It's Dolly's World, We're Just Reading in It: The Effects of an Early Childhood Literacy Program

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Abstract

High quality early childhood interventions play an integral role in early human capital development. However, little is known about the impact that pure capital inputs, such as educational resources, have on early skill development. Using the national roll out of a program that gives children new books for free, I assess how access to the program affects elementary academic achievement. I find that having access to the program before age five leads to small, negative effects on both third and fourth grade English Language Arts and Math standardized tests. Although still small, results from heterogeneity analyses suggest access to the program slightly decreases the achievement of already lower-achieving districts and increases the achievement of higher-achieving districts. After controlling for local participation, the results become a precise null, suggesting that small investments of capital alone are not an effective input into the early human capital production function.

Keywords: Early Childhood Education, Literacy, Human Capital Investment, Imagination Library

JEL Codes: I21, I26, J24

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1 Introduction

It is well-documented that high quality preschool and other early childhood interventions are an integral part of human capital development. Programs that provide childcare have had large impacts on short-term achievement and outcomes measured later in life (Heckman et al., 2010, 2013; Garces et al., 2002; Deming, 2009; Fitzpatrick, 2008). High quality preschool programs that combine both time and capital inputs yield robust early human capital returns. Specifically, preschool caregivers not only spend time with children, but they also use capital, such as toys, books, and other stimulating resources. The use of these inputs individually and in combination supports early skill development, which is critical for later progress in formal schooling (Heckman, 2006).

Less is known about the effects and productivity of capital and labor inputs at home. Examples of inputs at home are parental time, quality of that time, and material resources. Given time and resource constraints on parents, a simple capital investment might seem like the most efficient way to develop early human capital. For example, a program that sends parenting tips and ideas via text messages helps boost not only engagement between parents and children, but also leads to short-term literacy gains (York et al., 2019). Research has not ruled out the effectiveness of a pure capital investment on improving early childhood human capital development. Increasing cash assistance or tax rebates to families with older children improves high-school achievement and later-life human capital (Dahl & Lochner, 2012; Chetty et al., 2011b), so it is plausible that pure capital investments can benefit children's early skill development.

In this paper, I measure the impact of a pure capital investment on early reading and math development. Specifically, I estimate the effects of Dolly Parton's Imagination Library, a childhood literacy program that mails free books to children, on early academic achievement. There are three primary mechanisms through which the books could influence achievement scores. First, having physical books in the home sets the stage for literacy development down the road (Feng et al., 2014; Sénéchal & LeFevre, 2002; Sénéchal et al., 1998). For instance, the number of books in the home is positively correlated with greater levels of educational attainment (Evans et al., 2010). Second, having books around the home encourages a more positive home learning environment, with increased engagement between parents and their children (Sénéchal et al., 1998). A positive home literary environment is correlated with improved child development, seen in increased literacy and numeracy achievement (Melhuish et al., 2008; Bradley et al., 2001; Hood et al., 2008). Third, books affect achievement by encouraging parents to spend more time reading with their children, an activity that has large positive effects on children's literacy development rates (Hood et al., 2008; Bus et al., 1995; Kalb & Van Ours, 2014). Receiving books in the mail from Imagination Library could nudge parents to read to their children. Additionally, access to the program can also provide educational resources that were previously too expensive.

To investigate the effects of the Imagination Library program on educational achievement, I exploit temporal and geographic variation in its national rollout. Imagination Library launched a nationwide expansion program in 2000 and since then, twenty percent of public school districts in the United States have gained access to the program. Using a two-way fixed effects difference-in-differences strategy, I compare outcomes between treated and untreated cohorts while controlling for unobserved heterogeneity across cohorts and school districts. I use a longitudinal panel of educational achievement data combined with treatment status information for each cohort-school district observation. Because I primarily measure access rather than individual treatment, my results represent intent-to-treat estimates, which can be interpreted as a conservative, lower bound of the true individual treatment effect.

I find that granting free books to children has uncertain, but likely small, effects on elementary English Language Arts and Mathematics achievement. Access to the program leads to an increase in fourth grade Math achievement by a magnitude close to three percent of a standard deviation. However, a cohort with access to the program scores between

0.008 and 0.014 standard deviations less on third grade English Language Arts (ELA) exams and between 0.006 and 0.008 standard deviations less on fourth grade ELA exams. Although negative, the magnitude of these effect sizes are still extremely small when compared to other early education interventions (Kraft, 2018). When I use total county participation to measure Imagination Library access, I do find a small, statistically significant, positive effect on both third and fourth grade ELA exams after increases in cohort participation. In addition, I measure how the length of time a cohort has access to the program affects achievement and find an additional year of access leads to positive effects on all four exams. An additional year leads to a statistically significant increase of 0.01 standard deviations on both third and fourth grade Math achievement. Previous research suggests additional time reading can have large educational achievement gains (Kalb & Van Ours, 2014; Figlio et al., 2018), but the small magnitudes of my results suggest that simply increasing a capital input (i.e. giving children books) does not drive the large positive effect. Next, I explore heterogeneous treatment effects across across achievement, race, and socioeconomic status. I use unconditional quantile regression to assess how access to the program affects students from across the achievement spectrum (Firpo et al., 2009). Having access to the program shrinks the achievement distribution for third grade exams, which suggests the program mitigates the inequality between high- and low-achieving school districts. I also assess whether the results differ across race and socioeconomic status and find that the results from my primary specification are not largely driven by any racial or socioeconomic group.

The results I present are robust to several different specifications and models. Following de Chaisemartin & D'Haultfœuille (2020), I address issues of heterogeneous treatment timing and find that my primary results are not biased by any existing negative weights. The small results I find using my primary specification are also robust to a population weighted Imagination Library access indicator, to changes in migration patterns, and to changes in levels of public school enrollment.

With this paper, I contribute to the vast literature on early childhood interventions by investigating an understudied mechanism, pure capital investments. Earlier studies on capital inputs focus on investments that are indirectly related to educational outcomes. For example, expansive public health insurance programs increase children's primary school achievement and lead to increased high school and college completion rates (Levine & Schanzenbach, 2009; Cohodes et al., 2016). In addition, cash assistance, whether provided through a public assistance program or tax credits, allows families to lend greater support to child development, leading to increases in secondary school achievement (Chetty et al., 2011a; Dahl & Lochner, 2012; Duncan et al., 2011). While these studies support capital investments in early childhood, capital inputs better targeted towards education have less success. In a randomized field experiment, Fairlie & Robinson (2013) finds that giving students computer access at home has no effect on student achievement.[1] This paper contributes to that literature by providing further evidence that pure capital investments, even at younger ages, have little to no effect on relevant academic achievement outcomes. Additionally, this study contributes to a smaller field of human capital development that studies how the collective home environment contributes to human capital development. Families that participate in programs aimed to increase the length and quality of time spent with children often have positive effects on academic achievement (Olds et al., 1998).[2] For example, a low-intensity text message program that helps parents engage with their children led to increases in letter sound and lowercase alphabet recognition at kindergarten entrance (York et al., 2019). Results from the simple text-messaging program highlight the clash between the effects of investments that focus on only time improvements or only capital. This paper contributes to this field by suggesting that a pure capital investment fails to improve the home environment for early literacy on its own.

[1] Other studies from developing countries show similarly small effects from pure capital inputs. See Cristia et al. (2017) for evidence on giving laptops to students in Peru and Sabarwal et al. (2014) for evidence on giving textbooks to students in Sierra Leone.

[2] For example, children whose mothers participate in programs they receive visits from a registered nurse who provides medical and parenting guidance were less likely to display language delays and had higher mental development at age two (Olds et al., 1998).

This paper is most related to the work in Thompson et al. (2017), which studies the effects of Imagination Library participation on Kindergarten readiness in a Missouri school district. Using propensity score matching, the authors find no differences between standardized test scores of program participants and non-participants. My study differs from Thompson et al. (2017) based on the data sample and the empirical methods. I include data from all fifty states, which makes my results more representative. In addition, I use changes in access across time and location to identify causal affects of the program.

The results that a pure capital input has small effects on children's academic achievement has important implications for policymakers, parents, and caregivers. Pure capital investments on their own do not improve elementary level achievement, ruling out potential policies that provide specific in-kind educational benefits to families without including aspects that involve parental or caregiver involvement. Results from previous studies suggests that a capital input might be more beneficial as a complement to quality investment of time from caregivers.

The structure of the rest of the paper is as follows. In Section 2, I discuss the Imagination Library program in further detail. Section 3 describes the data and Section 4 presents the empirical model. I present the results in Section 5. Section 6 concludes.

2 Dolly Parton's Imagination Library and Community Book Programs

The Imagination Library program mails age-appropriate books to eligible children from birth through their fifth birthday on a monthly basis at no expense to enrolled families. If the program is provided in an area, eligibility only requires satisfying child age requirements; it does not take family type or income into account. Children enrolled for the full duration will receive 60 books by the time they exit the program at age five. The books differ by and are selected based on a participant's age, such that birth cohorts receive different books each month. In addition, a share of the book selection changes over time. A four year old in 2005 received a different book than a four year old in 2010.

After beginning in a rural county in Tennessee in 1995, the program was quick to spread to other areas. By the year 2000, a national expansion was underway and one year later, there were 27 local programs in 11 states. In 2004, Tennessee pledged statewide coverage, which it achieved by 2006. As of June 2018, twenty percent of all school districts nationwide had access to the program. Since the program's inception in 1995, Imagination Library has mailed over 100 million books and has spread across the US into all fifty states and internationally to Canada, the United Kingdom, Australia, and Ireland. Figure 1 maps program adoption across the country based on the first year the zip code gained access. The lighter colors represent earlier adoptions. Tennessee and several surrounding school districts in Georgia, South Carolina, and Kentucky were all early adopters of the program. West Virginia, North Carolina, and some areas in the western United States adopted the program after 2010.

Imagination Library operates through partnerships with local sponsors (hereafter called *affiliates*) such as libraries, school districts, and organizations like the United Way or Lions Club. These affiliates pay a monthly cost of $2.10 per child, advertise the program, and provide support for local families. The central Imagination Library staff covers all overhead and administrative expenses; manages the nationwide database; and selects, purchases, and ships books. To receive books, a child's caregiver must enroll the child in the program. Online enrollment is always available, but parents can also enroll their children through the mail or at some hospitals at birth, doctor's offices, and many public libraries. Once enrolled, each child receives a book in the mail monthly until he or she ages out of the program.

