8
Heterogeneous effects by school locality.

	ELA		Math	
	Pair fixed effects	Distance < Q1	Pair fixed effects	Distance <Q1
	(1)	(2)	(3)	(4)
CRE_{t-1}	0.009	−0.004	0.001	−0.011
	(0.023)	(0.027)	(0.023)	(0.027)
CRE_{t-1}×Benefit	−0.008	−0.016	−0.006	−0.024
	(0.030)	(0.034)	(0.030)	(0.034)
CRE_{t-1}×Metro	−0.026	0.001	−0.015	0.017
	(0.027)	(0.030)	(0.026)	(0.030)
CRE_{t-1}×Benefit×Metro	0.052	0.059	0.035	0.047
	(0.035)	(0.039)	(0.035)	(0.039)
Benefit	−0.346***	−0.340***	−0.337***	−0.323***
	(0.004)	(0.007)	(0.004)	(0.007)
Benefit×Metro	−0.024***	−0.018**	−0.024***	−0.021***
	(0.004)	(0.008)	(0.004)	(0.007)
Observations	1,123,673	365,015	1,123,673	365,015
R-squared	0.211	0.195	0.213	0.203
Effect on Students w/o Benefits in Metro Areas	−0.017	−0.003	−0.014	0.005
	(0.013)	(0.014)	(0.013)	(0.014)
Effect on Students w/ Benefits in Non-metro Areas	0.001	−0.020	−0.005	−0.035
	(0.021)	(0.025)	(0.021)	(0.024)
Effect on Students w/ Benefits in Metro Areas	0.027**	0.039***	0.015	0.029**
	(0.012)	(0.013)	(0.012)	(0.013)
Effect on Students w/ Benefits: F(Metro = Non-metro)	0.026	0.059**	0.021	0.064**

Note: *** p< 0.01, ** p<0.05, * p<0.1. Dependent variable is normalized ELA scores in Columns 1–2 and math scores in Columns 3-4. Robust standard errors in parentheses are clustered at the school-grade level. All specifications control for gender, benefit recipient status, race, student-teacher ratio, the share of teachers with advanced degrees, expenditure per pupil, average teacher salary, the share of students on benefit in a school, whether the school is a Title I school, grade-by-year fixed effects, and school fixed effects. The last seven rows report the estimated total effect on subgroups with the standard errors and the F-stat for whether CRE has the same impact on students with benefits in metro and non-metro areas.

Table 9
Heterogeneous effects by school library resources.

	ELA		Math	
	Pair fixed effects	Distance <Q1	Pair fixed effects	Distance <Q1
	(1)	(2)	(3)	(4)
CRE_{t-1}	0.007	0.007	0.011	0.020
	(0.015)	(0.017)	(0.015)	(0.018)
CRE_{t-1} × Benefit	0.006	0.006	−0.007	−0.009
	(0.019)	(0.022)	(0.020)	(0.022)
CRE_{t-1} × Low-resource	−0.034	−0.016	−0.041*	−0.032
	(0.023)	(0.025)	(0.023)	(0.025)
CRE_{t-1} × Benefit × Low-resource	0.058*	0.046	0.066 **	0.054
	(0.031)	(0.034)	(0.031)	(0.034)
Benefit	−0.353***	−0.343 ***	−0.337***	−0.324 ***
	(0.003)	(0.005)	(0.003)	(0.005)
Benefit×Low-resource	−0.028***	−0.027***	−0.043***	−0.037***
	(0.004)	(0.007)	(0.004)	(0.007)
Observations	1,050,094	334,025	1,050,094	334,025
R-squared	0.207	0.190	0.209	0.196
Effect on Students w/o Benefits in Low-resource Schools	−0.027	−0.009	−0.031*	−0.013
	(0.018)	(0.019)	(0.017)	(0.018)
Effect on Students w/ Benefits in High-resource Schools	0.013	0.014	0.004	0.010
	(0.013)	(0.015)	(0.013)	(0.015)
Effect on Students w/ Benefits in Low-resource Schools	0.036**	0.043**	0.029*	0.032*
	(0.018)	(0.020)	(0.018)	(0.019)
Effect on Students w/ Benefits: F(Low- = High-resource)	0.024	0.030	0.025	0.022

Note: *** p< 0.01, ** p<0.05, * p<0.1. Dependent variable is normalized ELA scores in Columns 1–2 and math scores in Columns 3-4. Robust standard errors in parentheses are clustered at the school-grade level. All specifications control for gender, benefit recipient status, race, student-teacher ratio, the share of teachers with advanced degrees, expenditure per pupil, average teacher salary, the share of students on benefit in a school, whether the school is a Title I school, grade-by-year fixed effects, and school fixed effects. The last seven rows report the estimated total effect on subgroups with the standard errors and the F-stat for whether CRE has the same impact on students with benefits in low- and high-resource schools.

non-metro areas, and poor students in metro areas) and the F-statistic for the difference in the effect on poor students in metro versus non-metro areas. Neither the estimated main effect nor its interaction effects are significant. However, among the four subgroups, poor students in metropolitan areas appear to experience the greatest and statistically significant test score increases (0.03 to 0.04 standard deviations in ELA and 0.01 to 0.03 standard deviations in math). The estimated score improvements among metro students on social benefits are also significantly larger than students from similarly poor households in non-metro areas at the 5% significance level in the trimmed sample.

Table 10
Heterogeneous effects by public library locations.

