Do State-Funded Pre-K Programs Improve Long-Run Outcomes?

Maya Mikdash* Jiee Zhong†

July 8, 2026

Abstract

We study the long-term effects of access to state-funded early childhood education on educational attainment and labor market outcomes. To do so, we exploit quasi-random variation across districts and cohorts generated by the phased rollout of Texas's large-scale public Pre-K program. Using restricted-access administrative data, we find that access to state-funded Pre-K substantially increases enrollment, particularly in districts without alternative preschool options. Additional results show that in districts that provide Pre-K simultaneously for three- and four-year-olds, access to Pre-K increases earnings at age 27 and high-school graduation by about 4 percent, with no effect on college enrollment. In contrast, access to Pre-K has no effects on labor market and educational outcomes in districts that primarily serve four-year-olds only. We provide suggestive evidence that the effectiveness of state-funded Pre-K depends on program funding rather than access alone.

*Department of Economics, Louisiana State University and IZA Institute of Labor Economics. Email: mayamikdash@lsu.edu.

†Department of Economics, Miami University and IZA Institute of Labor Economics. Email: zhongj8@miamioh.edu.

We thank Chloe Gibbs, Jocelyn Wikle, Taryn Morrissey, Anamarie Auger Whitaker, and seminar and conference participants at Miami University, MidSouth Education Policy Workshop, APPAM 2025, Vancouver Applied Microeconomics Conference 2026, PPCW 2026, and AEFP 2026 for helpful comments. This research was conducted using restricted-access data from the Texas Education Research Center (ERC). The views expressed are those of the authors and do not necessarily reflect those of the ERC or its partner agencies. This paper received no external funding. The authors declare no conflicts of interest.

1 Introduction

In recent decades, the landscape of state-funded prekindergarten (Pre-K) in the United States has changed substantially. State investment in preschool programs has surged, with spending reaching almost $12 billion in 2022–2023—more than double the inflation-adjusted average from 2002–2003 (NIEER (2025a)). At the same time, enrollment has expanded significantly: 35% of four-year-olds now attend state-funded preschool, compared to just 14% in 2002. This growing investment reflects a broader belief in the long-term benefits of early childhood education. Indeed, a substantial body of research suggests that high-quality early childhood programs can yield lasting positive outcomes. However, much of this evidence comes from small-scale, resource intensive interventions such as the Abecedarian Project (e.g., Ramey and Campbell (1984)) or from the federally-funded Head Start program (e.g., Currie and Thomas (1999)). These models differ in funding, scale, and counterfactual conditions from state-funded preschool programs. For instance, the cost of participation in the Abecedarian Project exceeded $100,000 per child, compared to an average of less than $8,000 per child of state-funding for Pre-K in 2024 (Whitaker et al. (2026)). Given these differences, it is unclear whether large-scale early childhood education programs that are implemented in modern settings generate comparable long-term returns.

In this paper, we provide new causal evidence on the effect of access to state-funded Pre-K on long-run educational and labor market outcomes. We focus on the large-scale Texas state-funded Pre-K program, which currently serves more than 50 percent of four-year-olds and 10 percent of three-year-olds (NIEER (2025b)). Our analysis follows more than 100,000 individuals who were age-eligible for Pre-K between 1995 and 2000. To identify causal effects, we exploit plausibly exogenous variation in program availability across cohorts and districts arising from the phased rollout of the program and use restricted-access data from the Texas Education Research Center (ERC), which allow us to observe earnings, high school graduation, college enrollment, and a range of intermediate educational and behavioral outcomes.

In 1985, the state of Texas required school districts with at least 15 eligible four-year-old children to offer free public preschool. Eligibility is determined by characteristics such as household income or English proficiency. As a result, districts with high enough anticipated enrollment offered state-funded Pre-K, while others did not. In addition, districts with at least 15 eligible three-year-old children are permitted—but not required—to offer Pre-K to three-year-olds, creating additional variation in age of exposure. Exploiting variation in the timing of Pre-K availability within and across districts, we compare the outcomes of children who turn four (or three) in districts where Pre-K is available to those of the same age in districts where it is not.

Our first stage results show that Pre-K availability substantially increases public Pre-K enrollment for both eligible and ineligible children: 29-52 percent and 4-14 percent of eligible and ineligible children attend state-funded Pre-K when it is available, respectively. Additionally, we find that positive effects of access to Pre-K are driven by districts that provide Pre-K for both three- and four-year-olds simultaneously. In these districts, access to Pre-K increases positive earnings in the mid- to late-twenties. For instance, those who have access to Pre-K earn about $1,700 more at age 27, which is about a 4% increase relative to the control mean. These increases appear to be driven by an increase in the likelihood of high school completion, but not an improvement in post-secondary educational outcomes. Specifically, we find that access to Pre-K in these districts increases the likelihood of high-school graduation and on-time high-school graduation by about 4%, with no statistically significant impact on college enrollment of any type nor college graduation. In contrast, we find that access to Pre-K in districts that primarily serve four-year-olds does not generate positive long-term returns. Individuals who have access to Pre-K in these districts experience no increases in earnings nor improvements in educational attainment.

To understand the drivers of the long-run returns in districts providing Pre-K for both age groups simultaneously, we estimate the effect of access to Pre-K on intermediate cognitive and non-cognitive outcomes. We find that the long-run improvements in these districts are driven by an improvement in cognitive abilities, as measured by fourth grade math and reading test scores. For instance, those who have access to Pre-K in these districts experience a 23% increase in their fourth grade math test scores. However, we find no evidence of improvements in non-cognitive skills, as measured by the number of disciplinary incidents, juvenile crime, and grade retention. This indicates that access to Pre-K improves long-term outcomes primarily through cognitive channels, opposite to previous evidence (e.g., Chetty et al. (2011), Anders et al. (2023)). In line with the long-run null effects in districts that primarily serve four-year-olds, we find no effects on intermediate cognitive and non-cognitive outcomes in these districts.

We then examine several potential explanations to better understand why the effects of state-funded Pre-K vary substantially across districts. In particular, we consider whether the positive effects observed in districts that provide Pre-K to both three- and four-year-olds simultaneously are driven by differences in counterfactual preschool environments, differences in the timing and duration of exposure, or differences in program resources and implementation.