The Imagination Library program is not alone among other community book distribution programs. Reach out and Read and Bookstart are two additional programs that give families books for free. Reach out and Read (ROR) operates through medical practitioner's

and pediatrician's offices. ROR offices give books to children during their well visits and speak with caregivers about the importance of reading aloud. Research on the effectiveness of ROR suggests that participation in the program leads to improvements in parent-child interactions and emergent literacy skills (Weitzman et al., 2004; Diener et al., 2012). Bookstart, a program available in the United Kingdom, sends two installments of books to participating children before they turn five years old: once around twelve months and once between ages three and four. In addition to the books, there is information and strategies for caregivers included in the packages. The program also supports activities at local libraries and provides access to online games and activities that coordinate with the books. A randomized assessment of the program found improvements in parent-reported literacy interactions among participating families, but there were no differences in cognitive or non-cognitive skills between treated and untreated children (O'Hare & Connolly, 2010). There are two distinct differences between Imagination Library and the alternative programs. First, Imagination Library provides a constant monthly stream of books during the entire duration of treatment, while the alternative programs provide books at distinct points over a similar timeframe. One might expect that a constant stream of capital inputs might be more effective at improving outcomes than fewer, singular inputs. The second difference is that ROR and Bookstart focus on inputs in addition to capital (the books themselves). The alternative programs include training for parents or other activities for children that relate to the books they were given. These differences make the Imagination Library program a distinct early childhood literacy program to study.
Although the Imagination Library is an ideal setting to study the affects of pure capital inputs, the practical setup of the program introduces several layers of selection. The largest layer is at the program provision level. Local affiliates select into program adoption. Many factors could drive this decision, which I further inspect in Section 4 by assessing the validity of observable characteristics and pre-treatment Census variables to predict program adoption. The next two layers of selection occur at the family level. First, chil-

dren must be enrolled in the program by a parent or guardian. I control for this selection in all models with district level fixed effects, assuming that parental attitudes toward education are similar within school districts. I also estimate the effects of take-up rates on a reduced sample size to assess differences in enrollment that the school district level analysis masks. I discuss more about this procedure in section 3. The final layer of selection at the family level deals with the decision to use the capital. Parents or guardians can choose to engage with the books and read to their children. Unfortunately, due to data limitations, I cannot control for the third level of selection.[3] Therefore, my research strategy measures the affect of access to the program and provides intent-to-treat estimates.

3 Data

My analysis relies upon two main data sources: novel affiliate program information from Imagination Library databases and school district-level measures of academic achievement from the Stanford Education Data Archive (SEDA).

Dolly Parton's Imagination Library maintains two separate databases. The first logs information about each affiliate program that has sponsored children between 2000-2018. Recall that Imagination Library works through affiliates, which, for example, can be libraries or local fraternal organizations, to provide financial support for the program in a specified geographical area. The database includes all zip codes covered by the local affiliate, the start date of the relationship between Imagination Library's head office and the local affiliate, the date the affiliate program becomes operational, and any status changes thereafter. Status changes include program suspension, program resumption, and program termination. I used the database to determine treated cohorts based on the geographical coverage and timing of affiliate programs.[4]

[3]Although controlling for individual use of the books would be ideal, Imagination Library does not collect survey data about book use on a national level.

[4]The Imagination Library affiliate data was originally formatted at the zip code level. The educational achievement data that I use is measured at the school-district level. I reformat all data to the school-

The Imagination Library's second database collects monthly individual level mailing lists between 2009-2018. The de-identified version includes a monthly record of zip codes, birth dates, registration dates, and the affiliate provider for all children that received a book. I aggregate the individual data to create a county-level take-up rate using US Census Bureau data on the number of children under the age of five in a county. Figure 2 shows average county-level take-up rates across the country in 2018. Similar to program adoption, take-up rates are higher in the southeastern region of the United States, specifically Tennessee, South Carolina, North Carolina, Kentucky, and West Virginia. Imagination Library has also heavily invested in Native American reservations across the country, which explains the high take-up rates in North and South Dakota. Table 1 displays average take-up rates across years. Between the years 2010 to 2018, average take-up among both treated and untreated counties rose by three percentage points, from 15 percent to 18 percent. Treated counties take-up increased by two percentage points in the same time frame. The last three columns in the table show the distribution of take-up rates among treated cohorts. Columns three and four reveal that there are many treated areas with little to no participation. The median take-up rate for treated counties in 2018 is two percent. The final column of Table 1 suggests that participation is distributed unevenly among high take-up counties and low take-up counties. The 90^{th} percentile county averages suggest that in areas where there is positive take-up, participation is extremely high. Mechanisms driving this could be marketing or network related. For example, the state government of Tennessee financially supports Imagination Library inside its borders, which led to high amounts of news coverage and information dissemination from state agencies. Other states with coordinated efforts could see similar participation rates once they become more estab-

district level. I use the National Center for Education Statistic's Education Demographic and Geographic Estimates (EDGE) data to match each zip code in the Imagination Library affiliate database to the respective public school district. Because multiple zip codes can compose one school district, I assume that if one zip code has access to the program, then the whole school district has access. In school districts that have more than one zip code and where at least once zip code has access, an average of half the zip codes are treated. To combat measurement issues driven by this discrepancy, I present results using both a raw and a population weighted indicator variable that weights the Imagination Library indicator variable using decennial census population information at the zip code level.

lished.

To estimate the effect of Imagination Library program access on children's educational achievement, I combined the Imagination Library data with a nationwide database of third and fourth grade standardized test scores for school years beginning in 2008 through the school year ending in 2018 available from SEDA (Reardon et al., 2021). My analysis relies on academic achievement data that is comparable across school district, county, and state borders. The SEDA dataset exploits information that was reported to the National Center for Education Statistics (NCES) under a federal mandate by the No Child Left Behind Act. This act mandated that states annually test all public school students in grades 3-8 in both Math and English Language Arts (ELA) and report the number of students scoring in each proficiency level. States are allowed to design their own exams, which previously made comparisons across state lines difficult. Using the National Assessment of Educational Progress (NAEP) as a national comparison group, SEDA provides standardized mean test scores at the school district level relative to the NAEP scores. The scores are disaggregated by race, grade, and subject. Average achievement is reported for student subgroups if there were more than 20 students in that subgroup tested (i.e. if there were more than 20 students in that grade, year, school district, and subject group).

In my analysis, I use estimates of school district-level average achievement that are standardized within subject and grade and measured in national student-level standard deviation units. The average achievement measurement can be interpreted as an effect size; a 1 unit increase in average achievement can be interpreted as the average student within a school-district scoring approximately one standard deviation higher than the national reference cohort in that same grade. I also use covariates from the SEDA database, which include variables from both the American Community Survey (ACS) and the Common Core of Data (CCD).[5] For more information about the construction of the SEDA dataset,

[5]The ACS data describe the demographics of the total populations that lives within the school district geographic bounds. SEDA found that these measures were highly correlated with estimates restricted to only include families with school-aged children (Reardon et al., 2021). The CCD data describe school district characteristics and varies at the cohort-district-year level.

I refer the reader to Reardon et al. (2021).

My research strategy relies on the ability to observe cohorts before formal schooling and again during elementary school. Figure 3 shows the birth cohorts, their treatment windows, and their reappearance in the SEDA dataset in elementary school. I assume traditional kindergarten entry based on state-specific cutoff dates. Most children enter around the age of five, if not older, which coincides with the age at which children are no logner eligible for Imagination Library. Children born between August 2000 and August 2001 (C_1) are treated between August 2000 and August 2006, depending on their birth month and the treatment status of their residence. The C_1 cohort enters kindergarten in the fall of 2006 and are first observed as third graders in the 2009–2010 school year.

Summary statistics are reported in Table 2. The table presents average values of all outcome and control variables used for the full sample of third and fourth grade observations. Column (1) represents the sample of untreated observations, column (2) represents summary statistics for the sample of treated observations, and column (3) represents all school districts. Around thirteen percent of all observations had access to the program. School districts with access to the program had it for almost four years. Asian and white students (with or without the program) scored above average. Black, Hispanic, and economically disadvantaged students scored below average.

4 Empirical Model

The basic model follows a two way fixed effects difference-in-differences strategy, where I compare elementary school achievement outcomes for those with early childhood Imagination Library exposure to those without. Because of the delay between program participation and standardized tests, I assume students participated in the program and sit for exams in the same school district, or that they don't migrate to different school districts. The results are robust to controlling for varying levels of migration, as seen in Section 5.3.

To assess the effects of Imagination Library, I estimate

$$Y_{dct} = \lambda_c \ + \eta_d + \delta IL_{dc} \ + \ X_{dct}\beta_1 + Z_{dt}\beta_2 + \theta V_{d00}t + \epsilon_{dc} \tag{1}$$

where the dependent variable is the achievement level for cohort c at school district d in year t.[6]

IL_{dc} represents one of four Imagination Library access measurements. The first measure is an indicator variable equal to one if the cohort living in the school district had access to the Imagination Library program for at least one month and zero otherwise. The second access measure is a population weighted version of the dummy indicator variable, weighted by zip code populations that make up the school district.[7] The third access measure reports the number of years a cohort had access to the program, which measures differences between cohorts who might have only had access to the program for one year and those who had access for the full five years. The final access measure is the county-level participation rate. Due to data limitations, there are three points of concern with the final access measurement. First, the model when using the fourth access variable is measured at the county level, rather than the school district level as in Equation 1.[8] The second cause for concern is that the measure is highly aggregated across years. The take-up estimate for year t is equal to the total participation rate for all eligible children in year t; it is not separated at the cohort level. Therefore, the measure approximates the cohort participation

[6] I assume normal kindergarten entrance based on annual kindergarten start dates set by state legislatures.

[7] The population weighted dummy variable measures a weighted average of Imagination Library coverage based on the treatment status of each zip code within a school district. The population weighted Imagination Library estimate in county x with n zip codes is equal to:

$$IL_x^{pop} = \sum_{i=1}^{n} \frac{population_i}{population_x} \mathbb{1}[IL_i = 1]$$

[8] The county take-up rate is equal to:

$$\frac{\text{Number of children in the county who received books}_t}{\text{Number of children aged 0-5 in the county}_t}.$$

rate by averaging together seven calendar year estimates to cover all months that a child in a respective cohort could have received books. Another data limitation drives the final issue with the take-up measurement. Imagination Library's individual dataset begins in 2009. Because of the almost four year gap between treatment and outcome observation, only estimates for the final cohorts average across all seven years of treated children. Given these three barriers to a cleanly defined measure, I primarily rely on estimates using the first three access indicators.

I include several controls in my preferred specification to ensure comparisons across treatment groups are confounded with as little outside variation as possible. The covariates, X_{dct}, include time varying cohort-by-district controls for the percent of students across race and ethnicity groups (percent non-Hispanic white, percent non-Hispanic Black, and percent Hispanic), the percent of students eligible for free or reduced price lunch, and the percent of students categorized as English Language Learners (ELL). In addition to those controls, I also include vector Z_{dt}, which controls for district-specific controls that are representative of the entire population that lives inside the school district's geographic bounds. These include the log of median income, educational attainment (the percent of residents that hold a Bachelor's degree or higher), the poverty level, the unemployment level, the percentage of households receiving SNAP benefits, and the percentage of households led by a single mother. In addition to the two vectors of control variables, estimates from my preferred specification include unrestricted cohort effects λ_c and unrestricted school district effects η_d to control for unobserved heterogeneity that could affect standardized test scores. The results are weighted by the number of students in a school district cohort that take the exam and standard errors are clustered at the school district level.[9]

When using the differences-in-differences strategy, it is important to consider the experiment created by the national program rollout. In an ideal situation, school districts would gain access to the program at random, or at least independent of any factor that could in-

[9]A selection of unweighted results can be found in Appendix A

fluence student achievement scores. Additionally, school districts that adopt the program should not be different from districts that do not adopt the program in any time-varying characteristics. Finally, students in school districts with access would have experienced changes in achievement scores similar to those in districts without access in absence of the program. If these conditions are met, my main specification will provide an unbiased estimate of the average treatment effect.

Therefore, the validity of my identification strategy relies on the exogenous introduction of Imagination Library across school districts, conditional on exogenous changes in time-varying characteristics, and a parallel trends assumption. To address these concerns, I conduct a covariate balance test, assess the ability of pre-treatment characteristics of school districts to predict program adoption, and run an event study specification.