Presence of public libraries	In the same city				Within One mile			
	ELA		Math		ELA		Math	
	Pair fixed effects	Distance <Q1	Pair fixed effects	Distance <Q1	Pair fixed effects	Distance <Q1	Pair fixed effects	Distance <Q1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CRE_{t-1}	−0.068***	−0.028	−0.099***	−0.022	−0.024*	−0.022	−0.020	−0.008
	(0.025)	(0.027)	(0.025)	(0.027)	(0.013)	(0.014)	(0.013)	(0.014)
CRE_{t-1}×Benefit	0.065*	0.019	0.054	−0.014	0.033*	0.038**	0.007	0.008
	(0.034)	(0.037)	(0.033)	(0.036)	(0.017)	(0.019)	(0.017)	(0.019)
CRE_{t-1}×Presence	0.070**	0.031	0.107***	0.031	0.056**	0.082***	0.038	0.047
	(0.028)	(0.031)	(0.028)	(0.030)	(0.027)	(0.030)	(0.027)	(0.030)
CRE_{t-1}×Benefit×Presence	−0.041	0.011	−0.041	0.031	−0.014	−0.048	0.041	0.006
	(0.038)	(0.041)	(0.037)	(0.040)	(0.035)	(0.037)	(0.034)	(0.037)
Benefit	−0.357***	−0.334 ***	−0.348***	−0.314 ***	−0.365***	−0.360***	−0.357***	−0.346***
	(0.005)	(0.009)	(0.005)	(0.008)	(0.002)	(0.004)	(0.002)	(0.004)
Benefit×Presence	−0.009*	−0.022**	−0.009*	−0.029***	0.000	0.027 ***	0.004	0.027***
	(0.005)	(0.009)	(0.005)	(0.009)	(0.005)	(0.008)	(0.004)	(0.007)
Observations	1,123,673	365,015	1,123,673	365,015	1,123,673	365,015	1,123,673	365,015
R-squared	0.211	0.195	0.213	0.203	0.211	0.195	0.213	0.203
Effect on Students w/o Benefits Near Public Libraries	0.002	0.004	0.009	0.009	0.032	0.061**	0.019	0.039
	(0.013)	(0.014)	(0.013)	(0.014)	(0.024)	(0.027)	(0.024)	(0.026)
Effect on Students w/ Benefits Not Near Public Libraries	−0.003	−0.009	−0.045*	−0.036	0.009	0.016	−0.012	−0.000
	(0.025)	(0.027)	(0.024)	(0.027)	(0.013)	(0.014)	(0.012)	(0.014)
Effect on Students w/ Benefits Near Public Libraries	0.026**	0.033***	0.021*	0.025**	0.052***	0.051**	0.067 ***	0.052**
	(0.012)	(0.013)	(0.012)	(0.013)	(0.019)	(0.021)	(0.019)	(0.021)
Effect on Students w/ Benefits: F(Near Libraries = Not Near Libraries)	0.029	0.042	0.066 **	0.062**	0.0433**	0.035	0.079***	0.053 **

Note: *** p< 0.01, ** p<0.05, * p<0.1. Dependent variable is normalized ELA scores in Columns 1–2, 5–6 and math scores in Columns 3–4, 7–8. Robust standard errors in parentheses are clustered at the school-grade level. All specifications control for gender, benefit recipient status, race, student-teacher ratio, the share of teachers with advanced degrees, expenditure per pupil, average teacher salary, the share of students on benefit in a school, whether the school is a Title I school, grade-by-year fixed effects, and school fixed effects. The last seven rows report the estimated total effect on subgroups with the standard errors and the F-stat for whether the CRE effect on students with benefits varies by the proximity to public libraries.

Table A.1
Examples of books that CRE handed out.

Book title	Author	Illustrator	Publisher
10 Things I Can Do to Help My World	Melanie Walsh	Melanie Walsh	Candlewick
Every Little Thing	Cedella Marley	Vanessa Brantley-Newton	Chronicle Books
Happy in Our Skin	Fran Manushkin	Lauren Tobia	Candlewick
I Ain't Gonna Paint No More	Karen Beaumont	David Catrow	HMH Books
The Shocking Truth About Energy	Loreen Leedy	Loreen Leedy	Holiday House
Wemberly Worried	Kevin Henkes	Kevin Henkes	Greenwillow Books
What If You Had Animal Teeth?	Sandra Markle	Howard McWilliam	Scholastic Inc.

Possibly, the CRE event motivates students and their parents to utilize other academic-related resources available in metropolitan areas that do not necessarily exist in non-metro regions. After all, the influence of one-time program participation and a single book gifted by CRE may be limited in their own right. However, such events may have altered the attitudes toward reading among the children and their parents and made them aware of other reading materials they can utilize. Hence, CRE may have a more substantial effect on students in metropolitan areas than elsewhere.

6.4.2. School library resources

Next, I inspect how the effect of CRE varies with the number of available recourses in school libraries. Besides one's own book collection, school libraries may be the most accessible source for students to acquire reading materials. Therefore, I use the number of resources available per student in the school library media center from SC DOE's annual report cards to quantify school library recourses. Notably, school library media centers provide printed materials and audiovisual and computer resources. Because SC DOE did not collect information on school libraries before 2015, I use the number in 2015 to categorize schools. Schools with a per-student number of available resources in their library media center higher than the median are regarded as "high-resource" schools. Those with this number equal to or below the median are "low-resource" schools. Admittedly, this data limitation may lead to the misclassification of schools if there are dramatic changes in the number of library books and other materials during the analysis period prior to 2015.

To distinguish the effect of CRE on students in low- versus high-resource schools, I introduce a binary indicator for low-resource schools to the regressions through interactions and present the regression results in Table 9. The outcome is the ELA score in Columns 1–2 and the math score in Columns 3–4. CRE does not show a significant impact on students in high-resource schools, regardless of the welfare recipient status. In contrast, poor students in low-resource schools demonstrate significant increases in ELA and math scores in the year following a CRE visit (0.04 and 0.03 standard deviations, respectively). Compared to students with an adequate amount of books available at school, the ownership of books from CRE may play a more critical role in establishing a reading habit for those whose school library resources are limited. Nevertheless, the total CRE effect on poor students in low-resource versus high-resource schools does not differ significantly, according to the last row of Table 9. The potential misclassification of schools could bias the estimated between-group difference toward zero.

Notably, there is a marginally significant post-CRE decrease in math scores among nonpoor students in low-resource schools, as estimated in Column 3. One possible explanation is that nonpoor students may have access to fewer books and even get less attention from teachers post-CRE when low-SES students seek more reading materials.

Table A.2
Alternative fixed effects.

	ELA		Math	
	Pair fixed effects	Distance <Q1	Pair fixed effects	Distance <Q1
Panel A: Individual Fixed Effects	(1)	(2)	(3)	(4)
CRE_{t-1}	−0.009	−0.004	−0.004	−0.010
	(0.009)	(0.013)	(0.008)	(0.012)
CRE_{t-1}×Benefit	0.022*	0.025	0.017	0.026 *
	(0.011)	(0.016)	(0.011)	(0.016)
Benefit	−0.003	0.004	−0.001	0.003
	(0.003)	(0.006)	(0.003)	(0.006)
Observations	974,209	252,445	974,209	252,445
R-squared	0.858	0.865	0.870	0.871
Effect on Students w/ Benefits	0.013*	0.022**	0.013	0.016
	(0.008)	(0.011)	(0.008)	(0.010)
Panel B: School-Grade Fixed Effects	(1)	(2)	(3)	(4)
CRE_{t-1}	−0.011	−0.001	−0.011	0.009
	(0.011)	(0.013)	(0.011)	(0.013)
CRE_{t-1}×Benefit	0.033**	0.031*	0.022	0.015
	(0.015)	(0.016)	(0.015)	(0.016)
Benefit	−0.365***	−0.354***	−0.356***	−0.340***
	(0.002)	(0.003)	(0.002)	(0.003)
Observations	1,123,673	410,729	1,123,673	410,729
R-squared	0.211	0.198	0.213	0.210
Effect on Students w/ Benefits	0.022 **	0.032**	0.011	0.024**
	(0.011)	(0.012)	(0.010)	(0.012)