Results provide little support that counterfactual conditions matter. Specifically, we show that the effectiveness of access to Pre-K does not meaningfully vary by whether alternative preschool options are available within a given district or not. It is also unlikely that differences in the timing or duration of exposure explain the heterogeneous effects across district types, as fewer than 9% of children enroll in Pre-K at age three even in districts where Pre-K is available for three-year-olds. Instead, the results are most consistent with differences in resources and implementation capacity. Districts that generate positive long-run effects tend to be larger, more urban, and substantially better funded, and within these districts, higher levels of Pre-K funding are associated with larger gains in educational and labor market outcomes.

The contribution of this paper is twofold. First, we provide new evidence on the long-run labor market and educational effects of a large-scale, contemporary state-funded Pre-K program using linked administrative data. In doing so, we contribute to a growing literature that studies the importance of early childhood education. The earliest evidence supporting the effectiveness of early childhood programs is drawn from small-scale randomized experiments such as the Perry Preschool Program and the Abecedarian Project. A large body of work documents positive long-term effects of these interventions, including Ramey and Campbell (1984), Campbell and Ramey (1994), Nores et al. (2005), Barnett and Masse (2007), Heckman et al. (2010b), Heckman et al. (2010a) and García et al. (2023).[1] However, these programs differ substantially from contemporary state-funded Pre-K initiatives along several dimensions. Specifically, they are highly intensive programs that incorporate home visits, parental services, and health support in addition to classroom instruction. As a result, average spending sometimes exceeded $100,000 per child in 2023 dollars, more than ten times current state spending on early childhood education. Moreover, these programs were implemented in small samples of highly disadvantaged populations in the 1960s and 1970s, when counterfactual childcare environments differed substantially from those today.

A second strand of the literature examines the impacts of Head Start, a federally funded program targeting disadvantaged children. Numerous studies document positive and persistent effects on educational attainment, labor market outcomes, and behavior (e.g., Currie and Thomas (1999), Garces et al. (2002), Ludwig and Miller (2007), Deming (2009), Bailey et al. (2021), Johnson and Jackson (2019), Barr and Gibbs (2022), and Anders et al. (2023)), while others find limited or null effects (e.g., Pages et al. (2020)). Importantly, most of this evidence comes from cohorts born in the 1970s and 1980s. Since then, the childcare landscape has changed dramatically: private provision has expanded, maternal labor force participation has risen, and childcare costs have increased substantially, altering both the availability and quality of alternative options.

The most recent form of public early childhood education in the United States is state-funded Pre-K. Given the recency of these programs, most existing research focuses on their

[1]For a more comprehensive list, check Currie (2001) and Duncan and Magnuson (2013).

short-term effects. In short, the evidence regarding the efficacy of these programs is mixed and is mainly focused on short-term outcomes, with the exception of Gray-Lobe et al. (2023). For instance, Gormley Jr et al. (2005) show that attending Oklahoma's universal Pre-K program improves short-term test scores. Other studies showing positive impacts on short-term test scores include Cascio (2023), Fitzpatrick (2008), and Wong et al. (2008), all of which focus on universal Pre-K programs. On the other hand, Durkin et al. (2022) show that Tennessee's targeted Pre-K program had negative effects on test scores. Importantly, newer evidence suggests that short-run test score results may not fully capture long-term benefits. For instance, Gray-Lobe et al. (2023) show that Boston's high-resource Pre-K program has no impact on short-term outcomes but increases high school graduation, college enrollment, and SAT scores. Overall, the existing evidence suggests that universal programs tend to generate positive short- to intermediate-term effects, while targeted programs often yield weaker or null results.

Second, our study sheds light on the conditions under which early childhood education programs generate long-run benefits. We document substantial heterogeneity in the effects of Pre-K access across districts, with positive impacts concentrated in districts that appear to have greater resources. These findings help reconcile the mixed evidence on modern state-funded Pre-K programs and the larger effects documented for earlier, more intensive interventions. In doing so, our results contribute to a growing literature that focuses not only on whether early childhood education works, but on the factors that determine when and where it does. Prior research highlights the importance of both counterfactual care environment (e.g., Kline and Walters (2016) and Whitaker et al. (2026)) and program quality in shaping impacts (e.g., Gibbs (2026)). Consistent with the latter, we find that expanding access to Pre-K alone is not sufficient to generate long-run gains. Rather, our evidence suggests that the returns to large-scale Pre-K programs depend on the resources and institutional capacity available for implementation. By identifying district characteristics associated with stronger impacts, our findings provide new evidence on the mechanisms underlying successful early childhood investments and help explain the considerable variation in results across modern Pre-K evaluations.

2 Institutional Background

In 1984, the Texas Legislature enacted a set of major educational reforms aimed at supporting at-risk students, including a mandate requiring school districts to offer *free* half-day (approximately four hours per day) preschool educational services to eligible children. A child is considered eligible if they are low-income, i.e., receiving free or reduced-price lunch, if they are homeless, or if they are considered English learners.[2] Unlike most state-funded Pre-K programs elsewhere in the US, Texas permits school districts to offer tuition-based Pre-K to ineligible children.[3] As a result, even though the program is targeted (free for eligible children only), children from different socioeconomic backgrounds may attend Pre-K together.

According to state law, any school district with at least 15 eligible four-year-old children in a given academic year is required to offer Pre-K services (TXE (2025)). Additionally, school districts with at least 15 eligible three-year-old children are allowed– but not required– to provide Pre-K for three-year-olds as well. According to conversations with the Texas Education Agency, school districts estimate the number of eligible children using prior enrollment data or surveys and determine whether to offer Pre-K based on anticipated demand in a given year.[4]

While large districts with substantial numbers of eligible children generally began offering Pre-K soon after funding became available, smaller and more rural districts were more likely

[2]In 2006-2007, eligibility expanded to include children of armed forces who are in active duty or have been injured or killed on duty. In 2007-2008, foster care children became eligible as well. Later, eligibility included children of individuals who are eligible for the Star of Texas Award (e.g., law enforcement officers).

[3]Tuition is determined by the school district and must be approved by the commissioner of education at the Texas Education Agency (TEA) (Texas Education Agency (nd)).

[4]School districts estimate the number of eligible children using several methods, including: school system divisions dedicated to enrollment analysis based on birth rate studies, collaborations with colleges and universities to conduct enrollment analyses, or using historical enrollment trends within the school system.

to be affected by the 15-student threshold and consequently adopted Pre-K at different points in time. For example, the Houston Independent School District has offered Pre-K services since the 1985-1986 school year (HISD, Department of Research and Accountability (2017)), whereas many smaller districts adopted the program years later.