The covariate balance test is a placebo test that ensures that Imagination Library program adoption does not predict covariates included in the model, which should not be affected by Imagination Library program adoption. These tests use a single right hand side variable as the dependent variable in a model similar to my main specification. If Imagination Library is a poor predictor of these variables, program adoption does not alter the composition of a school district. Results for the covariate balance test on the sample of third grade cohorts are displayed in Table 3. Many of the coefficients presented are very small, and some are statistically insignificant for third grade exams. Imagination Library access is a good predictor of changes in four control variables: the percent of students eligible for free and reduced price lunch, the percent of English language learners in a school district, the log median income, and the unemployment rate. Because access to Imagination Library is associated with statistically significant, although small, changes in control variables, there is concern that the population changes as a result of the program. The results in Table 3 suggest that areas become slightly less wealthy after program adoption (increase in free or reduced price lunch eligibility and decreased median income). It is unclear if these changes are the result of families moving into the school district to participate in the program or if there are changes within the pre-treatment population. I include all school-district characteristics as controls in my primary specification to correct for changes in sample composition across treatment statuses.

Following Hoynes & Schanzenbach (2009) and Acemoglu et al. (2004), I also assess the ability of pre-treatment characteristics of school districts to predict program adoption as a means to test the exogeneity of the program. The pre-treatment characteristics I analyze come from the 2000 Census, which was the first year of Imagination Library's national expansion.[10] The outcome variable I use to assess program exogeneity is an index of the month and year a school district gained access to the program normalized to one in January 2000 (such that January 2001 is equal to 13). The Census characteristics include the percent of the 2000 population that lives in an urban area, is less than five years of age, is older than sixty-five years of age, and is Black. I also include the natural log of population served by the school district and the natural log of the median income in the school district.

Table 4 presents results from this test.[11] A negative coefficient indicates the independent variable of interest associates with earlier program adoption. Once I control for time invariant state level characteristics, I find that school districts with an urban classification adopt the program about half a month earlier than their more rural counterparts. The weak fit of the model and the statistically insignificant coefficients for the rest of the characteristics suggest that many of the deciding factors to adopt Imagination Library are idiosyncratic. Nonetheless, to control for differences in observable population trends that may be correlated with program adoption as well as later achievement, I include interactions of these pre-treatment values with time trends in my main specification, represented as V_{d00}.

In addition to the exogenous adoption assumption, the canonical difference-in-differences

[10] Although it coincides with a few program start dates, the majority of programs started much later than 2000.

[11] Summary statistics for all variables used in the covariate balance test are in Table B.1.

model requires the traditional common trends assumption, which relies on the assumption that important unobserved variables are either time invariant group attributes that are captured by the district fixed effect or time varying factors that are group invariant and thus captured by the cohort fixed effect. In other words, identification relies on the assumption that in absence of Imagination Library adoption, the outcome variables of treatment and control districts would have evolved similarly. To assess the plausibility of the assumption, I compute several estimates that compare average achievement of treated and untreated cohorts during years prior to treatment, where the control group has a stable treatment status. I use the placebo estimator developed by de Chaisemartin & D'Haultfœuille (2020) that accounts for differences in treatment timing.

Figure 4 displays results for all four exams studied. Year zero represents the difference between treated and untreated cohorts for the first treated cohort in a school district. In general, the figures suggest no statistically significant evidence of differential group trends in any sample. In the years prior to treatment in the third grade sample in Figures 4a and 4b, there appears to be a small peak two years prior to program adoption for treated cohorts and then a slight decline back, although none of the coefficients are statistically significant from zero. An opposite pattern appears among the fourth grade sample, as seen in Figures 4c and 4d. There is a downward trend two years prior to treatment, but by the time treatment occurs, the difference has mostly disappeared. Again, none of the coefficients are statistically different from zero. The coefficients from all four of these figures suggest that there is no statistically significant difference between the average achievement trends of treated and untreated cohorts during the three years prior to treated cohorts sitting for their third and fourth grade exams.

5 Results

5.1 Full Sample Results

I begin with estimates for the full sample of students and school districts, regardless of their racial or socioeconomic composition. I consider four main outcome variables: third and fourth grade English and Math average achievement levels. The outcome variables are standardized and weighted such that a 1 unit increase in the outcome achievement variable can be interpreted as an increase in a student's average achievement by one standard deviation above the national reference cohort.

The basic difference-in-differences results for third and fourth grade achievement scores are presented in Table 5. The estimates in the first row assess the affects of access to the Imagination Library on four different exam scores, when access is measured using the raw indicator variable. The estimate in column 1 of -0.0079 suggests that access to the program reduces achievement by 0.79 percent of a standard deviation on third grade ELA exams. Column 2 shows an effect closer to zero for third grade Math exams. None of the estimates in the first row are statistically different from zero.

The second row of results in Table 5 shows estimates of the effects of Imagination Library access using the population weighted access indicator. The estimate in column one suggests that gaining access to the Imagination Library program prior to kindergarten matriculation leads to a reduction in a cohort's third grade ELA scores by 1.38 percent of a standard deviation. Estimates in columns two and three show a more modest impact on third grade Math and fourth grade ELA. I find a relatively large effect on fourth grade math exams. Access to the program increases average fourth grade Math exam scores by almost three percent of a standard deviation relative to the national cohort. The estimates from both panels of Table 5 indicate that gaining access to Imagination Library prior to formal schooling fails to have a positive effect on ELA exams, but leads to increases in Math scores. The estimates using the population weighted estimator magnify any effects estimated using the raw indicator variable, suggesting that there is significant within-district variation of program adoption.

Although raw program access to Imagination Library fails to have a positive effect on ELA achievement, the estimates in Table 6 present a different story. These estimates describe the effects of the average county participation rate on average county-level achievement for each respective grade-subject group. I find a small, positive effect on both third and fourth grade ELA achievement in counties with higher participation rates. A one percentage point increase in the county-level participation rate increases third grade ELA achievement scores by 0.0008 standard deviations. Although the point estimate is extremely small, the average participation rate in counties within my sample is close to 20 percent. A back of the envelope calculation indicates that the program has increased average ELA achievement by 1.6 percent of a standard deviation, which comes close to the estimates in Table 5.

Estimates of the impact of the number of years a cohort has access to Imagination Library are presented in Table 7. These results indicate that an additional year of access has a very small positive affect on achievement scores across third and fourth grade exams. Columns 1 and 2 show that having access to the program for one additional year leads to an increase in third grade ELA achievement by 0.07 percent of a standard deviation and an increase in third grade Math achievement by 1.14 percent of a standard deviation, respectively. The estimates on Math exams are both statistically different from zero. Within five years of adopting the program, the total cumulative effect on third grade Math exams can be above five percent of a standard deviation for the youngest cohort, which is a decent effect size in the literature (Kraft, 2018).

5.2 Heterogeneity

I also explore heterogeneity in treatment effects, as different subgroups could be more responsive to participation in the program. Specifically, I consider heterogeneous effects by achievement level, race, and socioeconomic status.

First, to examine if there is treatment effect heterogeneity among low and high-achieving students, I examine how access to Imagination Library affects students at different points in the average achievement distribution using unconditional quantile regression as described in Firpo et al. (2009).[12] Tables 8–11 display results from estimates using the treatment indicator variables. The tables present the average effect (estimated using Ordinary Least Squares) and the unconditional quantile estimates from the 10^{th}, 25^{th}, 50^{th}, 75^{th}, and 90^{th} percentiles in the average achievement score distribution. The estimates in the first row of Table 8 show that access to Imagination Library shrinks the distribution of average achievement scores for the third grade ELA exams, such that the scores of school districts in the bottom quarter of achievement increase and the scores of higher achieving school districts decrease. These results are suggestive evidence that access to the program could mitigate inequality between school districts of different achievement levels. However, they are not quite robust to using the population weighted indicator and none of the estimates reach statistical significance. After controlling for population by zip code, the signs flip for estimates in the twenty-fifth percentile and above. Estimates for the effect on third grade Math scores are presented in Table 9. Similar to the third grade ELA results, using the raw indicator suggests the distribution shrinks across the achievement distribution. Again, the results are not completely robust to the population weighted indicator variable and none of the parameters reach statistical significance.

I present estimates from unconditional quantile regression showing the effects of an additional year of access in Tables 12 and 13. For both grades, an additional year of Imagination Library access leads to a decrease in average achievement scores for several points in the achievement distribution. For example, one more year of Imagination Library access leads to a decrease in average third grade ELA exam scores by 0.12 percent for school districts at the median of the achievement distribution. An additional year of access is 12

[12] This method allows for investigation of treatment on different quantiles of the achievement distribution without changing the distribution of control variables.

books, so a back of the envelope calculation reveals that each additional book decreases the average school district achievement level by 0.01 percent of a standard deviation, a change practically equal to zero. In addition, none of the estimates for ELA exams are statistically different from zero. However, additional years of access to Imagination Library have positive effects on Math exams at many points in the distribution for both grade levels. Additional years of access leads to an increase on Math exams for both grades at all points in the distribution except at the 90th percentile for third grade exams.

Next, I assess how access to Imagination Library affects students of different subgroups. Table 14 presents the effects of the Imagination Library indicator variable, both population weighted and unweighted, on the full sample of students, Hispanic Students, Non-Hispanic Black students, and Non-Hispanic white students. The results are from the third grade sample, where Panel A indicates effects on English Language arts exams.[13] The estimates in the first row of -0.0128 suggest Hispanic students' third grade ELA achievement decreases more than their peers in other subgroups when they have access to Imagination Library. The results are fairly robust to using the population weighted indicator variable, but none reach statistical significance. These results suggest there is no one single subgroup primarily driving the full sample estimates and although there are slight differences in effects between subgroups, all are statistically insignificant and small effect sizes relative to the literature.

Panel B of Table 14 presents results on third grade Math exams. Estimates using both the raw indicator variable and the population weighted access measures reveal that the full sample estimate is primarily driven by the non-Hispanic Black population. Estimates for the other two subgroups, non-Hispanic white students and Hispanic students, are smaller in magnitude and fail to reach statistical significance at the ten percent level. The estimate in the fourth row of column 3 of 0.0567 suggests that non-Hispanic Black students who had access to the program see increases in their third grade math achievement by

[13]Results from the fourth grade sample are in Table B.3 in the appendix.

5.67 percent of a standard deviation. The heterogeneous analysis of Math exams by student subgroup revealed slight differences in magnitudes of estimates affecting third grade achievement.

I now turn to heterogeneous treatment effects across socioeconomic status (SES). Imagination Library gives free books to all age-eligible children, regardless of family income. However, one might expect children from families of low-socioeconomic status to benefit greater from participation in the program because these families might own less books due to financial constraints. To investigate effects across heterogeneous socioeconomic status, I estimate my primary specification on achievement scores for low-socioeconomic students across all school districts. Column 2 in Table 15 present results on third grade scores of low-SES students, relative to the full sample estimate presented in column 1.[14] Following the patterns previously discussed in the main results, the estimated effects on ELA exams are slightly negative and the estimates for Math exams are positive. The effects on ELA exams for low-SES students, if anything, are closer to zero than the estimates using the full sample, although none reach statistical significance. The estimates for Math exams, however, are larger and are statistically different from zero when I use the population weighted Imagination Library indicator. The estimate in the fourth row of column 2 of 0.0358 suggests students with low-socioeconomic backgrounds see increases in their third grade Math scores of 3.5 percent of a standard deviation. These results are suggestive evidence of narrowing inequality. Students from low-SES backgrounds either have larger increases when they gain access (Math exams) or smaller decreases (ELA exams) relative to the full sample estimates.