Note: *** p< 0.01, ** p<0.05, * p<0.1. Dependent variable is normalized ELA scores in Columns 1–2 and math scores in Columns 3–4. Robust standard errors in parentheses are clustered at the school-grade level. All specifications control for gender, benefit recipient status, race, student-teacher ratio, the share of teachers with advanced degrees, expenditure per pupil, average teacher salary, the share of students on benefit in a school, and whether the school is a Title I school. In addition, Panel A controls for grade-by-year fixed effects and student fixed effects; Panel B controls for school-by-grade fixed effects and year fixed effects. The last two rows of each panel report the estimated total effect on students with benefits and the standard errors.

Table A.3
Alternative cutoffs for sample trimming.

Cutoff for sample trimming	50th percentile		10th percentile	
	ELA	Math	ELA	Math
	(1)	(2)	(3)	(4)
CRE_{t-1}	−0.012	0.009	−0.014	0.018
	(0.012)	(0.015)	(0.012)	(0.015)
CRE_{t-1}×Benefit	0.040 ***	0.017	0.027*	0.018
	(0.016)	(0.019)	(0.016)	(0.019)
Benefit	−0.362***	−0.359***	−0.350***	−0.350***
	(0.003)	(0.005)	(0.002)	(0.005)
Observations	644,893	143,591	644,893	143,591
R-squared	0.194	0.199	0.198	0.220
Effect on Students w/ Benefits	0.028 ***	0.026*	0.013	0.035**
	(0.011)	(0.014)	(0.011)	(0.014)

Note: *** p< 0.01, ** p<0.05, * p<0.1. Columns 1–2 analyze a sample with the nearest-neighbor matching distance below the median, and Columns 3–4 with the distance below the 10th percentile. Robust standard errors in parentheses are clustered at the school-grade level. All specifications control for gender, benefit recipient status, race, student-teacher ratio, the share of teachers with advanced degrees, expenditure per pupil, average teacher salary, the share of students on benefit in a school, whether the school is a Title I school, grade-by-year fixed effects, and school fixed effects. The last two rows report the estimated total effect on students with benefits and the standard errors.

6.4.3. Public libraries

Public libraries are essential to the literacy development of a community. Public libraries provide an array of books for young readers, hold early literacy programs for children and their parents, and offer space for children to read and play. To better understand the urban-rural difference in the impact of CRE found previously and to learn how CRE interacts with other community literacy resources in improving the reading skills of children, I investigate the heterogeneous effects of CRE according to the availability of public libraries.

I acquire the list of public libraries in South Carolina and their locations.[24] I consider two ways to measure the proximity of public libraries: (1) whether a public library is present in the city where a school is located, and (2) whether a public library is within one mile of a school.[25] On one hand, while a public library serves the general population, its city or county residents usually enjoy the full benefits. On the other hand, as city size varies, small city residents may easily access the libraries in neighboring cities even if there is no public

[24] Source: https://publiclibraries.com/state/south-carolina/

[25] As I have no information on the residential locations of students, I calculate the geographic distance between a public library and the school that a student attends instead. Since the catchment area of an elementary school is typically not large, students who attend schools with a nearby public library should live close to that library. Note that the geographic distance between a school and a library may be shorter than the actual traveling distance from the school to that library.

Table A.4
CRE visits and school characteristics.

Variables	Pair fixed effects					Distance<Q1				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Total Enrollments	0.006	0.010	0.014	0.034	0.028	0.014	0.018	0.041	0.295	0.157
	(0.009)	(0.009)	(0.010)	(0.030)	(0.024)	(0.060)	(0.062)	(0.076)	(0.284)	(0.211)
% Students on Benefits	0.028	0.035	0.025	0.112	0.141 *	0.078	0.149	0.135	0.165	0.729*
	(0.022)	(0.023)	(0.026)	(0.079)	(0.069)	(0.092)	(0.092)	(0.105)	(0.441)	(0.383)
% Black	0.024	0.008	−0.012	0.017	−0.025	0.008	−0.052	−0.120	−0.047	−0.138
	(0.014)	(0.015)	(0.018)	(0.075)	(0.059)	(0.050)	(0.056)	(0.077)	(0.470)	(0.497)
% Hispanic	0.001	−0.013	0.045	−0.002	0.086	0.232	0.169	0.253	0.528	0.616
	(0.024)	(0.027)	(0.032)	(0.109)	(0.115)	(0.147)	(0.149)	(0.174)	(0.775)	(0.590)
Student-Teacher Ratio	−0.000	−0.000	0.001	0.001	−0.000	−0.005	−0.006	−0.002	−0.005	−0.004
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)	(0.004)	(0.004)	(0.006)	(0.007)
% Teachers w/ Advanced Degrees	0.000	−0.000	−0.000	−0.001	−0.001	−0.000	0.000	0.000	−0.002	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
Expenditures per Pupil	0.000	0.001	0.002	−0.000	−0.001	0.010	0.006	0.011	0.010	−0.001
	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.011)	(0.011)	(0.014)	(0.029)	(0.032)
Average Teacher Salary	0.000	0.000	0.000	−0.000	0.000	0.000	−0.000	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
1(Title I School)	0.003	0.004	0.002	−0.002	−0.006	−0.023	−0.016	−0.019	−0.037	−0.033
	(0.006)	(0.006)	(0.005)	(0.007)	(0.008)	(0.031)	(0.032)	(0.033)	(0.060)	(0.068)
1(Non-traditional School)	−0.008	−0.005	0.004	−0.024	0.003	0.121	0.119	0.130*	0.061	0.156
	(0.010)	(0.010)	(0.010)	(0.015)	(0.017)	(0.074)	(0.074)	(0.078)	(0.112)	(0.137)
1(Not Visited in Past 3 Years)	−0.055***	−0.047***	−0.008	0.135***	−0.009	−0.082***	−0.075***	0.018	0.223 ***	0.234***
	(0.014)	(0.013)	(0.011)	(0.015)	(0.011)	(0.028)	(0.028)	(0.027)	(0.044)	(0.054)
1(Elementary & Middle School)	0.019	0.020	−0.000		0.008	0.288	0.294	0.296		0.277
	(0.025)	(0.025)	(0.019)		(0.024)	(0.206)	(0.214)	(0.275)		(0.822)
1(Middle School)	−0.030***	−0.031***	−0.035***		−0.019	−0.060***	−0.056 ***	−0.089***		0.079
	(0.004)	(0.004)	(0.004)		(0.016)	(0.020)	(0.020)	(0.024)		(0.304)
Fixed Effects		DMA	District	School	Principal		DMA	District	School	Principal
Observations	7,415	7,415	7,415	7,439	7,413	1,881	1,881	1,881	1,881	1,880
R-squared	0.089	0.101	0.141	0.259	0.340	0.037	0.048	0.129	0.414	0.504