This institutional setting generates quasi-random variation in access to state-funded Pre-K across districts and cohorts. As a result, following 1985, some individuals became age-eligible for Pre-K (turned four or three of age) in districts where state-funded Pre-K is available for their age group, while others became age-eligible in districts where it is not. Figure A1 shows the number of districts offering Pre-K services over time. As shown in the figure, more than 40% of school districts provided Pre-K in 1994, with coverage gradually increasing over time. By 2023, more than 50% of four-year-old children in Texas were enrolled in state-funded Pre-K, compared to only 6% enrolled in Head Start.[5]

Districts with 15 or more eligible four-year-old children receive state Pre-K funding through the Foundation School Program (FSP) to provide Pre-K services for eligible students. Local Education Agencies (LEAs) may supplement state funding using other local revenue sources. In 2001, the state spent slightly more than $2,000 per enrolled child, approximately one-third of per-child spending in Head Start (NIEER (2024)). Although state funding in Texas has increased over time, it remains below national averages. For example, in 2023, Texas spent $4,235 per enrolled child, compared to roughly $7,000-$8,000 per child nationwide.

According to a study conducted by the Texas Education Agency (TEA) in 1992, state-funded Pre-K is primarily housed on traditional school campuses, with some districts partnering with child care or early learning centers (of Policy Planning and Evaluation (1992)). However, only 5% of campuses share facilities and classroom instruction with other agencies, including Head Start. On average, the adult-child ratio is 1:11, with 22 students per class. Most teachers have six or more years of experience, but less than half of them have

[5]Source: NIEER, 2023

received any formal training in early childhood education. In addition, unlike programs such as Head Start, parental involvement is minimal. Schools primarily communicate with parents through calls or teacher-parent conferences, and home visits are not part of the program. In the 1990s, Texas state-funded Pre-K did not follow a single mandated statewide curriculum. Instead, districts implemented early childhood instructional activities based on local decisions and professional practice.

More formal curriculum guidance was developed and implemented statewide in the late 2000s. For instance, Texas developed the "Texas Pre-K Curriculum Guidelines" in 2008, when professional training was offered to early childhood education providers in the state (NIEER (2012)). In 2015, Texas implemented the High-Quality Prekindergarten Components to strengthen the delivery of Pre-K services statewide. These guidelines address multiple areas, including curriculum standards, reporting of student progress, teacher qualifications, and teacher-to-student ratios. For example, Pre-K teachers are now required to hold a current Texas teaching certification and complete specialized training in early childhood education. While Pre-K guidelines have evolved over time, school districts have always been required to implement Pre-K curricula that span areas related to social and emotional development, as well as intellectual development in areas such as math, science, language skills, physical development, and technology.

3 Data

We use the restricted-access data maintained by the Texas Education Research Center (ERC), which track the records of every individual who attends public school in Texas. The ERC combines Pre-K through twelfth grade (PK-12) educational records from the Texas Education Agency (TEA) with post-secondary educational records from the Texas Higher Education Coordinating Board (THECB), post-secondary education records from other states via the National Student Clearinghouse (NSC), and earnings information from the Texas

Workforce Commission (TWC). To link these data, we use a uniquely generated identifier that serves as a substitute for the Social Security Number (SSN). This allows us to observe each individual's Pre-K access information, as well as intermediate- and long-term educational, behavioral and labor market outcomes. Most datasets are available from 1994 to 2023. Below is a discussion of how we measure treatment and define our main outcomes of interest.

3.1 Sample selection and treatment definition

Using the Texas Education Agency (TEA) data, we observe educational records for all individuals attending public schools in Texas, including their school district, enrollment, attendance, test scores, and disciplinary outcomes from Pre-K through grade 12.

An individual is considered treated if they turn four (or three) in a school district that offers Pre-K services for four- (or three-) year-old children, and untreated otherwise. Since school district information is observed based on enrollment, Pre-K district information is unknown for individuals who do not attend Pre-K. Hence, we use an individual's kindergarten district as a proxy for the district in which they become age-eligible for Pre-K. To assess the validity of this approach, we examine the likelihood that the individuals attending Pre-K, and for whom Pre-K district information is available, attend kindergarten in the same district. Among these students, nearly 90% attend kindergarten in the same district in which they attended Pre-K, suggesting that kindergarten district is a reliable proxy for district of residence at the time of Pre-K eligibility.

We use district-level Pre-K enrollment data to determine whether a district offers Pre-K in a given year. In any given year, a district is classified as offering Pre-K if at least one student is enrolled in Pre-K in that year, indicating that Pre-K services are available. We define $t = 0$ for each district as the first year in our sample with positive Pre-K enrollment. Based on this definition, we classify districts into three treatment groups: (1) districts that, at $t = 0$, provide Pre-K for both three- and four-year-olds simultaneously (20 percent), (2) districts that, at $t = 0$, provide Pre-K for four-year-olds only and never offer Pre-K for three-year-olds during our sample period (27 percent), and (3) districts that, at $t = 0$, provide Pre-K for four-year-olds only but later expand to include three-year-olds (7 percent). Control districts are defined as those that do not offer Pre-K during our sample period.

As we show in Figure A1, the timing of Pre-K availability varies across districts. In 1994, Pre-K for four-year-olds (three-year-olds) was available in 44 percent (22 percent) of the school districts. These include districts that began offering Pre-K in 1994 or before. Since enrollment data begin in 1994, we are unable to distinguish between districts that begin offering Pre-K in 1994 and those that do so before. We therefore exclude districts with positive Pre-K enrollment in 1994 from our main analysis. Since, in these districts, prior availability is unknown, treatment status cannot be determined for children who become age-eligible for Pre-K before 1994. Among the remaining districts, Pre-K availability expands gradually over time, generating variation in exposure across cohorts and districts. In total, our sample includes 431 school districts.

Since we are interested in examining the effect of Pre-K access on long-term outcomes, we restrict our sample to individuals who attend kindergarten between 1994 and 2001. This allows us to observe high-school graduation, college enrollment and graduation, in addition to earnings until age 27 for all the cohorts in our sample.[6] Our final sample consists of 132,293 individuals who attended kindergarten in Texas between 1994 and 2001.

3.2 Main outcome variables

Our key outcome of interest to measure long-term economic self-sufficiency is earnings. Using the Texas Work Commission (TWC) data, we observe quarterly earnings information for every individual who ever worked in Texas, which we use to calculate annual earnings at every age between 23-27.[7] Note that earnings are only observed for individuals who are

[6]This also allows us to observe kindergarten district to proxy for Pre-K district for all in-sample individuals given that enrollment data begin in 1994.