[14]Estimates for the fourth grade sample can be found in Table B.4 in the appendix.

5.3 Robustness Specifications

5.3.1 Migration and Private School Enrollment

Using data from the American Community Survey, I estimate an additional specification in Table 16 that addresses concerns about migration out of or into school districts. Because there is a four year gap between students exiting treatment and reappearing in achievement data at the third grade level, the main specifications would be biased if there were large amounts of migration, either in or out of the school district. To ensure the results are not driven by migration behavior, I restrict the sample to differing levels of relative migration. Each column represents a different sample of third grade ELA scores based on migration, with increasing columns including a higher proportion of the data, beginning at the bottom 25 percent of relative migration. Column 4 repeats results from the primary specification with full controls. This specification controls for the full vector of school district characteristics as well as unobserved heterogeneity at the school district and cohort level. Estimates from columns 1-3 are all slightly positive and none are statistically different from zero. The estimate in column 1 of 0.0125 is the largest in magnitude, suggesting that school districts with the lowest levels of migration actually see the highest increases in third grade ELA achievement. The estimates in columns two and three are slightly smaller, at 0.0055 and 0.0067, respectively. Because average scores increase less in school districts with higher levels of in-migration (columns 2-3), there is some evidence that there could be a small influx of families who migrate to school districts with access to Imagination Library. It is unlikely that families choose one school district over another solely on whether the children have access to Imagination Library, but program access could be indicative of other local attitudes or academic programs in the area. A school district fixed effect should capture the majority of these time-invariant characteristics.

Columns 1-3 of Table 17 follow the same pattern explained above, but restrict the sample based on public school enrollment. Because the federal mandatory reporting requirement only applied to public schools, private school data is not included in the SEDA dataset. If a large proportion of Imagination Library participants living inside a geographic school district attended private schools, the results from the main specification would not capture achievement effects. By restricting the sample to include school districts where a large proportion of the children attend private schools, I can compare estimates from different settings to see if enrollment decisions are driving any of the effects. The estimates from the most severely restricted sample in the first column is not significantly different from zero and is the only negative effect estimated. The estimates in columns two and three are positive and larger than the full sample estimate, but again, none of the estimates reach statistical significance so I cannot say with confidence whether there are true differences between school districts with differing levels of enrollment.

5.3.2 Variation in Treatment Timing and Heterogeneous Effects

An additional concern with my empirical strategy is that the treatment timing differs across school districts. de Chaisemartin & D'Haultfœuille (2020) shows that the traditional two-way fixed effects (TWFE) difference-in-differences (DID) estimator may produce biased estimates when the treatment effects differ across either fixed effect dimension. Specific to my research design, the DID estimator might be biased if treatment effects differ across time or school district. The average treatment effect (ATE) is a weighted average of the comparisons between all group-time pairs. Differences in treatment period or effects across groups could cause negative weights, leading to an ATE that is a non-convex combination of all treatment effects (de Chaisemartin & D'Haultfœuille, 2020). This is a concern in my analysis as school districts differ in their adoption of the program and effects can be heterogeneous across district.

I follow de Chaisemartin & D'Haultfœuille (2020)'s prescription to assess the problem and it's potential affects on my original DID estimate and generate estimates of the ATE with their new estimator, DID_M. This includes initially assessing the weights and calculating a test statistic to determine the extent of the issue they could cause. Using Stata code provided by the authors, I assess whether my basic specification suffers from bias due to negative weights. In the two way fixed effects (TWFE) regression of achievement on Imagination Library access, school district FEs, and cohort FEs, there are 18,274 total ATEs estimated by comparing across group-time cells. In the model, close to 70 percent are strictly positive and 30 percent are strictly negative, a nontrivial amount. The negative weights sum to -0.33681. The negative weights could cause the TWFE estimator from my primary specification to be of a different sign or magnitude than the true average treatment on the treated if the standard deviation of those ATEs is equal to 0.0096, a plausible magnitude. Because it is possible that the negative weights cause the sign of the two way fixed effects estimator to differ from the ATE, I estimate the model using a new estimator that corrects for the bias. Table 18 shows the estimates from the primary TWFE difference-in-differences model (β_{FE}) and the new estimator that corrects for the bias (DID_M) for all four exams studied. DID_M estimates the ATEs across all school district-cohort cells where treatment changed between $t-1$ and t (de Chaisemartin & D'Haultfœuille, 2020). The DID_M estimator predicts effects of different signs on third grade exams, but none of the estimates reach statistical significance and all estimates are relatively small.

6 Discussion and Conclusion

In an effort to further understand how individual inputs affect early human capital development, I present the effects of an early childhood intervention that gives free books to children on elementary age standardized test scores. I exploit the variation from the nationwide rollout of the program in a two way fixed effects difference-in-differences framework. My findings suggest that access to the program fails to have a large, positive effect on aggregate test scores. I find uncertain, but likely small, effects on third and fourth grade ELA and Math achievement. Simply having access to Imagination Library for at least one month decreases a cohort's average third grade ELA scores by around one percent of a standard deviation relative to a national control cohort and increases fourth grade math scores by almost three percent of a standard deviation. Once participation levels are controlled for, the effect on Math scores becomes null, but I find small positive effects on ELA scores.

The overall results imply a relatively small effect from gaining access to the program. This suggests that a pure capital educational input during early formative years has little effect on improving overall test scores at the third and fourth grade level. This is not equivalent to saying that a pure capital investment is pointless; giving free books to children does not harm students, and if anything, my estimates are a conservative lower bound. Further, any positive effects of the program that exist immediately after treatment could fade out to an unrecognizable level by third or fourth grade (Duncan & Magnuson, 2013). Although treatment effects of early childhood interventions on cognitive skills only last for roughly ten years, there are often positive effects on outcomes later in life that can be attributed to permanent personality skill changes (Duncan & Magnuson, 2013; Heckman et al., 2013). Personality skills, such as aggressive or social behavior, might not be accurately measured in cognitive exams designed to measure specific aptitudes. However, these skills (or lack of, in the case of aggressive behavior) can improve a number of labor market and health outcomes later in life. The data do not yet exist to examine long-term effects of Imagination Library, but it is possible that a similar early literacy intervention has positive long-run effects.

Access to Imagination Library has slightly different effects across the achievement distribution, but the results are still in the spirit of the full sample estimates: small and periodically imprecise. Results from unconditional quantile analysis analysis on English Language Arts exams suggest gaining access to the program reduces inequality between high- and low- achieving school districts, although estimates fail to reach statistical significance. The estimates for school districts with average achievement below the 25^{th} percentile see increases in average third grade ELA achievement after gaining access. Gaining access to the program has a negative affect on achievement for above median achieving school districts. These results suggest that access to the program could reduce inequality in achievement between school districts. The books themselves could induce learning activities in the home prior to formal schooling that better prepare students in school districts that have been historically low-performing. I also assess differences between the achievement levels of the full sample of students and only those who come from low socioeconomic backgrounds. These results also provide suggestive evidence that the program could mitigate some inequality between students of different backgrounds.

Due to program limitations, I leave for future work the question of how complementary in-kind capital inputs and parental time are in early human capital development. Understanding which features of a compound program (capital, time, or the combination of the two) would be valuable to those designing and the policymakers implementing early interventions. I have shown here that capital on its own fails to leave a large lasting effect on elementary academic achievement and other research suggests that time inputs are successful (York et al., 2019). However, it remains unknown whether a program that capitalizes on both inputs, like pairing parental guidance with the Imagination Library program, could be more successful at increasing early human capital development in the early years of a child's life at home.

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Tables and Figures

Table 1: Imagination Library Takeup Rates Across Years

	All Cohorts	Treated Cohorts	Treated Cohorts 10^{th} percentile	Treated Cohorts 50^{th} percentile	Treated Cohorts 90^{th} percentile
2010	15%	21%	0%	1%	61%
2011	16%	21%	0%	1%	62%
2012	16%	21%	0%	1%	62%
2013	15%	20%	0%	1%	59%
2014	15%	20%	0%	1%	59%
2015	16%	21%	0%	1%	62%
2016	17%	23%	0%	1%	63%
2017	19%	25%	0%	2%	68%
2018	18%	23%	0%	2%	63%

Table 2: Descriptive Statistics

	Total		Treated		Untreated	
	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
IL Access Variables						
IL Program Indicator	0.48	(0.50)	0.00	(0.00)	1.00	(0.00)
Pop. Weighted IL Indicator	0.26	(0.42)	0.00	(0.00)	0.55	(0.47)
Number of Years with IL	2.07	(2.75)	0.00	(0.00)	4.29	(2.49)
Achievement Outcomes						
Total Student Ach.	0.01	(0.37)	-0.00	(0.37)	0.03	(0.37)
Asian Student Ach.	0.45	(0.51)	0.43	(0.50)	0.46	(0.51)
Black Student Ach.	-0.46	(0.31)	-0.45	(0.30)	-0.47	(0.31)
Hispanic Student Ach.	-0.29	(0.32)	-0.31	(0.32)	-0.28	(0.32)
White Student Ach.	0.14	(0.33)	0.14	(0.33)	0.14	(0.33)
ECD Student Ach.	-0.23	(0.32)	-0.25	(0.32)	-0.22	(0.32)
ACS Controls						
Urban location	0.40	(0.49)	0.41	(0.49)	0.38	(0.48)
Rural	0.60	(0.49)	0.59	(0.49)	0.62	(0.48)
Log Meidan Income	10.76	(0.26)	10.76	(0.27)	10.76	(0.25)
Rate of Bachelor's Degree	0.20	(0.10)	0.20	(0.10)	0.19	(0.09)
Poverty Rate	0.14	(0.06)	0.14	(0.06)	0.14	(0.06)
Unemployment Rate	0.07	(0.03)	0.08	(0.03)	0.07	(0.03)
SNAP Receipt Rate	0.12	(0.06)	0.11	(0.06)	0.12	(0.06)
Female Headed HH Rate	0.15	(0.06)	0.15	(0.06)	0.15	(0.06)
CCD Controls						
Total Cohort Size	315.20	(1278.25)	335.37	(1504.24)	293.51	(978.00)
% Native American Students	0.04	(0.13)	0.03	(0.12)	0.04	(0.14)
% Asian Students	0.01	(0.03)	0.01	(0.03)	0.01	(0.02)
% Hispanic Students	0.10	(0.16)	0.11	(0.18)	0.09	(0.14)
% Black Students	0.08	(0.17)	0.09	(0.18)	0.07	(0.15)
% White Students	0.78	(0.26)	0.76	(0.27)	0.80	(0.24)
% FRL	0.53	(0.20)	0.54	(0.21)	0.53	(0.20)
% ECD	0.55	(0.21)	0.55	(0.22)	0.54	(0.21)
% ELL	0.04	(0.07)	0.04	(0.08)	0.03	(0.07)

Parameters listed are mean values for all cohorts. ECD refers to economically disadvantaged students, as defined by the SEDA composite variable. Standard deviations are in brackets where applicable. * FRPL stands for Free/Reduced Price Lunch

Table 3: Covariate Balance Test: Third Grade

	Outcome Variable						
	Number of Students	Urban Neighborhood	Percent Black	Percent FRL	Percent ELL	Log Median Income	Unemp. Rate
IL Access Indicator	-0.8695	-0.0041	0.0006	0.0035*	0.0017***	-0.0030**	0.0004***
	(2.154)	(0.006)	(0.000)	(0.002)	(0.001)	(0.001)	(0.000)
Observations	41419	41419	41419	41419	41419	41419	41419