Note: *** p< 0.01, ** p<0.05, * p<0.1. Dependent variable is a binary indicator for whether CRE visits a school in a year. Columns 1–5 control for matched-pair fixed effects, using the full sample; Columns 6–10 examine a trimmed sample with the nearest-neighbor distance in the bottom quartile. Robust standard errors in parentheses are clustered at the school level. All specifications control for year fixed effects.

Table A.5
CRE Visits and Student Performance.

	ELA						Math					
	Full sample		Pair fixed effects		Distance <Q1		Full sample		Pair fixed effects		Distance<Q1	
Panel A: Books Per Student	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\text{Books/Student}_{t-1}$	−0.003	−0.015	0.001	−0.017	0.008	−0.009	−0.008	−0.016	0.001	−0.008	0.011	0.006
	(0.007)	(0.011)	(0.008)	(0.011)	(0.009)	(0.012)	(0.007)	(0.010)	(0.008)	(0.011)	(0.008)	(0.012)
$\text{Books/Student}_{t-1}\times$Benefit		0.025 *		0.034**		0.031 **		0.015		0.016		0.008
		(0.014)		(0.014)		(0.016)		(0.014)		(0.014)		(0.015)
Benefit	−0.362***	−0.362 ***	−0.364***	−0.365 ***	−0.353***	−0.354***	−0.352***	−0.352***	−0.356***	−0.356***	−0.339***	−0.339***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Observations	1,304,351	1,304,351	1,123,492	1,123,492	364,837	364,837	1,304,351	1,304,351	1,123,492	1,123,492	364,837	364,837
R-squared	0.210	0.210	0.210	0.210	0.195	0.195	0.213	0.213	0.213	0.213	0.203	0.203
Effect on Students w/ Benefits		0.009		0.017*		0.022 **		−0.001		0.008		0.014
		(0.010)		(0.010)		(0.011)		(0.010)		(0.010)		(0.011)
Panel B: No. of Visits	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Visits_{t-1}	−0.004	−0.014	0.000	−0.013	0.006	−0.005	−0.008	−0.024***	−0.001	−0.017**	0.008	−0.007
	(0.006)	(0.009)	(0.006)	(0.009)	(0.007)	(0.010)	(0.006)	(0.008)	(0.006)	(0.009)	(0.007)	(0.009)
$\text{Visits}_{t-1}\times$Benefit		0.020 *		0.027**		0.022*		0.031***		0.032***		0.029**
		(0.011)		(0.012)		(0.013)		(0.011)		(0.012)		(0.013)
Benefit	−0.362***	−0.362 ***	−0.364***	−0.365 ***	−0.353***	−0.354***	−0.352***	−0.353***	−0.356***	−0.356***	−0.339***	−0.340***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Observations	1,304,532	1,304,532	1,123,673	1,123,673	365,015	365,015	1,304,532	1,304,532	1,123,673	1,123,673	365,015	365,015
R-squared	0.210	0.210	0.211	0.211	0.195	0.195	0.213	0.213	0.213	0.213	0.203	0.203
Effect on Students w/ Benefits		0.006		0.014*		0.017 **		0.008		0.015*		0.022**
		(0.008)		(0.008)		(0.009)		(0.008)		(0.008)		(0.009)

Note: *** p< 0.01, ** p<0.05, * p<0.1. Dependent variable is normalized ELA scores in Columns 1–6 and math scores in Columns 7–12. Robust standard errors in parentheses are clustered at the school-grade level. All specifications control for gender, benefit recipient status, race, student-teacher ratio, the share of teachers with advanced degrees, expenditure per pupil, average teacher salary, the share of students on benefit in a school, whether the school is a Title I school, grade-by-year fixed effects, and school fixed effects. The last two rows of each panel report the estimated total effect on students with benefits and the standard errors.

Table A.6
Alternative sample criteria.

	ELA		Math	
	Pair fixed Effects	Distance <Q1	Pair fixed Effects	Distance <Q1
Panel A: Elementary & Middle Schools	(1)	(2)	(3)	(4)
CRE_{t-1}	−0.018*	−0.004	−0.024**	−0.002
	(0.011)	(0.012)	(0.011)	(0.013)
CRE_{t-1}×Benefit	0.043***	0.031*	0.035**	0.017
	(0.014)	(0.016)	(0.014)	(0.016)
Benefit	−0.369***	−0.355***	−0.362***	−0.340***
	(0.001)	(0.003)	(0.001)	(0.003)
Observations	2,194,953	410,731	2,194,953	410,731
R-squared	0.208	0.193	0.215	0.202
Effect on Students w/ Benefits	0.025**	0.027**	0.012	0.011
	(0.010)	(0.010)	(0.012)	(0.011)
Panel B: Schools w/ Single Visit per Year	(1)	(2)	(3)	(4)
CRE_{t-1}	−0.011	−0.002	−0.009	0.006
	(0.012)	(0.013)	(0.012)	(0.013)
CRE_{t-1}×Benefit	0.030*	0.026	0.016	0.005
	(0.015)	(0.016)	(0.015)	(0.016)
Benefit	−0.365***	−0.354***	−0.356***	−0.340***
	(0.002)	(0.003)	(0.002)	(0.003)
Observations	1,121,314	363,991	1,121,314	363,991
R-squared	0.211	0.195	0.214	0.204
Effect on Students w/ Benefits	0.019*	0.025**	0.007	0.011
	(0.011)	(0.012)	(0.011)	(0.012)

Note: *** p< 0.01, ** p<0.05, * p<0.1. Dependent variable is normalized ELA scores in Columns 1–2 and math scores in Columns 3–4. Robust standard errors in parentheses are clustered at the school-grade level. All specifications control for gender, benefit recipient status, race, student-teacher ratio, the share of teachers with advanced degrees, expenditure per pupil, average teacher salary, the share of students on benefit in a school, whether the school is a Title I school, grade-by-year fixed effects, and school fixed effects. The last two rows of each panel report the estimated total effect on students with benefits and the standard errors.

library in a particular city. About 85% of the sample attend schools in cities with at least one public library. About 21% of the students go to schools with a public library within a one-mile radius.[26]

Similar to Tables 8 and 9, I introduce the binary indicators for public library presence in the neighborhood by interacting them with the CRE measure, benefit status, and their product and report the estimates in Table 10. Columns 1–4 examine the presence of public libraries within a city, and Columns 5–8 the library presence within a one-mile radius of a school.