[7]As of the latest draft, earnings data are available through 2023.

employed in Texas and where the employer pays at least $1,500 in gross wages to employees or have at least 1 employee during 20 different weeks in a calendar year. This means that earnings are missing if an individual is unemployed, is not participating in the labor force, is self-employed, or if they are employed in a different state. However, the unemployment rate in Texas is 3% (US Census Bureau (2023)), the nationwide self-employment rate is about 1.9% (Hipple and Hammond (2016)), and the share of Texans relocated and paid tax outside of Texas in 2021–22 is only 1.8% (Internal Revenue Service (2022)). In addition, according to the US Census Bureau (2023), 35% of Texans over age 16 are out of the labor force, and the low participation rate is more pronounced among young adults. This indicates that if an individual's earnings are unobserved in any given year, it is most likely due to unemployment or lack of participation in the labor force. In our analysis, we consider the effect of access to Pre-K on positive earnings (excluding missing values) at ages 23-27. To assess whether this measure is biased due to missing earnings, we also estimate the effect on the probability of having positive earnings at each age and on unconditional annual earnings (by replacing missing values with zeros).

In addition, we observe several measures of long-term educational achievement, including high-school graduation and college outcomes. First, we estimate the impact on the likelihood of ever graduating high-school, which is a binary variable that takes the value one if an individual ever obtains a high-school diploma, irrespective of the timing. We obtain this information from the Texas Education Agency (TEA), which is available from 2003 through 2023. We also examine the impact on on-time high-school graduation, defined as completing high-school within 13 years after kindergarten. Second, we examine the impact on post-secondary outcomes, including college enrollment and the type of institution attended. These data are obtained from the Texas Higher Education Coordinating Board (THECB) and the National Student Clearinghouse (NSC), which together allow us to track enrollment in both in-state and out-of-state institutions. College information is available from 2006 through 2021. We construct an indicator variable equals to one if an individual ever enrolls in any post-secondary institution, regardless of institution type, and zero otherwise. We also estimate effects on enrollment by institution type, focusing separately on public universities and community colleges. Additionally, we examine the effect of Pre-K access on on-time college enrollment, defined as enrolling in college within 13 years after kindergarten. Finally, we consider the impact on college graduation, which is a binary variable equals one if an individual ever graduates college, and zero otherwise.

3.3 Secondary outcome variables

We use the Texas Education Agency (TEA) data to measure outcomes prior to college enrollment. Specifically, this database contains school enrollment records and students' test scores for the state standardized assessment. To measure the impact on individual test scores, we standardize the test scores by subtracting the mean and dividing by the standard deviation of a subject-grade/year-cohort. We focus on third and fourth grade reading and math test scores as a measure of intermediate academic performance. We also examine the effect of Pre-K access on the likelihood of having missing third and fourth grade test scores. A missing test score can be due to absence due to sickness or other reasons (e.g., cheating), failure to take the first attempt, or exiting the Texas school system.[8]

In addition to test scores, we consider the effect on grade retention, absenteeism, special education, and disability, medical, or therapy services. For each one of these outcomes, we measure the total number of incidents recorded from kindergarten until 12th grade. Finally, the Texas Education Agency (TEA) data also allow us to observe every individual's disciplinary records, including minor infractions and more serious offenses that result in a criminal charge. We use this information to estimate the impact of access to Pre-K on the total number of disciplinary incidents from kindergarten until 12th grade. We also observe criminal records beginning in 1999, which we use to measure the impact on juvenile crime

[8]Beginning in 2003, individuals have three attempts to pass their third grade exams. If they fail the test after the three attempts, they are required to repeat the grade. In our sample, only the 1999 and 2000 Pre-K cohorts are affected by this grade retention policy. For all individuals in our sample, we focus on the test scores from the first attempt.

within 5 to 12 years post kindergarten.[9] Table A1 shows a list of criminal incidents that appear in the data.

3.4 Supplementary data

In addition to the Education Research Center (ERC) data, we collect information on the availability of alternative preschool options in any given district, including Head Start and private preschool. Data on Head Start centers come from the Program Information Reports compiled by the Office of Head Start (Office of Head Start (2026)), while data on private preschool providers come from the Private School Universe Survey (National Center for Education Statistics (2000)). Using these sources, we construct a measure of alternative preschool availability at the zip-code level. A zip-code is classified as having an alternative preschool option if it contains at least one Head Start center or private preschool provider throughout our sample period. We then aggregate this measure to the school district level using the set of zip-codes that overlap with each district. A district is considered to have access to alternative preschool options if at least one overlapping zip-code contains a Head Start center or a private preschool provider. As shown in Table 1, under this definition, we find that 64 percent of districts with Pre-K for both three- and four-year-olds have an alternative preschool option (Column (2)) compared to 39 percent and 42 percent in districts that only serve four-year-olds (Column (3)) and districts that sequentially expand to three-year-olds (Column (4)), respectively. Similarly, 41 percent of districts that never provide Pre-K have other available preschool options.

3.5 Descriptive statistics

In Table 1, we show descriptive statistics for individuals in our sample across different types of districts. Column (1) shows the average outcomes for individuals who are in the

[9]We focus on 5 to 12 years post kindergarten to ensure that we observe the same number of years of the outcome of interest for all the cohorts in our sample.

control districts, i.e., districts that never provide Pre-K during our sample period. Column (2) shows the average outcomes for individuals who attend kindergarten in districts that provide Pre-K for both three- and four-year-olds simultaneously. Column (3) shows the average outcomes for individuals who attend kindergarten in districts that offer Pre-K for four-year-olds only, while Column (4) shows the average outcomes for individuals who attend kindergarten in districts that offer Pre-K for four-year-olds only first and later expand to serve three-year-olds as well.

We also show district-level characteristics across all four types of districts. As can be seen in the table, most of the districts in our sample – regardless of Pre-K accessibility status – are considered to be rural districts. For instance, Columns (1)-(4) show that 54-66 percent of the districts are considered "rural." The most notable differences across district types are average Pre-K funding and size. As shown in the table, districts that offer Pre-K for both age groups simultaneously are significantly larger than all other districts, with an average kindergarten enrollment of 78 (Column (2)), relative to 35-48 in other districts (Column (1),(3) and (4)). They are also slightly more likely to be considered urban or suburban.[10] Additionally, average per-child Pre-K funding is substantially higher in these districts relative to others. While per-child Pre-K funding is more than $13,000 in districts that offer Pre-K for both age groups at the same time (Column (2)), other districts with Pre-K access spend between $482 (Column (4)) and $1,510 (Column (3)), on average. On the other hand, average K-12 per-student funding does not seem to meaningfully vary across treated districts, but it is slightly higher in control districts (almost $20,000/child in control districts vs $17,000-19,000/child in treated districts).