Notes: This table shows results from a covariate balance test that measures how well IL access predicts changes in control variables. Each column presents estimates using a different control variable as the outcome variable. The independent variable in each regression equals one when a school district gains access to Imagination Library. The sample includes all observations from third grade ELA exams. Standard errors are in parenthesis and are clustered at the school district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Test for 2000 Census Pre-treatment Trends

	Index	
	1	2
Percent of population urban	0.4565***	0.5127***
	(0.032)	(0.185)
Percent of population <5	-6.3500***	-5.6230
	(0.518)	(3.659)
Percent of population >65	0.6997***	0.8092
	(0.117)	(0.646)
Percent of population Black	0.0620*	0.0362
	(0.037)	(0.341)
Log median income	19.8408***	10.5330
	(1.813)	(9.131)
Log Population	-1.1469	-4.8165
	(0.731)	(2.896)
Observations	151769	151769
State Fixed Effect		X
Observations	5396	4804
R^2	0.03	0.27

Notes: This table shows results from a test of the predictive power of pre-treatment area characteristics. The data used in this table are at the school district level. The dependent variable is a calendar month index (normalized to one in January 2000) that a zip code within the school district adopted the program. The control variables come from the 2000 Census. Estimates are weighted using the cohort-school district enrollment. Standard errors are in parenthesis and are clustered at the school district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Effects of IL Access on Elementary ELA and Math Scores

	3rd Grade ELA Exams (1)	3rd Grade Math Exams (2)	4th Grade ELA Exams (3)	4th Grade Math Exams (4)
IL Access Indicator	-0.0079	0.0019	-0.0060	0.0151
	(0.007)	(0.011)	(0.007)	(0.010)
Observations	37379	37329	33408	33360
Population Weighted IL Indicator	-0.0138*	0.0133	-0.0088	0.0298**
	(0.008)	(0.015)	(0.008)	(0.013)
Observations	37379	37329	33408	33360
Cohort & District FE	X	X	X	X
2000 Census Time Trends	X	X	X	X

Notes: This table presents estimates of the effect of having access to Imagination Library on four different exams, each in a separate column. The IL access indicator equals one if the school district had access to Imagination Library for at least one month. The population weighted IL indicator is a measurement weighted by zip code populations that are within the school district boundary. The outcome variable for all estimates is the average achievement score for the entire school district in the respective subject. The results are weighted by the school district-cohort population. Estimates are weighted using the cohort-school district enrollment. Standard errors are in parenthesis and are clustered at the school district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: Effects of Local Participation Rate on Elementary ELA and Math Scores

	3rd Grade ELA Exams (1)	3rd Grade Math Exams (2)	4th Grade ELA Exams (3)	4th Grade Math Exams (4)
Take-up Rate	0.0008*	-0.0007	0.0008*	0.0000
	(0.000)	(0.001)	(0.000)	(0.001)
Observations	16504	16357	13300	13218
Cohort & District FE	X	X	X	X
2000 Census Time Trends	X	X	X	X

Each parameter is from a separate regression of the outcome variable on an indicator variable equal to one if the school district had access to Imagination Library for at least one month. The outcome variable for all estimates is the average achievement score for the entire school district in the respective subject. Estimates are weighted using the cohort-school district enrollment. Standard errors are in parenthesis and are clustered at the school district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 7: Effects of Years of IL Access on Elementary ELA and Math Scores

	3rd Grade ELA Exams	3rd Grade Math Exams	4th Grade ELA Exams	4th Grade Math Exams
	(1)	(2)	(3)	(4)
Years of IL Access	0.0007	0.0114***	0.0003	0.0099***
	(0.002)	(0.003)	(0.002)	(0.003)
Observations	37379	37329	33408	33360
Cohort & District FE	X	X	X	X
2000 Census Time Trends	X	X	X	X

Notes: Each parameter is from a separate regression of the outcome variable on an indicator variable equal to one if the school district had access to Imagination Library for at least one month. The sample in panel B includes results from math exams. The outcome variable for all estimates is the average achievement score for the entire school district in the respective subject. Estimates are weighted using the cohort-school district enrollment. Standard errors are in parenthesis and are clustered at the school district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: Effects of IL program availability on achievement: Third Grade English Language Arts

		Quantiles of Average Achievement			
	OLS	0.10	0.25	0.50	0.75
	(1)	(2)	(3)	(4)	(5)
IL Access Indicator	-0.0076	0.0161	0.0034	-0.0050	-0.0001
	(0.007)	(0.023)	(0.019)	(0.020)	(0.023)
Observations	37379	37485	37485	37485	37485
Population Weighted IL Indicator	-0.0137*	0.0002	-0.0131	-0.0141	0.0125
	(0.008)	(0.027)	(0.022)	(0.019)	(0.036)
Observations	37379	37485	37485	37485	37485

Notes: Results in this table come from an unconditional quantile regression that estimates the effect of having access to Imagination Library for at least one month on the school district average 3rd grade exam scores. All estimates include the full panel of control variables, district and cohort fixed effects, and 2000 Census level time trends. Column (1) represents the effect of an additional year of access to Imagination Library as estimated by an Ordinary Least Squares model for comparison. Estimates are weighted using the median number of exams in a cohort-school district group. Standard errors are presented in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9: Effects of IL program availability on achievement: Third Grade Math

		Quantiles of Average Achievement			
	OLS	0.10	0.25	0.50	0.75
	(1)	(2)	(3)	(4)	(5)
IL Access Indicator	0.0012	0.0212	0.0016	0.0018	-0.0087
	(0.011)	(0.028)	(0.018)	(0.022)	(0.023)
Observations	37329	37439	37439	37439	37439
Population Weighted IL Indicator	0.0113	0.0289	-0.0040	0.0150	0.0188
	(0.015)	(0.029)	(0.025)	(0.027)	(0.032)
Observations	37329	37439	37439	37439	37439

Notes: Results in this table come from an unconditional quantile regression that estimates the effect of having access to Imagination Library for at least one month on the school district average 3^{rd} grade exam scores. All estimates include the full panel of control variables, district and cohort fixed effects, and 2000 Census level time trends. Column (1) represents the effect of an additional year of access to Imagination Library as estimated by an Ordinary Least Squares model for comparison. Estimates are weighted using the median number of exams in a cohort-school district group. Standard errors are presented in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 10: Effects of IL program availability on achievement: Fourth Grade ELA

		Quantiles of Average Achievement			
	OLS	0.10	0.25	0.50	0.75
	(1)	(2)	(3)	(4)	(5)
IL Access Indicator	-0.0053	0.0061	-0.0056	0.0147	-0.0083
	(0.007)	(0.021)	(0.011)	(0.014)	(0.022)
Observations	33408	33497	33497	33497	33497
Population Weighted IL Indicator	-0.0082	-0.0118	-0.0239	0.0074	0.0029
	(0.008)	(0.024)	(0.017)	(0.026)	(0.024)
Observations	33408	33497	33497	33497	33497

Notes: Results in this table come from an unconditional quantile regression that estimates the effect of having access to Imagination Library for at least one month on the school district average 4^{th} grade exam scores. All estimates include the full panel of control variables, district and cohort fixed effects, and 2000 Census level time trends. Column (1) represents the effect of an additional year of access to Imagination Library as estimated by an Ordinary Least Squares model for comparison. Estimates are weighted using the median number of exams in a cohort-school district group. Standard errors are presented in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 11: Effects of IL program availability on achievement: Fourth Grade Math

		Quantiles of Average Achievement			
	OLS	0.10	0.25	0.50	0.75
	(1)	(2)	(3)	(4)	(5)
IL Access Indicator	0.0153	-0.0070	0.0078	0.0391**	0.0116
	(0.010)	(0.026)	(0.020)	(0.016)	(0.024)
Observations	33360	33449	33449	33449	33449
Population Weighted IL Indicator	0.0296**	0.0111	0.0134	0.0702***	0.0456
	(0.013)	(0.039)	(0.030)	(0.024)	(0.034)
Observations	33360	33449	33449	33449	33449

Notes: Results in this table come from an unconditional quantile regression that estimates the effect of having access to Imagination Library for at least one month on the school district average 4th grade exam scores. All estimates include the full panel of control variables, district and cohort fixed effects, and 2000 Census level time trends. Column (1) represents the effect of an additional year of access to Imagination Library as estimated by an Ordinary Least Squares model for comparison. Estimates are weighted using the median number of exams in a cohort-school district group. Standard errors are presented in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 12: Effects of length of IL program availability on achievement: Third Grade

		Quantiles of Average Achievement				
	OLS	0.10	0.25	0.50	0.75	0.90
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: ELA						
Years of IL Access	0.0009	-0.0007	-0.0027	-0.0012	0.0060	-0.0063
	(0.002)	(0.007)	(0.005)	(0.004)	(0.006)	(0.009)
Observations	37379	37485	37485	37485	37485	37485
Panel A: Mathematics						
Years of IL Access	0.0113***	0.0134	0.0099	0.0096*	0.0147**	-0.0021
	(0.003)	(0.012)	(0.006)	(0.006)	(0.007)	(0.006)
Observations	37329	37439	37439	37439	37439	37439

Notes: Results in this table come from an unconditional quantile regression that estimates the effect of having access to Imagination Library for at least one month on the school district average score on third grade exams. All estimates include the full panel of control variables, both district and cohort fixed effects, and 2000 Census level time trends. Column (1) represents the effect of an additional year of access to Imagination Library as estimated by an Ordinary Least Squares model for comparison. Estimates are weighted using the cohort-school district enrollment. Standard errors are in parenthesis and are clustered at the school district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 13: Effects of length of IL program availability on achievement: Fourth Grade

	OLS	Quantiles of Average Achievement				
		0.10	0.25	0.50	0.75	0.90
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: ELA						
Years of IL Access	0.0004	-0.0053	-0.0058	-0.0028	0.0066	-0.0029
	(0.002)	(0.006)	(0.005)	(0.004)	(0.009)	(0.005)
Observations	33408	33497	33497	33497	33497	33497
Panel A: Mathematics						
Years of IL Access	0.0099***	0.0064	0.0026	0.0135**	0.0103	0.0172**
	(0.003)	(0.010)	(0.007)	(0.007)	(0.008)	(0.008)
Observations	33360	33449	33449	33449	33449	33449

Notes: Results in this table come from an unconditional quantile regression that estimates the effect of having access to Imagination Library for at least one month on the school district average score on fourth grade exams. All estimates include the full panel of control variables, both district and cohort fixed effects, and 2000 Census level time trends. Column (1) represents the effect of an additional year of access to Imagination Library as estimated by an Ordinary Least Squares model for comparison. Estimates are weighted using the cohort-school district enrollment. Standard errors are in parenthesis and are clustered at the school district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 14: Effects of IL program availability on average school district achievement by race: third grade sample

	All Enrolled Students	Hispanic Students	Non-Hispanic Black Students	Non-Hispanic White Students
	(1)	(2)	(3)	(4)
Panel A: ELA Exams				
IL Access Indicator	-0.0079	-0.0128	-0.0054	-0.0031
	(0.007)	(0.012)	(0.012)	(0.008)
Observations	37379	9101	8545	34286
Population Weighted IL Indicator	-0.0138*	-0.0163	-0.0056	-0.0082
	(0.008)	(0.012)	(0.014)	(0.009)
Observations	37379	9101	8545	34286
Panel B: Math Exams				
IL Access Indicator	0.0019	0.0119	0.0275	0.0018
	(0.011)	(0.017)	(0.018)	(0.011)
Observations	37329	9426	8561	34224
Population Weighted IL Indicator	0.0133	0.0188	0.0567**	0.0135
	(0.015)	(0.024)	(0.025)	(0.016)
Observations	37329	9426	8561	34224
District & Cohort FE	X	X	X	X
2000 Census Time Trends	X	X	X	X