The estimates suggest that CRE exerts a significantly greater effect on students who have easy access to public libraries than those who do not, regardless of household income. Indeed, the former group experiences a significant increase up to 0.06 standard deviation in math scores, as shown in Column 6.

Among the students receiving social benefits, the test score improvements are 0.02–0.06 standard deviation higher in ELA and 0.05–0.08 standard deviation higher in math for students living close to public libraries than those who do not. The effect of CRE is the most positive and significant on students who receive SNAP or TANF and attend schools with a public library in proximity. This group of students experiences an approximate 0.02–0.05 standard deviation increase in their ELA scores and a 0.02–0.07 standard deviation increase in the math scores following a CRE visit. These results confirm the findings in Table 8. As discussed earlier, CRE visits may motivate students to utilize public library services more often. The closer the library is, the more likely the students would use its services (Gilpin & Bekkerman, 2020). Hence, students who reside close to a public library may show larger improvements in reading skills and academic performance than those with limited access to public libraries.

In contrast, CRE has little impact on the academic performance of students who live in poverty but do not have a public library nearby. The scores of the nonpoor students in such schools, especially in the specifications that control for matched-pair fixed effects, seem negatively impacted by CRE. Because the nearest-neighbor matching is performed in the whole sample, the treated and control groups may be less comparable within a subgroup of schools (categorized by public library presence). Consequently, the estimated main effect of CRE may be negatively biased, as CRE purposefully visits disadvantaged schools with more severe student poverty.

Another issue is that public libraries are not created equal. There could be substantial differences in the size and quality of their facilities, book collections, and early literacy programs. As a result, students may benefit differentially from the libraries, and the current analysis cannot capture such discrepancies.

Lastly, I investigate the heterogeneous effects of CRE based on the timing of the visit and the presence of public libraries in proximity. Gilpin and Bekkerman (2020) argue that library visits and intensity grow around school vacations for households with children in schools. I find that CRE visits near the start of a school vacation generally improve student test scores more than earlier visits, except for the ELA scores of poor students who do not reside close to public libraries. This finding may verify that CRE enhances student performance through increased public library usage.[27]

6.5. Discussions

As a reference, the Head Start Impact Study reports an increase of 0.16 to 0.31 standard deviations in the vocabulary achievement scores after one or two academic years in a high-quality Head Start program (Andrews et al., 2012; Peck & Bell, 2014) find that economically disadvantaged students who participated in the Texas targeted

[26] Since the choice of a geographic unit and a distance cutoff is arbitrary, I consider two alternative classifications based on (1) the presence of public libraries in a zip code area and (2) the presence within a five-mile radius from a school as robustness checks. Approximately 70% of the students go to a school with a public library sharing the same zip code, and 85% of the students' schools have one or more public libraries within five miles. I present the estimates in Appendix Table A.7. The results reveal a similar pattern as in Table 10.

[27] Appendix A.2 provides a more detailed discussion of this analysis.

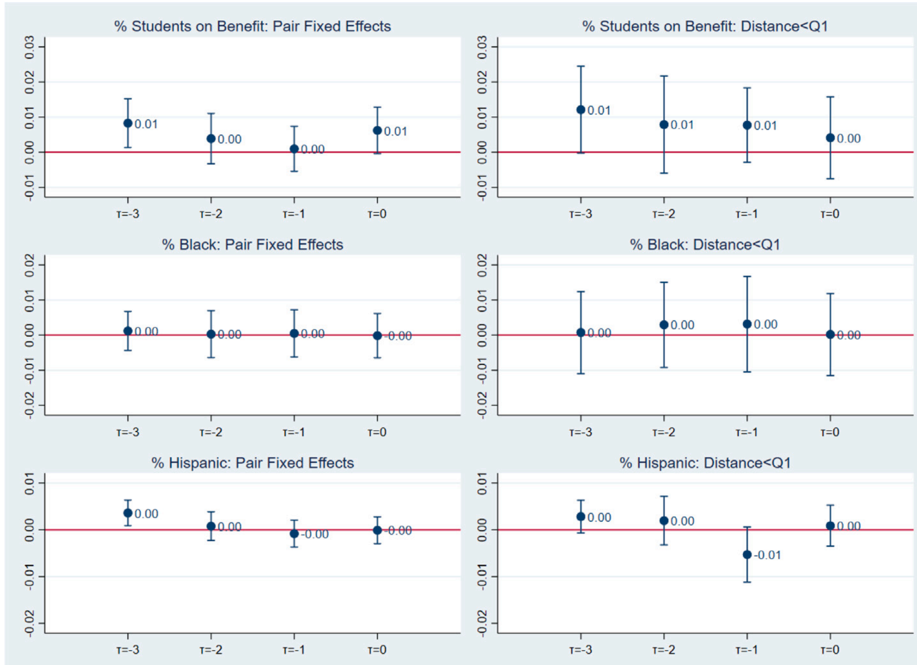

Fig. A.1. Pre-existing Trends in Student Demographics. Note: Bars stand for the 95% confidence intervals. The analysis is conducted at the school-grade-year level.