Across all groups, around 50% of individuals are male. Individuals in districts providing Pre-K for 3- and 4-year-olds simultaneously (Column (2)) and in districts that provide Pre-K for 4-year-olds only (Column (3)) are slightly less likely to be White relative to other

[10] In the data, districts are labeled as one of the following: charter school districts, independent town, major suburban, non-metropolitan fast growing, non-metropolitan stable, other central city suburban, or rural.

districts (73-77 percent compared to 80-82 percent) and are slightly more likely to be Black or Hispanic. Districts providing Pre-K for four-year-olds first (Columns (3) and (4)) are more likely to have economically disadvantaged individuals. For instance, Column (3) shows that 35 percent of individuals in districts that only offer Pre-K for four-year-olds are economically disadvantaged, relative to 30 percent in the control group. Finally, individuals in districts that offer Pre-K for both age groups (Column (2)) simultaneously are more to have limited English proficiency compared to all other groups (11 percent relative to 3-6 percent).

Turning to our main outcomes of interest, average positive annual earnings (excluding zeros) at age 27 for individuals in the control group is $45,830 (Column (1)), compared to $45,750 for those who attend kindergarten in districts that provide Pre-K for three- and four-year-olds (Column (2)), $44,070 for those who attend kindergarten in districts that provide Pre-K for four-year-olds (Column (3)), and $44,020 for those who attend kindergarten in districts that provide Pre-K for four-year-olds then three-year-olds (Column (4)). In addition, the likelihood of having positive earnings at age 27 is relatively similar across all districts, with individuals in districts with Pre-K access for both age groups simultaneously having the lowest likelihood of 58 percent (Column (2)), relative to 62 percent in other districts (Columns (1), (3) and (4)).

In terms of educational outcomes, individuals across all districts have similar fourth and third grade test scores. For instance, an average student scores 0.1-0.2 standard deviations above the mean in both reading and math in all four types of districts. Additionally, the likelihood of graduating high school for individuals in the control group (Column (1)) is about 73 percent, slightly higher than the likelihood of graduating high-school for individuals in districts that provide access to Pre-K (70-72 percent (Columns (2)-(4)). Similarly, the likelihood of college enrollment is slightly higher for individuals in the control group relative to others (64 percent vs 60-62 percent), with small differences in the likelihood of enrolling in community college or public university across the different groups. This is true for the timing of high-school graduation and college enrollment, with the likelihood of on-time high school graduation being between 70 and 72 percent.

Finally, individuals in all four groups are similar in terms of observable behavioral outcomes. For instance, on average, individuals across all types of districts have a total of 3 to 4 disciplinary incidents within 12 years of kindergarten and 0 to 1 crimes committed 5 to 12 years post kindergarten. On average, individuals in all groups have repeated the same number of grades (<1), have participated in special education for less than a year, and have the same number of years with disability (about 1.2). In addition, they have received less than 1 therapy services within 12 years of kindergarten.

4 Research Design

4.1 Empirical Strategy

To estimate the impact of access to state-funded Pre-K, we exploit variation in program availability across districts and cohorts generated by the gradual rollout of the program in Texas. We use a staggered difference-in-differences setting, comparing the outcomes of individuals who turn four (three) in districts where state-funded Pre-K is available to those who turn four (three) in districts where it is not, controlling for district and cohort fixed effects. Recent literature shows that using a two-way fixed effects model in a staggered difference-in-differences setting may lead to biased results if effects are heterogeneous across groups and/or over time (Borusyak and Jaravel (2018), De Chaisemartin and d'Haultfoeuille (2020), Goodman-Bacon (2021), Sun and Abraham (2021)). To address this concern, we use the interaction-weighted estimator from Sun and Abraham (2021) as our main estimator. We describe the estimation procedure in three steps.

Step 1. Using Sun and Abraham (2021), we estimate the dynamic treatment effects on the treated for each group that initiates Pre-K in year e using the following equation:

$$Y_{i,d,t} = \alpha_d + \lambda_t + \sum_{e \notin C} \sum_{\ell \neq -1} \delta_{e,\ell} \, \mathbf{1}\{E_d = e\} \, D^{\ell}_{i,d,t} + \varepsilon_{i,d,t} \tag{1}$$

Where $Y_{i,d,t}$ is the outcome of interest, such as earnings, college enrollment, high school graduation, or test scores, for individual i in district d and cohort t. $D^{\ell}_{i,d,t}$ is an indicator variable that takes the value 1 if an individual i turns four (or three) in district d, ℓ periods relative to the first year that Pre-K becomes available. E_d represents the period in which district d initiates Pre-K, and $1\{E_d = e\}$ is an indicator variable that equals one if district d initiates Pre-K in year e, where $e \in \{1995, 2000\}$. This indicates that for every district d that provides Pre-K in year e, we observe at least one treated and one untreated cohort.[11]

In all our estimations, we control for district fixed effects (α_d) to account for district time-invariant characteristics in addition to cohort fixed effects (λ_t) to account for period-specific shocks. We cluster the standard errors at the school district level to account for correlations in the outcomes of individuals attending school in the same district.

Step 2. Based on the sample shares of each group e in relative time period l, we estimate the weights below:

$$\widehat{\Pr}\{\, E_d = e \mid E_d \in [-\ell,\ T - \ell] \,\} \tag{2}$$

Step 3. The interaction-weighted estimator is constructed in two stages. First, for each relative period ℓ, we compute a weighted average of the group-specific estimates $\widehat{\delta}_{e,\ell}$ from Step 1, using the group-share weights from Step 2 — this yields a single treatment-effect estimate for each relative period ℓ, averaged across treatment groups e. Second, we average these period-specific estimates across all relative periods ℓ in the post-treatment window g, dividing by the number of periods $|g|$. The resulting interaction-weighted estimator is:

[11] For instance, in districts that provide Pre-K for four-year-olds in 1995, we observe the treatment status and outcomes of interest for individuals who turn four in 1994, who would be considered the only "control" cohort. For districts that provide Pre-K for four-year-olds in 2000, we observe the treatment status and outcomes of interest for individuals who turn four in 2001, who would be considered the only "treated" cohort in these districts.

$$\widehat{v}_g = \frac{1}{|g|} \sum_{\ell \in g} \sum_{e} \widehat{\delta}_{e,\ell} \, \widehat{\Pr}\{\, E_d = e \mid E_d \in [-\ell,\ T-\ell] \,\} \tag{3}$$

Throughout the paper, we estimate the effect of access to Pre-K separately by district type. For districts that provide Pre-K for both three- and four-year-olds simultaneously, an individual is considered treated if they turn three after Pre-K becomes available in their district. This means that in these districts, since an individual may attend Pre-K at both ages, individuals who turn three in ℓ = -1 are considered to be the first treated cohort. In contrast, for districts that mainly serve four-year-olds, an individual is considered treated if they turn four after Pre-K is available in their district, and the first treated cohort are those who turn four in $\ell = 0$.