Notes: All results in this table regress an Imagination Library access parameter on the average school district achievement levels for a specific race. To measure access,the odd columns use an Imagination Library indicator variable and the even columns use the length of time a cohort has access. All estimates include the full panel of results, both district and cohort fixed effects, and a linear time trend of 2000 Census controls. Estimates are weighted using the number of exams in a cohort-school district group. Standard errors are presented in parenthesis. * $p < 0.05$,** $p < 0.01$, *** $p < 0.001$

Table 15: Effects of IL program availability on students of low-socioeconomic status (SES): third grade sample

	All Enrolled Students (1)	Low-SES Students (2)
Panel A: English Language Arts		
IL Access Indicator	-0.0079	-0.0061
	(0.007)	(0.008)
Observations	37379	30784
Population Weighted IL Indicator	-0.0138*	-0.0089
	(0.008)	(0.009)
Observations	37379	30784
Panel B: Mathematics		
IL Access Indicator	0.0019	0.0127
	(0.011)	(0.013)
Observations	37329	30887
Population Weighted IL Indicator	0.0133	0.0358**
	(0.015)	(0.017)
Observations	37329	30887
District & Cohort FE	X	X
2000 Census Time Trends	X	X

Notes: Results in this table are estimates from the standard DD model with and without district level time trends. Results from columns 1 and 2 are estimates based on the full sample of students while results from the latter two columns are based only on exam scores of low-socioeconomic status school districts, as defined by SEDA (Reardon et al., 2021). Estimates in the even numbered columns include a district level time trend of pre-treatment Census controls. Estimates are weighted using the number of exams in a cohort-school district group. Robust standard errors are clustered at the school district level and presented in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 16: Effects of IL after controlling for migration

	Bottom 25% relative migration	Bottom 50% relative migration	Bottom 75% relative migration	Full Sample
IL Access Indicator	0.0125	0.0055	0.0067	0.0007
	(0.010)	(0.008)	(0.008)	(0.008)
Observations	6244	12302	17853	22653

Notes: The estimates are from the basic specification on the third grade ELA sample. Each column represents a subset of the data restricted by the relative amount of migration into the school district. Column 1 is restricted to the lowest 25 percent of migration. Each additional column adds another 25 percent, such that column 4 represents the whole sample. The outcome variable for all estimates is the average achievement score for the entire school district. Estimates are weighted using the number of exams in a cohort-school district group. Standard errors are in parenthesis and are clustered at the district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 17: Effects of IL after controlling for private school enrollment

	Bottom 25% relative enrollment	Bottom 50% relative enrollment	Bottom 75% relative enrollment	Full Sample
IL Access Indicator	-0.0183	0.0033	0.0012	0.0008
	(0.019)	(0.011)	(0.008)	(0.008)
Observations	5244	11285	17238	22651

Notes: The estimates are from the basic specification on the third grade ELA sample. Each column represents a subset of the data restricted by the relative amount of public school enrollment. Column 1 is restricted to the lowest 25 percent of public school enrollment rate among all public school districts. Each additional column adds another 25 percent, such that column 4 represents the whole sample. The outcome variable for all estimates is the average achievement score for the entire school district. Estimates are weighted using the number of exams in a cohort-school district group. Standard errors are in parenthesis and are clustered at the district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 18: Effects from alternate estimator

	3^{rd} Grade ELA Exams (1)	3^{rd} Grade Math Exams (2)	4^{th} Grade ELA Exams (3)	4^{th} Grade Math Exams (4)
B_{fe}	-0.008	0.002	-0.006	0.015
	(0.007)	(0.011)	(0.007)	(0.010)
Observations	37379	37329	33408	33360
DID_M	0.003	-0.002	-0.0003	0.010
	(0.006)	(0.006)	(0.006)	(0.010)
Observations	6736926	6732731	6124370	6142107

Notes: The estimates in this table show how correcting for negative weights in the underlying TWFE model affect the average treatment effect. Estimates in the first block present average treatment effects calculated using the primary specification. Estimates in the second block are calculated following de Chaisemartin & D'Haultfœuille (2020) correction. The DID_M estimator calculates the average treatment effect for the change in outcome variable between the year immediately prior to treatment and the first treated year. The third block presents t-statistics from a t-test where the null hypothesis is equality between the two estimators. The t-statistics are calculated using a bootstrap procedure. Estimates are weighted using the number of exams in a cohort-school district group and standard errors are clustered at the district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