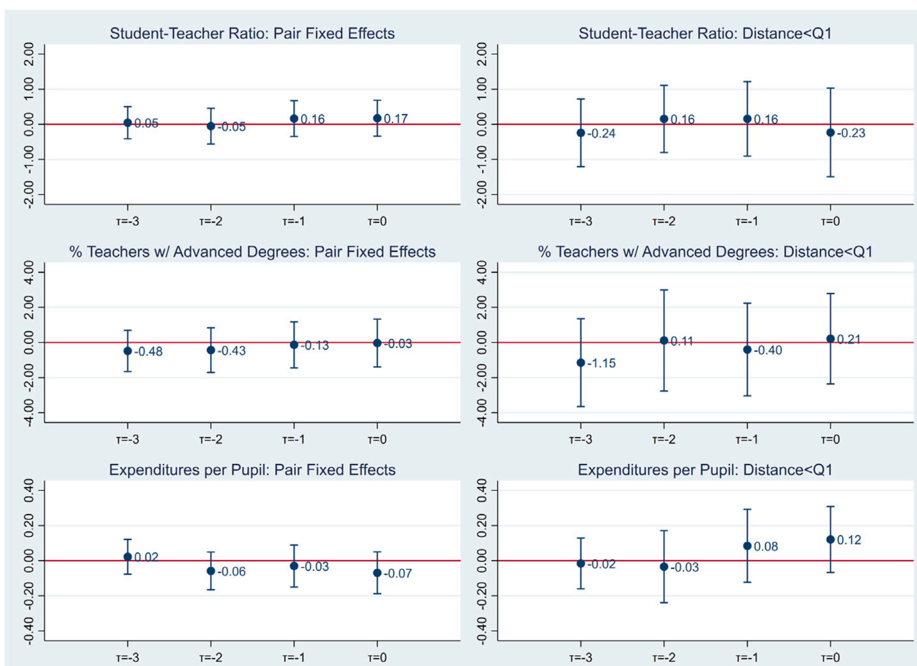

Fig. A.2. Pre-existing Trends in School Characteristics. Note: Bars stand for the 95% confidence intervals. The analysis is conducted at the school-year level.

pre-kindergarten program scored 0.06 standard deviations higher in their third-grade reading test and 0.05 standard deviations higher in the math test than students who did not attend the program; York et al. (2018) show that READY4K!, an eight-month-long text-messaging intervention for parents of preschoolers, leads to child gains in early literacy of about 0.11 standard deviations. Compared to these larger-scale early childhood interventions, as well as some prevalent book giveaway programs, CRE has a much lower treatment intensity but generates meaningful test score improvements among children from benefit-receiving households: a 0.02–0.03 standard deviation increase in the ELA scores in the year following a CRE visit. While the effect is smaller, the CRE program is much less costly than many other intervention programs. Nonetheless, the output and input of CRE are not directly comparable with those of other more sizable interventions, as the programs differ in nature.

Consistent with the finding of several studies on early childhood interventions that their effects on test scores fade out over time (Duncan & Magnuson, 2013; Peck & Bell, 2014), I observe a similar trend for CRE: the impact on the ELA test scores declines after the first year, and that on math scores diminishes after three years. In contrast, the literature suggests the positive effects of resource-intensive model programs persist for roughly ten years (Camilli et al., 2010; Duncan &

Table A.7
Heterogeneous effects by public library locations: Alternative measures.

Presence of public libraries	In the same Zipcode area				Within five miles			
	ELA		Math		ELA		Math	
	Pair fixed effects	Distance <Q1	Pair fixed effects	Distance <Q1	Pair fixed effects	Distance <Q1	Pair fixed effects	Distance <Q1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CRE_{t-1}	−0.077***	−0.058***	−0.067***	−0.034*	−0.058*	−0.051	−0.067**	−0.004
	(0.019)	(0.021)	(0.019)	(0.021)	(0.030)	(0.032)	(0.030)	(0.032)
CRE_{t-1}×Benefit	0.091***	0.089***	0.057**	0.037	0.064	0.059	0.024	−0.021
	(0.026)	(0.028)	(0.026)	(0.028)	(0.041)	(0.045)	(0.040)	(0.043)
CRE_{t-1}×Presence	0.097***	0.085***	0.082***	0.056**	0.053*	0.055	0.064**	0.008
	(0.024)	(0.026)	(0.024)	(0.026)	(0.032)	(0.035)	(0.032)	(0.035)
CRE_{t-1}×Benefit×Presence	−0.087***	−0.091***	−0.053*	−0.037	−0.036	−0.034	−0.003	0.039
	(0.032)	(0.034)	(0.032)	(0.034)	(0.044)	(0.048)	(0.043)	(0.047)
Benefit	−0.365***	−0.356 ***	−0.356***	−0.339 ***	−0.345***	−0.344***	−0.325***	−0.315***
	(0.003)	(0.006)	(0.003)	(0.005)	(0.005)	(0.008)	(0.005)	(0.008)
Benefit×Presence	0.001	0.004	−0.000	−0.001	−0.023***	−0.011	−0.037 ***	−0.029***
	(0.004)	(0.007)	(0.004)	(0.007)	(0.005)	(0.009)	(0.005)	(0.009)
Observations	1,123,673	365,015	1,123,673	365,015	1,123,673	365,015	1,123,673	365,015
R-squared	0.211	0.195	0.213	0.203	0.211	0.195	0.213	0.203
Effect on Students w/o Benefits Near Public Libraries	0.020	0.027*	0.016	0.022	−0.005	0.004	−0.003	0.003
	(0.012)	(0.014)	(0.012)	(0.014)	(0.014)	(0.016)	(0.014)	(0.016)
Effect on Students w/ Benefits Not Near Public Libraries	0.014	0.031	−0.010	0.0032	0.006	0.008	−0.043	−0.025
	(0.031)	(0.034)	(0.029)	(0.032)	(0.019)	(0.020)	(0.018)	(0.020)
Effect on Students w/ Benefits Near Public Libraries	0.024*	0.025*	0.020	0.022	0.024**	0.029**	0.018*	0.022*
	(0.011)	(0.012)	(0.011)	(0.012)	(0.013)	(0.014)	(0.013)	(0.014)
Effect on Students w/ Benefits: F(Near Libraries = Not Near Libraries)	0.010	−0.006	0.029	0.019	0.018	0.022	0.061**	0.047

Note: *** p< 0.01, ** p<0.05, * p<0.1. Dependent variable is normalized ELA scores in Columns 1–2, 5–6 and math scores in Columns 3–4, 7-8. Robust standard errors in parentheses are clustered at the school-grade level. All specifications control for gender, benefit recipient status, race, student-teacher ratio, the share of teachers with advanced degrees, expenditure per pupil, average teacher salary, the share of students on benefit in a school, whether the school is a Title I school, grade-by-year fixed effects, and school fixed effects. The last seven rows report the estimated total effect on subgroups with the standard errors and the F-stat for whether the CRE effect on students with benefits varies by the proximity to public libraries.