The validity of our method depends on (1) the parallel trends assumption and (2) the relevance of the treatment. Under (1), we assume that absent treatment, the outcomes of individuals who have access to Pre-K would have continued to trend the same way as the outcomes of individuals who have no access to Pre-K. Under (2), we assume that the presence of state-funded Pre-K induces enrollment in districts where it is available. In the next subsection, we provide empirical evidence that supports these assumptions. First, we estimate dynamic treatment effects using Sun and Abraham (2021) for our outcomes of interests to examine the pre-trends. Second, we estimate the effect of Pre-K availability on the likelihood of Pre-K enrollment.

Finally, the validity of our estimates depends on the assumption that the timing of Pre-K adoption is uncorrelated with other contemporaneous changes at the district level. One potential concern is that families may sort across districts in response to anticipated Pre-K availability, leading to changes in the composition of students exposed to the program. Such sorting could bias our estimates if the characteristics of students entering treated districts differ systematically from those of existing residents.

We address this concern in two ways. First, we examine whether the demographic composition of student cohorts changes following the introduction of Pre-K by estimating Sun and Abraham (2021) using indicators for ethnicity, low-income status, and English proficiency as outcome variables. If Pre-K availability induces substantial sorting, we would expect these characteristics to change differentially in treated districts relative to untreated districts after the introduction of Pre-K. Second, we assess the robustness of our main results to controlling for these observable characteristics at the individual level, including ethnicity, low-income status, and English proficiency.

4.2 Validity tests

In this section, we provide empirical evidence that supports our identification assumptions. First, while we cannot directly test for parallel trends, we assess the plausibility of this assumption using event-study specifications. We discuss these results extensively in the next subsections (subsection 5.1, subsection 5.2), but in sum, the results show no evidence of divergence in our outcomes of interest across treatment and control before the introduction of state-funded Pre-K (Figure 3-Figure 5). These patterns support the assumption that, absent access to Pre-K, the outcomes of treated and untreated individuals would have continued to trend similarly.

Second, to show that the policy change is relevant, we estimate the effect of Pre-K availability on the likelihood of Pre-K enrollment. The results in Figure 1 show the coefficient estimates that represent the impact of access to Pre-K on enrollment in Pre-K for each cohort. As shown in the figures, there is no change in enrollment prior to treatment, consistent with how we construct the treatment variable. Following the introduction of Pre-K at $t = 0$, across all three types of districts, we observe an increase in Pre-K enrollment of treated cohorts relative to untreated ones, primarily driven by individuals who are eligible for Pre-K.

This is also supported in Table 2, where we estimate the effect of access to Pre-K on enrollment using Equation 3. For instance, in Panel A of Table 2, Columns (2) and (3) show that when Pre-K becomes available in districts that provide Pre-K for both three- and four-year-olds simultaneously, enrollment increases by about 4 and 29 percent for ineligible and eligible individuals, respectively. Similarly, Panel B shows that after Pre-K becomes available in districts providing Pre-K for four-year-olds only, 10 percent and 38 percent of ineligible and eligible children enroll in Pre-K, respectively. Finally, Panel C shows that enrollment increases by 13 percent and 42 percent for ineligible and eligible children, respectively, after Pre-K becomes available for four-year-olds in the third type of districts. After the expansion to three-year-olds in these districts, enrollment for both eligible and ineligible individuals experiences a modest increase relative to the pre-expansion period.

As mentioned earlier, the validity of our estimates also depends on the assumption that there is no change in the composition of students as a result of Pre-K access. This could potentially arise due to (1) families sorting across districts based on Pre-K availability, and/or (2) systemic attrition. For instance, access to Pre-K might impact the likelihood that an individual remains in the public school system at a later grade.

We address the first concern by testing whether access to Pre-K is associated with changes in observable student characteristics. Specifically, we use Sun and Abraham (2021) to estimate the impact of Pre-K availability on the share of Black, Hispanic, limited English proficiency, and low income students in a given district. We report the results in Figure A2, Figure A3, and Figure B1 for all three district types. Across all twelve sub-figures, there is mostly no evidence of a correlation between access to Pre-K and individual characteristics. However, in Figure A2, individuals who turn three after Pre-K becomes accessible are slightly more likely to be Hispanic (Figure A2b), and in Figure A3d, individuals who turn four after Pre-K becomes accessible are more likely to be low-income.

In theory, if individuals who turn three (or four) after Pre-K becomes available are more likely to be low income or Hispanic, this could lead to attenuating our main estimates towards zero. To address this, we demonstrate the robustness of our results to controlling for individual observable characteristics, including ethnicity, income, and English proficiency status (Table A3, Table A4, and Table B4). We discuss these results in more depth in section 5, but in sum, controlling for individual observable characteristics does not affect our main estimates of interest.

Second, to assess whether there is a correlation between public Pre-K access and attrition, we estimate the effect of access to Pre-K on the likelihood of dropping out and the likelihood of exiting the Texas Public School System. Under Texas law, individuals may legally drop out of school at ages 17 or 18. We define "ever dropping out" as equal to 1 if an individual is observed to drop out between 2000 and 2013, and 0 otherwise. Exit is defined as equal to 1 if an individual leaves the Texas Public School System anytime between 1995 and 2015. Individuals may exit the public system for reasons including switching to private school, out-of-state school, or home schooling. The results are reported in Table A2 and show no evidence that access to Pre-K affects drop out or exit rates across any district type.

Taken together, the evidence presented in this section supports the validity of our identification strategy. In sum, we find no evidence of differential pre-trends, a strong first stage, and little evidence of bias due systemic compositional changes across districts with and without access to Pre-K.