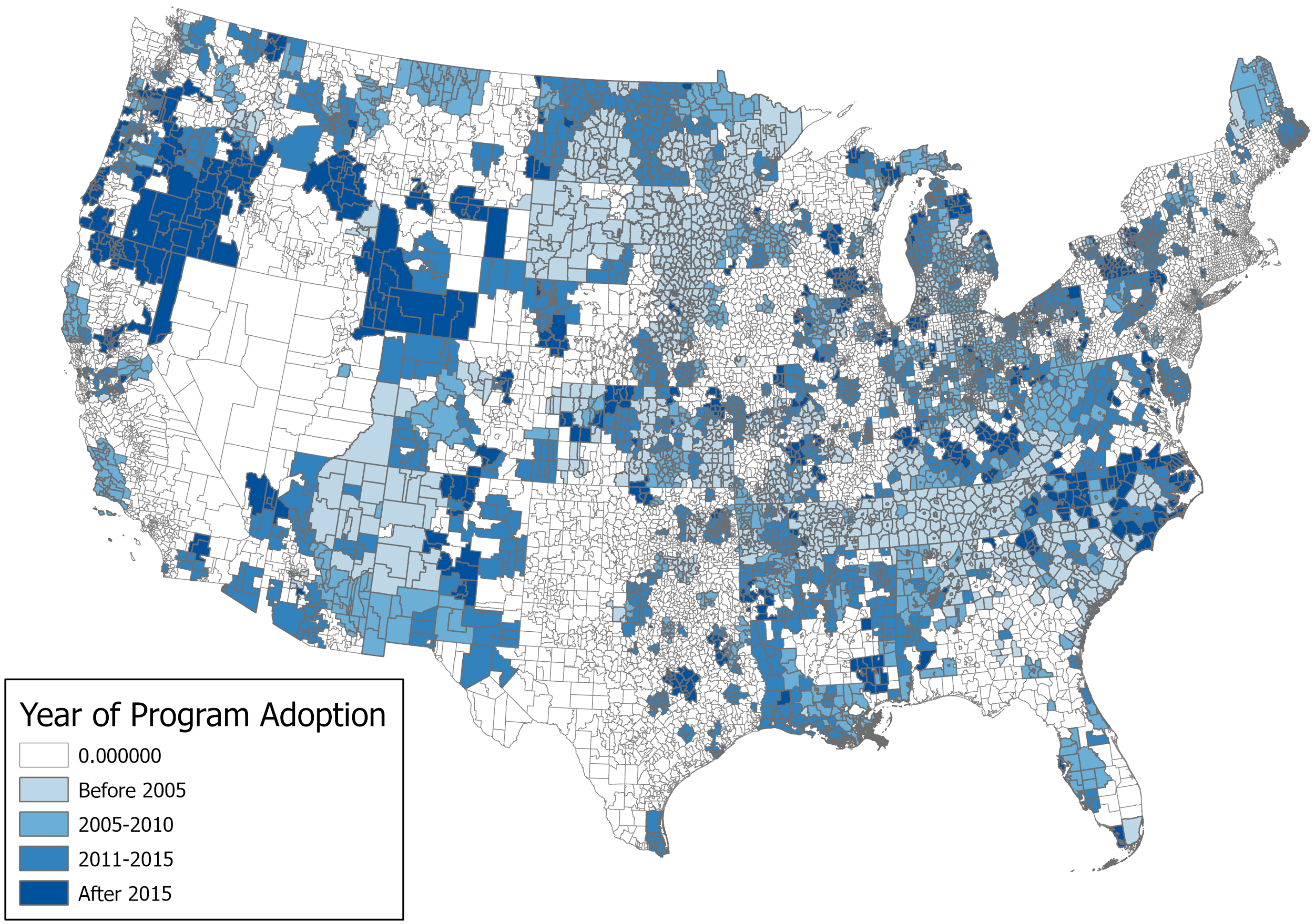


Figure 1: Year of Program Adoption across School Districts

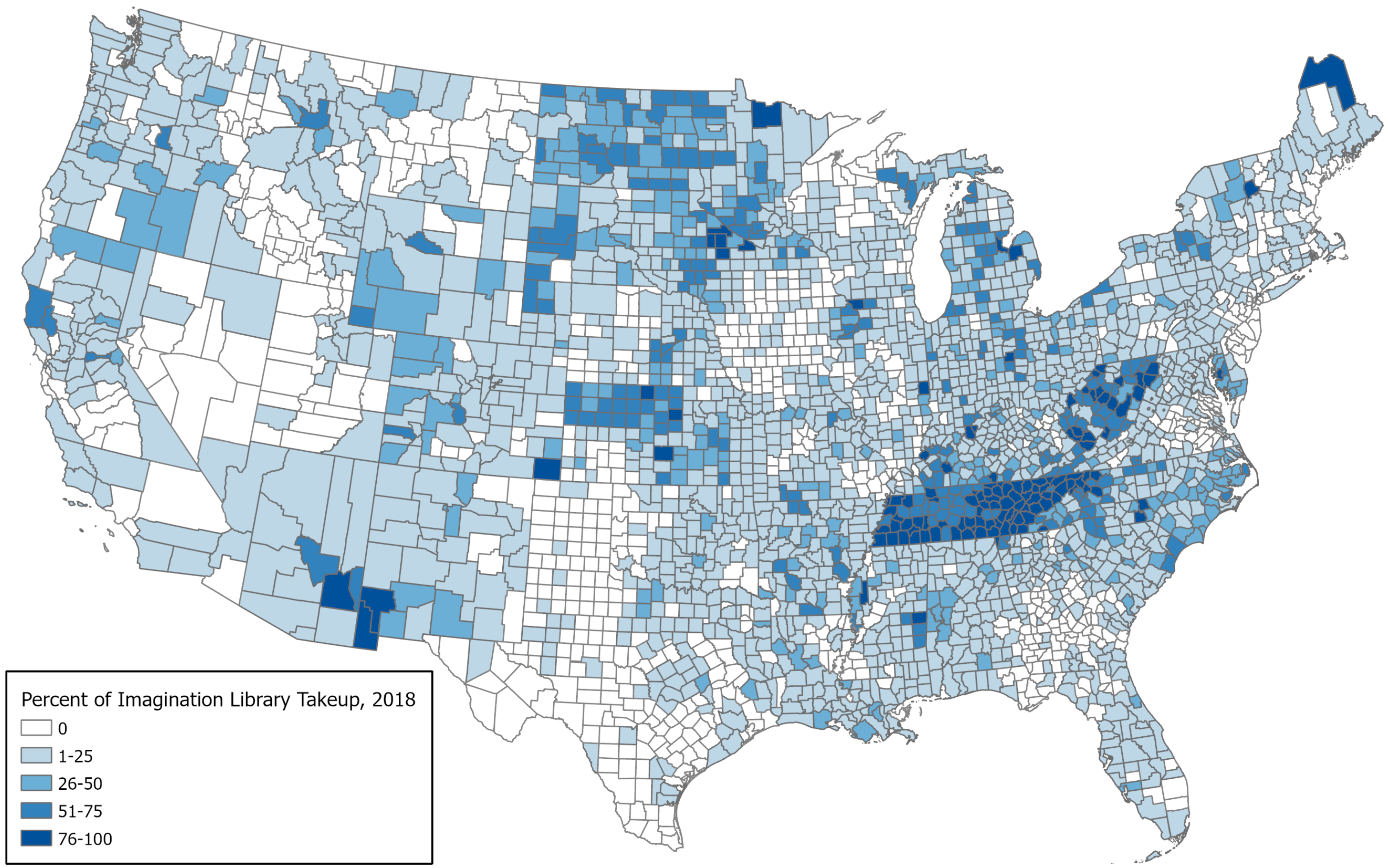


Figure 2: Imagination Library Takeup Rates across the United States, 2018

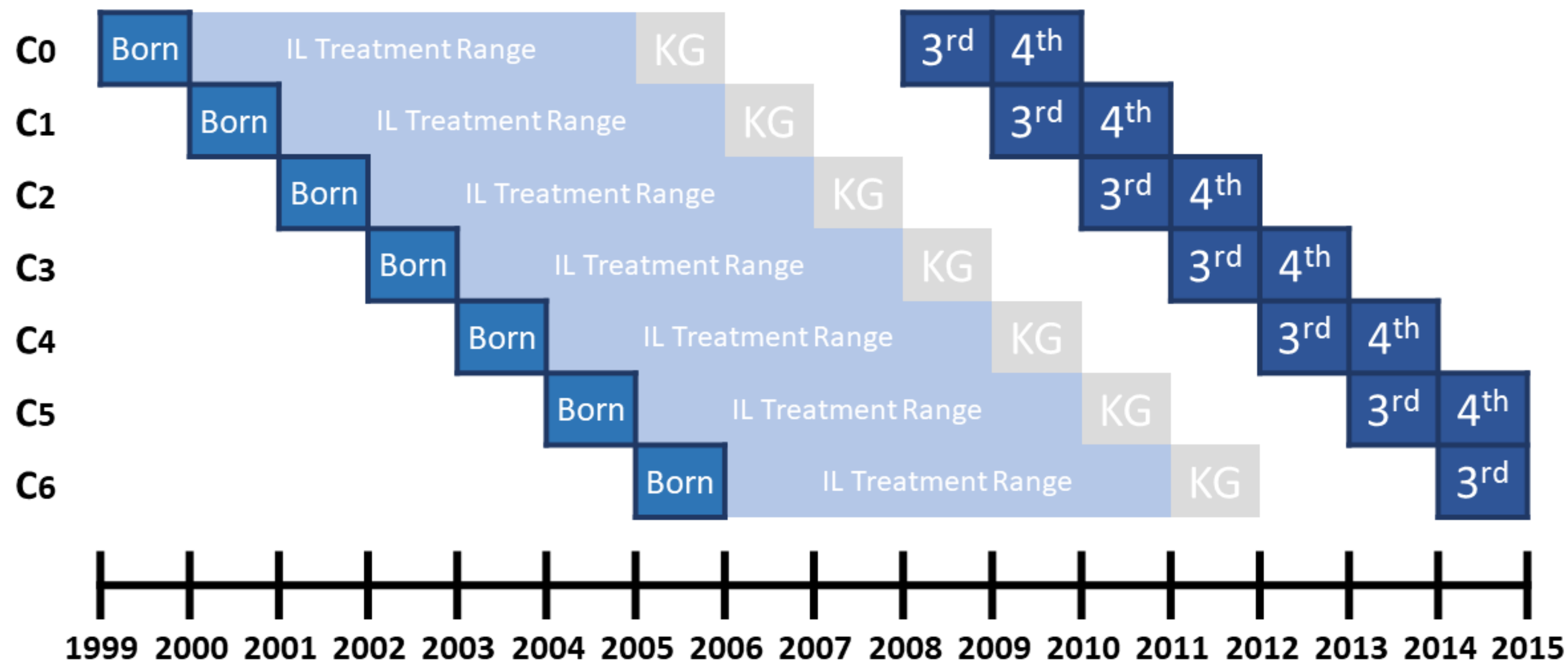
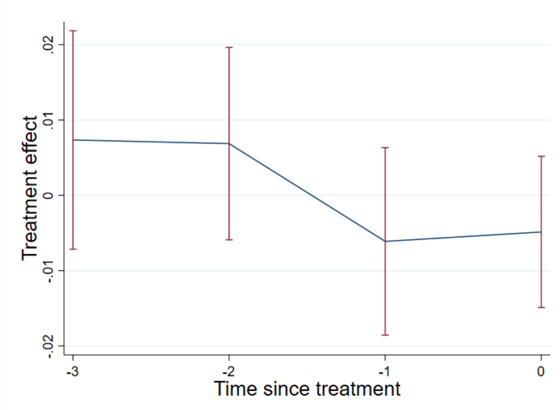
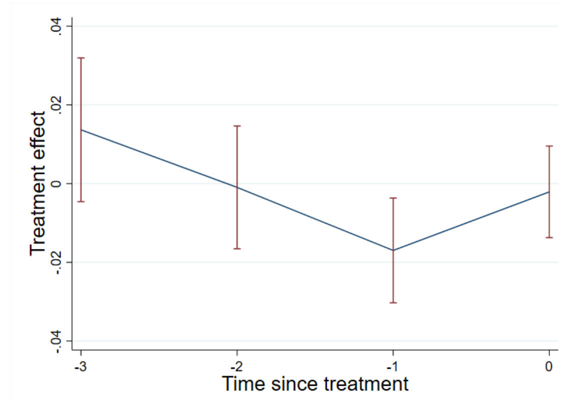


Figure 3: Cohorts represented in the IL and SEDA Data

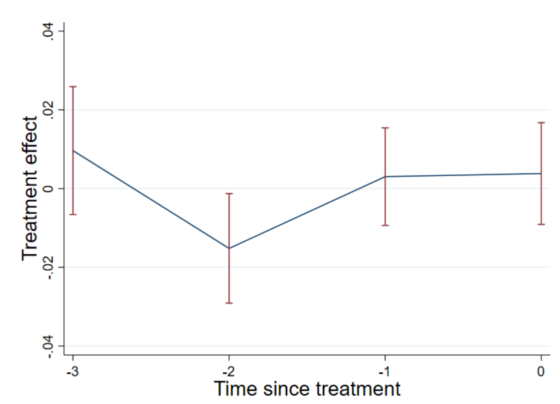
Figure 4: Event Study Estimates of the Effects on Average Achievement



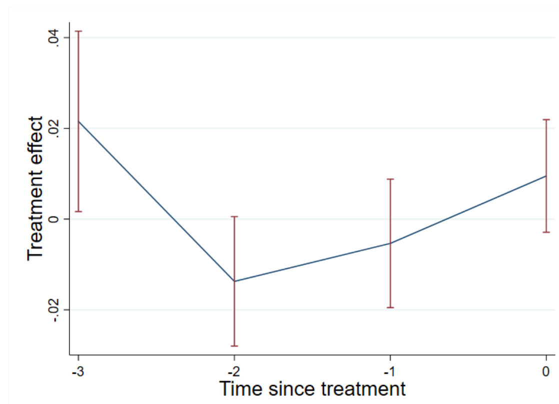
(a) Third Grade ELA



(b) Third Grade Math



(c) Fourth Grade ELA



(d) Fourth Grade Math

Appendices

A Unweighted Results

The primary results in Section 5 rely on regressions that are weighted by the district-cohort size. This places more emphasis on larger school districts and the estimates can be interpreted on the average effect on each student. In this appendix, I present the corresponding unweighted results. These estimates are not weighted, therefore each school district shares equal weight in the average treatment effects. In my scenario, estimates from unweighted regression models can be interpreted as the average treatment effect on the school district average achievement score.
Table A. 1 presents results from all four exams from my primary specification. The estimate in column 1 of -0.0178 suggests that access to the program reduces achievement by 1.78% standard deviations on third grade ELA exams. Column 2 shows an effect of a similar magnitude for third grade Math exams. The estimated effect on fourth grade exams is smaller, although the estimate in column 4 is not statistically different from zero at the 10 percent level. These estimates are similar to the weighted ones presented in Section 5. Many of them are larger, which indicates that a large number of smaller school districts could be driving the large negative unweighted results. The second row of results in Table A. 1 shows estimates of the effects of Imagination Library access using the population weighted access indicator (weights each individual zip code within a school district based on the zip code population). The estimate in column 1 suggests that gaining access to the Imagination Library program prior to kindergarten matriculation leads to a reduction in a cohort's third grade ELA scores by 4.96% of a standard deviation. Estimates in columns 2–4 show a more modest impact on third grade Math and both fourth grade exams. Similar to the first row, all estimates except the coefficient on fourth grade Math exams reach statistical significance at the one percent level. The estimates using the weighted IL indicator variable are much more negative than their counterparts using analytical weights. Again, small school districts are the culprit. The majority of school districts in the sample have small enrollment numbers (X percent of observations have cohort sizes smaller than the mean). Zip codes in small school districts are less likely to adopt the program (see Table 4), driving the population weighted indicator towards zero. The combination of these characteristics of small school districts causes a more negative estimate of the effect of IL when the school districts themselves are unweighted. When we include weights, more emphasis is placed on school districts with larger enrollment numbers, diluting the effects of smaller school districts.
Tables A. 2-A. 8 present more results from unweighted estimators. Overall, these results present estimates that are smaller than their weighted counterparts and are often negative.

Table A. 1: Unweighted Effects of IL Access on Elementary ELA and Math Scores

	3^{rd} Grade ELA Exams	3^{rd} Grade Math Exams	4^{th} Grade ELA Exams	4^{th} Grade Math Exams
IL Access Indicator	-0.0178***	-0.0186***	-0.0106**	-0.0014
	(0.005)	(0.006)	(0.005)	(0.006)
Observations	37379	37329	33408	33360
Population Weighted IL Indicator	-0.0496***	-0.0276***	-0.0348***	-0.0030
	(0.006)	(0.008)	(0.007)	(0.008)
Observations	37379	37329	33408	33360
Cohort & District FE	X	X	X	X
2000 Census Time Trends	X	X	X	X

Notes: This table presents estimates of the effect of having access to Imagination Library on four different exams, each in a separate column. The IL access indicator equals one if the school district had access to Imagination Library for at least one month. The population weighted IL indicator is a measurement weighted by zip code populations that are within the school district boundary. The outcome variable for all estimates is the average achievement score for the entire school district in the respective subject. All estimates include the full panel of control variables, district and cohort fixed effects, and 2000 Census level time trends. The results are unweighted. Standard errors are in parenthesis and are clustered at the district level where appropriate.

Table A. 2: Unweighted Effects of Years of IL Access on Elementary ELA and Math Scores

	3^{rd} Grade ELA Exams	3^{rd} Grade Math Exams	4^{th} Grade ELA Exams	4^{th} Grade Math Exams
Years of IL Access	-0.0015	-0.0009	-0.0005	0.0031
	(0.001)	(0.002)	(0.001)	(0.002)
Observations	37379	37329	33408	33360
Cohort & District FE	X	X	X	X
2000 Census Time Trends	X	X	X	X

Notes: This table presents estimates of the effect of having access to Imagination Library on four different exams, each in a separate column. Each parameter is from a separate regression of the outcome variable on an indicator variable equal to the number of years that a school district had access to the program. The outcome variable for all estimates is the average achievement score for the entire school district in the respective subject. All estimates include the full panel of control variables, district and cohort fixed effects, and 2000 Census level time trends. The results are unweighted. Standard errors are in parenthesis and are clustered at the district level where appropriate.