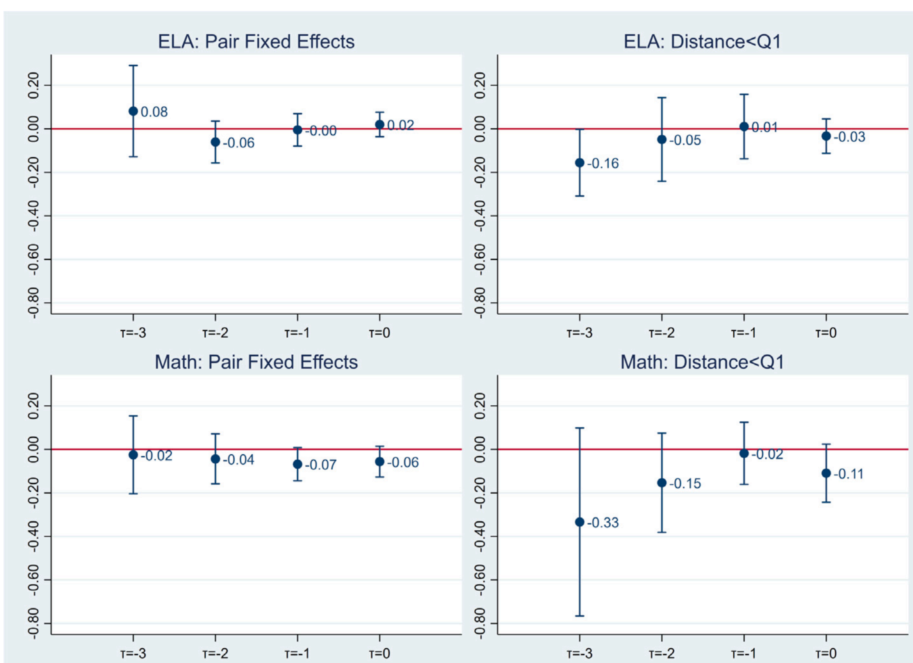

Fig. A.3. Pre-existing Trends in Test Scores of Students on Social Benefits.
Note: Bars stand for the 95% confidence intervals. The analysis is conducted at the school-grade-year level with only students on social benefits included. The outcome is ELA scores in the top panels and math scores in the bottom ones.

Magnuson, 2013). Therefore, follow-up CRE visits may prove helpful in maintaining the improvement in student academic outcomes and reducing the achievement gap.

Besides the academic fade-out, several papers point out the long-term benefits of early childhood intervention programs (*e.g.*, Deming, 2009; Heckman et al., 2010; Chetty et al., 2011). A reading routine established at an early age may facilitate a child's cognitive and socio-emotional developments that determine later-life human capital outcomes, even though test scores do not necessarily reflect all these developments. Reading programs and book gifts may benefit children from better-off family backgrounds in ways other than academic performance. Unfortunately, I do not observe non-academic outcomes of

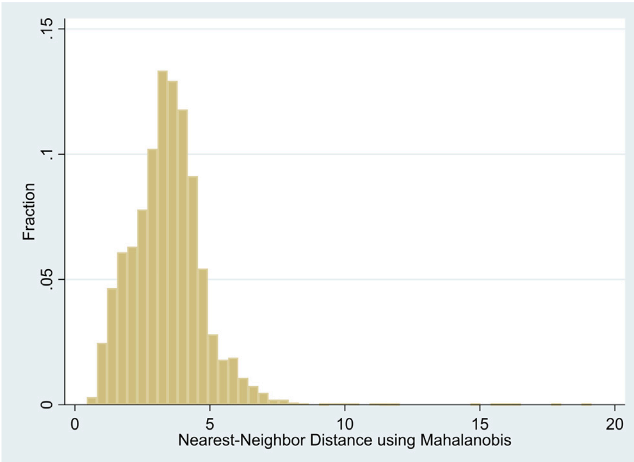

Fig. A.4. Distribution of Nearest-Neighbor Distance.

students in the current datasets. Nor can I examine the impact of CRE on individual life-cycle skill development in the long run.

7. Conclusions

To conclude, this paper assesses the impacts of a literacy intervention program, Cocky's Reading Express (CRE), on student academic performance. There is little evidence that CRE has a positive impact on the test scores of nonpoor students. However, empirical results suggest that CRE leads to statistically significant increases in the ELA and math scores of students whose families receive SNAP or TANF benefits. In particular, low-income students improve their ELA scores by an average of 0.02–0.03 standard deviations in the school year following a CRE visit, but the improvement dissipates afterward; the math scores of this demographic group by 0.03–0.04 standard deviations in the second and the third year post-CRE. These patterns suggest that the CRE program leads to behavioral changes in children and their parents. Enhanced literacy skills result in better performance in math tests. However, these changes are not persistent.

Finally, how CRE affects student outcomes varies by locality and access to community literacy resources. In particular, CRE results in a slightly larger increase in the test scores of low-income students whose school libraries have fewer resources, underscoring the importance of book ownership among students who lack reading materials. The improvements are also more prominent among students residing in metropolitan areas or near public libraries. These results indicate that the reading events and book gifts of CRE motivate children and parents not only to read more but also to actively seek additional reading materials. Therefore, the program is more effective in improving the reading and other cognitive skills of children who have access to books outside schools. Hence, disseminating information about community literacy resources may be equally important as providing free books to children in need, particularly since the CRE treatment is typically one-time per child.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

The authors do not have permission to share data.

Appendix

A.1. Nearest-neighbor matching

To improve the comparability of the treated and control groups, I adopt nearest-neighbor matching. Each school-year observation is matched to the "nearest neighbor" in the other group based on schools' enrollment size, student socio-demographics, inputs to education, school level and type, the metropolitan area of a school, and the urbanicity of the location. Also, all matches are of the same school year (*i.e.*, exact matching in the school year). The "distance" is calculated using a weighted function of the covariates for every observation. In particular, I use the Mahalanobis distance, in which the weights are based on the inverse of the covariates' variance–covariance matrix. Appendix Fig. A.4 displays a histogram of the calculated Mahalanobis distance.

A.2. Visits before school vacations

I classify the CRE visits into two groups: visits within one month prior to school vacations (*i.e.*, visits in November, December, May, and June) and visits in other months and then rerun the regressions in Columns 1–4 Table 10, replacing the general indicator for CRE visits with two separate indicators for the two types of visits. Fig. A.5 depicts the estimates obtained from specifications where the matched pair fixed effects are controlled for, and Fig. A.6 from a nearest-neighbor trimmed sample. In each figure, the four left panels show the results for students residing in cities without public libraries, and the right four show those for students in cities with at least one library; the top panels examine ELA test scores and the bottom ones math scores, distinguishing whether a student receives SNAP or TANF.[28]

The figures suggest that CRE visits near the start of a school vacation generally improve student test scores more than earlier visits, except for the ELA scores of poor students who do not reside close to public libraries. The effect difference between the two types of visits is statistically significant in the ELA and math scores of nonpoor students who live far from public libraries in both figures (the first panel of each row). The difference is marginally significant for poor students in cities with libraries when the pair fixed effects are controlled for (the last panel of each row in Fig. A.5). These patterns conform to the findings of Gilpin and Bekkerman (2020) and may imply that the differential use of public libraries accounts for the heterogeneous CRE effects based on the proximity of public libraries (and to the timing of a visit). For poor students who have a public library close by, CRE proves to be more effective if the visits occur close to the end of a semester, as students are more likely to use public libraries during school vacations. The same thing may happen to nonpoor students who do not live close to a library. They may have limited access to reading materials pre-CRE as there is no public library nearby; they become more motivated to visit libraries post-CRE and are more likely to do so when school is on break. The distance is less of an obstacle for them than for their low-SES peers. Students who receive social benefits but have no public libraries in proximity may be least likely to use library services, regardless of school vacations. Hence, the effect on them may not depend on the timing of the visits.