5 Results

5.1 Effects on labor market outcomes

We begin by examining the impact of access to Pre-K on earnings between ages 23-27. We do so separately for each district type. As a reminder, in districts that provide Pre-K for both three- and four-year-olds simultaneously, treatment is defined as turning three in a district where Pre-K is available. To observe the outcomes of interest, including earnings until age 27 for all individuals, we focus on individuals who attend kindergarten between 1994 and 2001. As we explain in section 3, earnings are observed for individuals who are employed within the state of Texas. Hence, we first estimate the effect on positive annual earnings, i.e. conditional on observing earnings. In addition, we estimate the effect on the likelihood of having positive earnings and on unconditional annual earnings, replacing missing values with zeros.

We begin by estimating the event-study models using Sun and Abraham (2021), where the outcome variable is positive earnings at age 27. The results are shown in Figure 2, where we report the estimated coefficients for each cohort with the corresponding 95% confidence intervals. In all figures, standard errors are clustered at the school district level. First, across all three types of districts, we see no evidence of divergence in the pre-trends across treatment and control.[12] This provides supporting evidence that absent treatment, the earnings of individuals who turn four (or three) in a district where Pre-K is available would have continued to trend similarly to those who turn four (or three) in a district where Pre-K is unavailable.[13]

Second, in districts providing Pre-K for three- and four-year-olds simultaneously, we show evidence of an increase in positive earnings at age 27 for individuals who have access to Pre-K compared to those that do not (Figure 2a), whereas in districts that provide Pre-K for four-year-olds only, we see no evidence of a change in earnings at age 27 for exposed cohorts (Figure 2b). Similarly, in districts that expand access for three-year-olds after t = 0, there is no evidence of an effect on earnings for those who have access to Pre-K (Figure B2a). Furthermore, we show that access to Pre-K in all three district types has no impact on the likelihood of having positive earnings (Figure 3 and Figure B2b).

We then estimate the intent to treat using Equation 3, and we report the results in Table 3. In districts that provide Pre-K for both age groups simultaneously, we aggregate the average treatment effects for individuals who turn three in periods t $\geq$ -1. The results in this table are in line with the evidence discussed above. In districts that provide Pre-K for both age groups simultaneously, we estimate that access to Pre-K increases positive earnings

[12]The event-study graphs for the third type of districts, those who expand provision to three-year-olds after t = 0, are reported in Figure B2.

[13]In districts with Pre-K access to both age groups, the first treated cohort are those who turn three in t = -1. These individuals turn four in t = 0, and thus may still attend Pre-K.

at age 27 by \$1,661, which is a 4 percent increase relative to the control mean. Prior to age 27, we also estimate an increase in earnings by about \$745 to \$1,322.

In contrast, we estimate a null effect on earnings at ages 23 to 26 for individuals who turn four in districts that provide Pre-K for four-year-olds only. In fact, we estimate a decrease in earnings at age 27 for exposed individuals of about 4 percent, which is statistically significant at the 5 percent level. The results for the districts that expand access to three-year-olds after t = 0 are reported in Table B1, and they follow the same pattern as those for districts that provide Pre-K for age four only.[14] For instance, Column (5) of Table B1 show that cohorts that turn four after Pre-K becomes available for four-year-olds experience no changes in earnings at age 27, similar to those who turn four after the expansion. This is true for earnings at ages 23 to 26 as well.

In addition to estimating the effect on positive earnings, we report the effect of access to Pre-K on the likelihood of having positive earnings at each age. Panel B in Table 3 show that in both types of districts, access to Pre-K has no statistically significant effect on the likelihood of having positive earnings at any age. The results are similar for districts that provide Pre-K for age 4 first then expand to age 3 (Panel B of Table B1). This indicates that the increase in positive earnings in districts that provide Pre-K for both age groups simultaneously is not driven by a change in the likelihood of being employed but rather is a reflection of an increase in earnings for those who are employed. As expected, when we estimate the effect on earnings by replacing the missing values with zeros (Panel C in Table 3), the coefficient estimates become smaller but remain in line with our estimates for positive earnings. For instance, even unconditional on observing positive earnings, the results in Panel C show that earnings at age 27 increase by about \$1,000 for individuals who have access to Pre-K at age 3, whereas the estimates for those who turn four in districts that provide Pre-K for age 4 only remain negative but become statistically insignificant.

Taken together, the results indicate that access to Pre-K improves labor market outcomes

[14]For districts that provide Pre-K for four- and three-year-olds sequentially, we estimate the effect of initial access and the effect of expansion, separately.

in adulthood, but only in districts that provide Pre-K to both three- and four-year-olds simultaneously. In these districts, access to Pre-K increases earnings throughout the mid- and late twenties, with gains of about 4 percent at age 27. These effects do not appear to be driven by changes in employment, as we find no evidence that access to Pre-K affects the likelihood of having positive earnings. In contrast, we find little evidence that access to Pre-K improves earnings in districts that primarily serve four-year-olds or in districts that expand access to three-year-olds only after initially serving four-year-olds.

5.2 Effect on secondary and post-secondary educational outcomes

Next, we estimate the effect of access to Pre-K on long-term educational outcomes, including college enrollment, college graduation, and high-school completion. First, using the event study model from Sun and Abraham (2021), we show no evidence of diverging pre-trends for college enrollment and high-school graduation across all district types (Figure 4 and Figure B3).

Second, we show that access to Pre-K has no effect on college enrollment in all districts (Figure 4a, Figure 4c, Figure B3a). We report the estimates from Equation 3 in Panel A of Table 4, where we show that we can reject economically meaningful increases in college enrollment across both district types. For instance, in districts that provide Pre-K for both age groups simultaneously, we can reject increases in the likelihood of college enrollment that are larger than 5%.[15] Similarly, there is no effect on the timing of college enrollment in these districts, nor on the likelihood of college graduation.

The results are similar in districts that provide Pre-K for age 4 only (Panel A of Table 4 and Table B2), except that in districts that provide Pre-K for age 4 only, we estimate an economically small but significant decrease in the likelihood of on time college enrollment. We also estimate the effect of access to Pre-K on enrollment in different types of colleges (e.g., public university and community college), and we see no meaningful changes (Figure A4).

[15]The 95% upper confidence interval for the college enrollment coefficient is 5% relative to the control mean of 0.64 in districts that provide Pre-K for both age groups simultaneously.