Table A. 3: Effects of IL program availability on achievement: Third Grade English Language Arts

		Quantiles of Average Achievement			
	OLS	0.10	0.25	0.50	0.75
	(1)	(2)	(3)	(4)	(5)
IL Access Indicator	-0.0178***	-0.0256**	-0.0341***	-0.0257***	-0.0066
	(0.005)	(0.012)	(0.008)	(0.008)	(0.009)
Observations	37379	37485	37485	37485	37485
Population Weighted IL Indicator	-0.0496***	-0.0795***	-0.0570***	-0.0454***	-0.0341***
	(0.006)	(0.016)	(0.010)	(0.009)	(0.010)
Observations	37379	37485	37485	37485	37485

Notes: Results in this table come from an unconditional quantile regression that estimates the effect of having access to Imagination Library for at least one month on the school district average 3rd grade ELA exam scores. All estimates include the full panel of control variables, district and cohort fixed effects, and 2000 Census level time trends. Column (1) represents the effect of an additional year of access to Imagination Library as estimated by an Ordinary Least Squares model for comparison. The results are unweighted. Standard errors are clustered at the school district level and are presented in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A. 4: Effects of IL program availability on achievement: Third Grade Math

		Quantiles of Average Achievement			
	OLS	0.10	0.25	0.50	0.75
	(1)	(2)	(3)	(4)	(5)
IL Access Indicator	-0.0186***	-0.0089	-0.0105	-0.0182**	-0.0250***
	(0.006)	(0.014)	(0.010)	(0.009)	(0.009)
Observations	37329	37439	37439	37439	37439
Population Weighted IL Indicator	-0.0276***	-0.0218	-0.0185	-0.0125	-0.0235*
	(0.008)	(0.020)	(0.013)	(0.012)	(0.014)
Observations	37329	37439	37439	37439	37439

Notes: Results in this table come from an unconditional quantile regression that estimates the effect of having access to Imagination Library for at least one month on the school district average 3rd grade Math exam scores. All estimates include the full panel of control variables, district and cohort fixed effects, and 2000 Census level time trends. Column (1) represents the effect of an additional year of access to Imagination Library as estimated by an Ordinary Least Squares model for comparison. The results are unweighted. Standard errors are clustered at the school district level and are presented in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A. 5: Effects of IL program availability on achievement: Fourth Grade ELA

		Quantiles of Average Achievement			
	OLS	0.10	0.25	0.50	0.75
	(1)	(2)	(3)	(4)	(5)
IL Access Indicator	-0.0106**	-0.0010	-0.0053	-0.0071	-0.0132
	(0.005)	(0.014)	(0.008)	(0.008)	(0.011)
Observations	33408	33497	33497	33497	33497
Population Weighted IL Indicator	-0.0348***	-0.0253*	-0.0288**	-0.0144	-0.0193*
	(0.007)	(0.015)	(0.012)	(0.010)	(0.011)
Observations	33408	33497	33497	33497	33497

Notes: Results in this table come from an unconditional quantile regression that estimates the effect of having access to Imagination Library for at least one month on the school district average 4^{th} grade E:A exam scores. All estimates include the full panel of control variables, district and cohort fixed effects, and 2000 Census level time trends. Column (1) represents the effect of an additional year of access to Imagination Library as estimated by an Ordinary Least Squares model for comparison. The results are unweighted. Standard errors are clustered at the school district level and are presented in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A. 6: Effects of IL program availability on achievement: Fourth Grade Math

		Quantiles of Average Achievement			
	OLS	0.10	0.25	0.50	0.75
	(1)	(2)	(3)	(4)	(5)
IL Access Indicator	-0.0014	-0.0063	-0.0052	0.0009	-0.0035
	(0.006)	(0.012)	(0.012)	(0.010)	(0.011)
Observations	33360	33449	33449	33449	33449
Population Weighted IL Indicator	-0.0030	0.0072	-0.0144	0.0160	-0.0010
	(0.008)	(0.019)	(0.016)	(0.011)	(0.012)
Observations	33360	33449	33449	33449	33449

Notes: Results in this table come from an unconditional quantile regression that estimates the effect of having access to Imagination Library for at least one month on the school district average 4^{th} grade math exam scores. All estimates include the full panel of control variables, district and cohort fixed effects, and 2000 Census level time trends. Column (1) represents the effect of an additional year of access to Imagination Library as estimated by an Ordinary Least Squares model for comparison. The results are unweighted. Standard errors are clustered at the school district level and are presented in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A. 7: Effects of IL program availability on average school district achievement by race: third grade sample

	All Enrolled Students	Non-Hispanic White Students	Non-Hispanic Black Students	Hispanic Students
	(1)	(2)	(3)	(4)
Panel A: ELA Exams				
IL Access Indicator	-0.0178***	-0.0150***	-0.0188*	-0.0194**
	(0.005)	(0.005)	(0.010)	(0.009)
Observations	37379	34286	8545	9101
Population Weighted IL Indicator	-0.0496***	-0.0463***	-0.0432***	-0.0355***
	(0.006)	(0.007)	(0.012)	(0.012)
Observations	37379	34286	8545	9101
Panel B: Math Exams				
IL Access Indicator	-0.0186***	-0.0175***	0.0070	-0.0075
	(0.006)	(0.006)	(0.012)	(0.011)
Observations	37329	34224	8561	9426
Population Weighted IL Indicator	-0.0276***	-0.0219**	0.0118	-0.0148
	(0.008)	(0.009)	(0.016)	(0.014)
Observations	37329	34224	8561	9426
District & Cohort FE	X	X	X	X
2000 Census Time Trends	X	X	X	X

Notes: All results in this table regress an Imagination Library access parameter on the average school district achievement levels for a specific race group. To measure access, the odd columns use an Imagination Library indicator variable and the even columns use the length of time a cohort has access. All estimates include the full panel of results, both district and cohort fixed effects, and a linear time trend of 2000 Census controls. Standard errors are presented in parenthesis. * $p < 0.05$,** $p < 0.01$, *** $p < 0.001$

Table A. 8: Effects of IL program availability on students of low-socioeconomic status (SES): third grade sample

	All Enrolled Students (1)	Low-SES Students (2)
Panel A: English Language Arts		
IL Access Indicator	-0.0178***	-0.0124**
	(0.005)	(0.005)
Observations	37379	30784
Population Weighted IL Indicator	-0.0496***	-0.0479***
	(0.006)	(0.007)
Observations	37379	30784
Panel B: Mathematics		
IL Access Indicator	-0.0186***	-0.0106
	(0.006)	(0.007)
Observations	37329	30887
Population Weighted IL Indicator	-0.0276***	-0.0173*
	(0.008)	(0.009)
Observations	37329	30887
District & Cohort FE	X	X
2000 Census Time Trends	X	X

Notes: Results in this table are estimates from the standard DD model with and without district level time trends. Results from columns 1 and 2 are estimates based on the full sample of students while results from the latter two columns are based only on exam scores of low-socioeconomic status school districts, as defined by SEDA (Reardon et al., 2021). Estimates in the even numbered columns include a district level time trend of pre-treatment Census controls. Robust standard errors are clustered at the school district level and presented in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

B Appendix Tables

Table B.1: Descriptive Statistics: 2000 Census Values

	School Districts without Imagination Library	School Districts with Imagination Library	All School Districts
Percent under 5 years old	0.0715	0.0715	0.0715
Percent over 65 years old	0.111	0.106	0.110
Percent Black	0.108	0.144	0.115
Log of Population	22.64	22.83	22.67
Log of Median Income	10.80	10.72	10.79
Percent living in Urban areas	0.923	0.845	0.909

Table B.2: Covariate Balance Test: Fourth Grade

	Outcome Variable						
	Number of Students	Urban Neighborhood	Percent Black	Percent FRL	Percent ELL	Log Median Income	Unemp. Rate
IL Access Indicator	-2.3593	-0.0025	0.0003	0.0013	0.0016**	-0.0022	0.0003
	(15.089)	(0.009)	(0.000)	(0.002)	(0.001)	(0.001)	(0.000)
Observations	22439	22439	22439	22439	22439	22439	22439

Notes: Each parameter is from a separate regression of that parameter on an indicator variable that equals one when a school district gains access to Imagination Library. The sample includes all observations from fourth grade exams. Estimates are weighted using the cohort-school district enrollment. Standard errors are in parenthesis and are clustered at the school district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.3: Effects of IL program availability on average school district achievement by race: fourth grade sample

	All Enrolled Students	Non-Hispanic White Students	Non-Hispanic Black Students	Hispanic Students
	(1)	(2)	(3)	(4)
Panel A: ELA Exams				
IL Access Indicator	-0.0060	-0.0079	0.0022	-0.0095
	(0.007)	(0.008)	(0.010)	(0.010)
Observations	33408	30701	7738	8254
Population Weighted IL Indicator	-0.0088	-0.0059	0.0045	-0.0161
	(0.008)	(0.009)	(0.012)	(0.012)
Observations	33408	30701	7738	8254
Panel B: Math Exams				
IL Access Indicator	-0.0060	-0.0079	0.0022	-0.0095
	(0.007)	(0.008)	(0.010)	(0.010)
Observations	33408	30701	7738	8254
Population Weighted IL Indicator	0.0298**	0.0293**	0.0597***	0.0200
	(0.013)	(0.014)	(0.020)	(0.019)
Observations	33360	30701	7792	8601
District & Cohort FE	X	X	X	X
2000 Census Time Trends	X	X	X	X

Notes: All results in this table regress an Imagination Library access parameter on the average school district achievement levels for a specific race. To measure access,the odd columns use an Imagination Library indicator variable and the even columns use the length of time a cohort has access. All estimates include the full panel of results, both district and cohort fixed effects, and a linear time trend of 2000 Census controls. Estimates are weighted using the cohort-school district enrollment. Standard errors are in parenthesis and are clustered at the school district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.4: Effects of IL program availability on students of low-socioeconomic status (SES): fourth grade sample

	All Enrolled Students (1)	Low-SES Students (2)
Panel A: English Language Arts		
IL Access Indicator	-0.0060	-0.0009
	(0.007)	(0.008)
Observations	33408	27548
Population Weighted IL Indicator	-0.0088	-0.0023
	(0.008)	(0.009)
Observations	33408	27548
Panel B: Mathematics		
IL Access Indicator	0.0151	0.0274**
	(0.010)	(0.013)
Observations	33360	27715
Population Weighted IL Indicator	0.0298**	0.0488***
	(0.013)	(0.016)
Observations	33360	27715
District & Cohort FE	X	X
2000 Census Time Trends	X	X

Notes: Results in this table are estimates from the standard DD model with and without district level time trends. Results from columns 1 and 2 are estimates based on the full sample of students while results from the latter two columns are based only on exam scores of low-socioeconomic status school districts, as defined by SEDA (Reardon et al., 2021). Estimates in the even numbered columns include a district level time trend of pre-treatment Census controls. Estimates are weighted using the cohort-school district enrollment. Standard errors are in parenthesis and are clustered at the school district level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$