A.3. Tables and figures

See Tables A.1–A.7 and Figs. A.1–A.6.

[28] I repeat the same regressions considering the presence of public libraries within a one-mile radius of the school a student attends. The results are similar and available upon request.

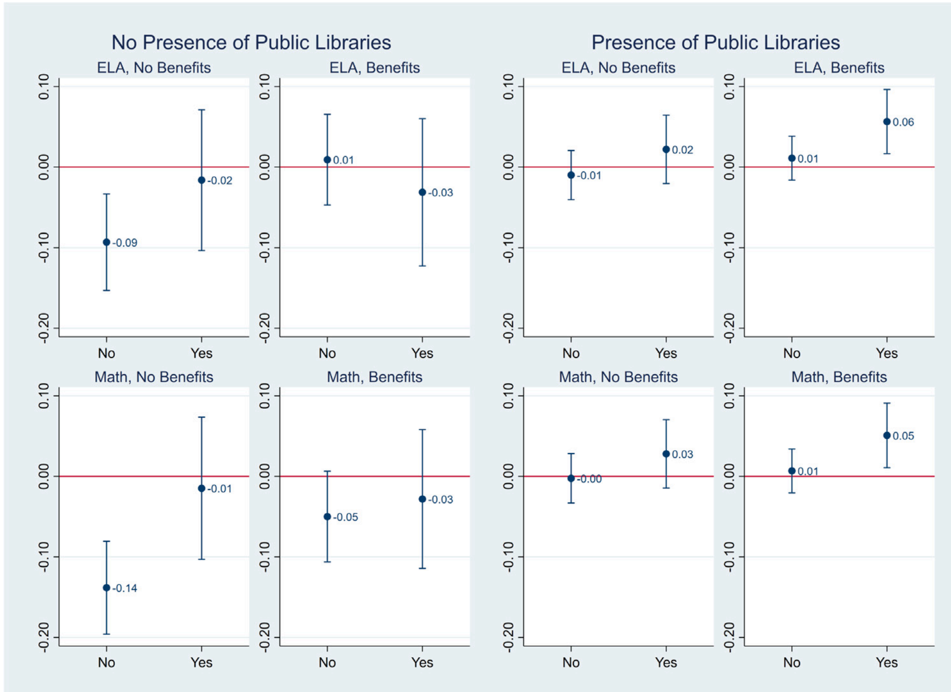

Fig. A.5. Effects by CRE Visit Status Within One Month Pre-Break: Pair Fixed Effects
Presence of Public Libraries within the Same City.
Note: Bars stand for the 95% confidence intervals. The outcome is ELA scores in the top panels and math scores in the bottom ones. Robust standard errors in parentheses are clustered at the school-grade level. All specifications control for gender, benefit recipient status, race, student-teacher ratio, the share of teachers with advanced degrees, expenditure per pupil, average teacher salary, the share of students on benefit in a school, whether the school is a Title I school, grade-by-year fixed effects, and school fixed effects.

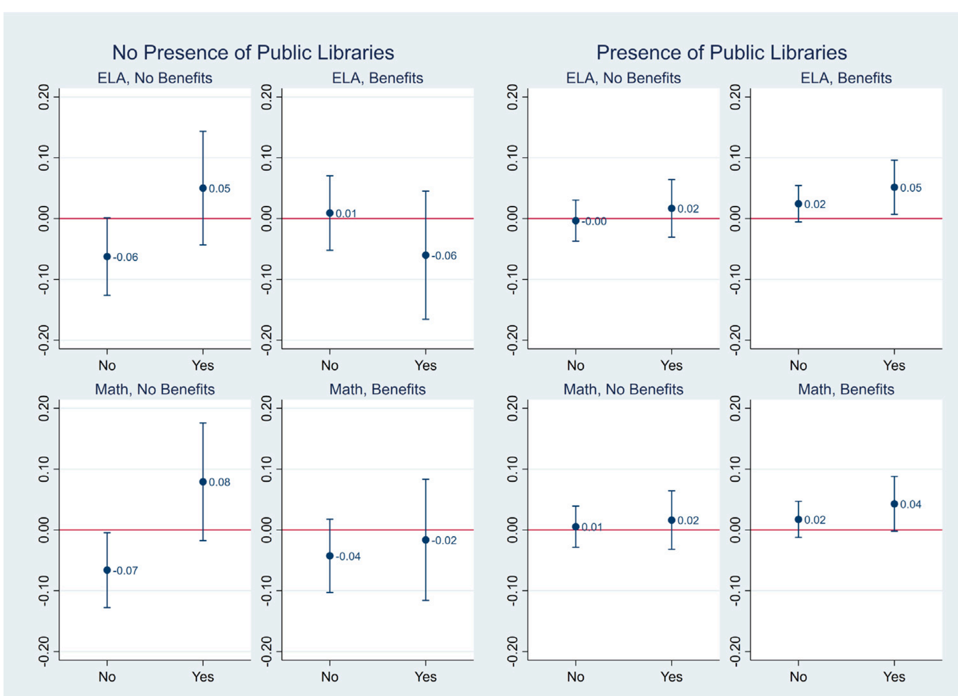

Fig. A.6. Effects by CRE Visit Status Within One Month Pre-Break: Distance <Q1.
Presence of Public Libraries within the Same City.
Note: Bars stand for the 95% confidence intervals. The outcome is ELA scores in the top panels and math scores in the bottom ones. Robust standard errors in parentheses are clustered at the school-grade level. All specifications control for gender, benefit recipient status, race, student-teacher ratio, the share of teachers with advanced degrees, expenditure per pupil, average teacher salary, the share of students on benefit in a school, whether the school is a Title I school, grade-by-year fixed effects, and school fixed effects.

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