While we find no effect on college enrollment and graduation even in districts that provide Pre-K for both age groups simultaneously, we show that in these districts, having access to Pre-K increases the likelihood of high-school graduation and graduating on time, as defined by graduating within 13 years of kindergarten. This is evident in Figure 4b, where the likelihood of high-school graduation increases for all the cohorts that turn three after Pre-K becomes available in their district. We report the coefficient estimates in Panel A of Table 4, where we show that access to Pre-K in districts with access for ages three and four increases the likelihood of high-school graduation (and on time high school graduation) by 3 percentage points. Relative to the outcome mean of 0.725, this suggests that access to Pre-K in these districts increases the likelihood of high school graduation by 4 percent.

This is in contrast to other districts that primarily serve four-year-olds, where we show no effect on high school outcomes (Figure 4d and Figure B3b). The coefficient estimate in Panel A of Table 4 is in line with the event-study graph results, and it is precisely estimated, such that we are able to reject increases in high school graduation that are larger than 3%.[16] Similarly, the results in Table B2 show that access to Pre-K in districts with sequential expansion does not improve the chances of graduating high school. In fact, access to Pre-K in these districts has a negative effect of 2% on the likelihood of high school graduation for cohorts who turn four after Pre-K becomes available for four-year-olds but before the expansion to three-year-olds.

In sum, these results indicate that access to Pre-K in districts that offer it to both age groups simultaneously increases the likelihood of high-school graduation, but these gains do not extend to college enrollment. In contrast, access to Pre-K in districts that make it available for four-year-olds only or provide it sequentially for three-year-olds does not improve long-term educational attainment, including secondary and post-secondary attainment.

Overall, the results on labor market and educational outcomes are closely aligned. In districts that provide Pre-K to both three- and four-year-olds simultaneously, access to Pre-K

[16] The 95% upper confidence interval is 3% relative to the control mean of 0.725.

increases the likelihood of graduating high school and leads to higher earnings in adulthood. The fact that improvements in earnings are accompanied by higher rates of high school completion suggests that Pre-K may improve individuals' attachment to and success within the educational system, with benefits that persist into the labor market. At the same time, we find no evidence of increases in college enrollment or college completion. This pattern indicates that the long-run gains associated with Pre-K in these districts are driven by improvements at the secondary-school margin rather than by increased post-secondary attainment.

The results also reveal substantial heterogeneity in the effectiveness of Pre-K across districts. While districts that provide Pre-K to both age groups simultaneously experience meaningful improvements in long-run outcomes, districts that primarily serve four-year-olds show no evidence of similar gains. These differences suggest that access to Pre-K alone is not sufficient to generate long-run benefits and raise the possibility that variation in district characteristics, program quality, funding, or the availability of alternative early childhood options may play an important role in determining program effectiveness. We explore these potential explanations in section 6.

5.3 Effect on intermediate outcomes

To better understand the mechanisms underlying the long-run effects documented above, we next examine the impact of access to Pre-K on a set of intermediate cognitive and behavioral outcomes. We estimate the effect of access to state-funded Pre-K on third- and fourth-grade math and reading test scores, as well as a range of behavioral outcomes, including disciplinary incidents, criminal incidents, and grade retention.

We report the results in Table 4. In Panel B, the results show that access to Pre-K in districts that serve both age groups simultaneously has a positive and marginally statistically significant effect on fourth grade math test scores. Specifically, access to Pre-K in these districts increases fourth grade math test scores by 23% relative to the control mean. The results also show an increase in fourth grade reading scores, third grade reading and math test scores, even though the coefficients are imprecisely estimated.

We also estimate the effect of access to Pre-K on the likelihood of having a missing test score in third and fourth grade. An individual may have a missing test score if they miss the first attempt for reasons such as being absent, sickness or cheating, or because they left the Texas public school system. Results show that access to Pre-K in districts with access for three- and four-year-olds significantly decreases the likelihood of having a missing third and fourth grade test score by about 17-19 and 21 percent, respectively. In contrast, access to Pre-K in districts with access to age 4 only has no statistically significant effect on the likelihood of having a missed third grade test score and a marginally statistically significant effect of -0.017 on the likelihood of having a missed fourth grade test score. Given that we show no impact of exiting the public school system in Table A2, this pattern is consistent with a decrease in the likelihood of missing the first attempt for reasons such as absenteeism, sickness or cheating. To the extent that the additional students induced to take the assessment are negatively selected—as might be expected if absenteeism is associated with poorer academic performance—our estimates of the effects on test scores are likely conservative.

Consistent with the long-term effects of Pre-K, results show no improvement in third or fourth grade test scores for individuals who turn four in districts that provide Pre-K for four-year-olds only (Panel B of Table 4) or in districts with sequential expansion (Figure B4 and Figure B5). These results are in line with the event study results in Figure 5.[17]

Finally, we find no evidence that access to Pre-K improves long-term outcomes through non-cognitive channels, as shown in Panel C of Table 4. For instance, even in districts that provide Pre-K for both age groups simultaneously, access to Pre-K has no effect on the number of disciplinary incidents after kindergarten. Additionally, access to Pre-K decreases the number of criminal incidents and the number of repeated grades, even though the estimates

[17]The event study graphs for third grade test scores are shown in Figure A6.

are not statistically significant at conventional levels. On the other hand, in districts that provide Pre-K for four-year-olds only, we estimate a 13% increase in the total number of disciplinary incidents but no change in the number of criminal incidents or the number of repeated grades. Lastly, in districts with sequential expansion to three-year-olds, the results suggest access to Pre-K causes a 20 percent decrease in disciplinary incidents and a 22 percent in the number of therapy services received, with no statistically significant effects on other behavioral outcomes, such as grade retention, criminal incidents, or absenteeism (Table B3).

Taken together, the results in this section reinforce the broader pattern of heterogeneity documented throughout the paper. Although we observe improvements in some behavioral outcomes among individuals who turn four in districts that subsequently expand Pre-K access to three-year-olds, the most consistent evidence of positive effects remains concentrated in districts that provide Pre-K to both three- and four-year-olds simultaneously. Consistent with the findings for educational attainment and labor market outcomes, access to Pre-K in these districts improves intermediate cognitive outcomes, including third- and fourth-grade test scores, whereas the effects in districts that primarily serve four-year-olds are generally null or negative.

Notably, the improvements in test scores in the districts where we find gains in high-school graduation and earnings are consistent with a role for cognitive skill development. This pattern differs from evidence on other early childhood interventions, where long-run impacts have often been attributed primarily to improvements in non-cognitive skills (e.g., Gray-Lobe et al. (2023)).

5.4 Robustness checks

We demonstrate the robustness of our results to (1) controlling for observable student characteristics and (2) including school fixed effects.

First, as discussed in subsection 4.2, the introduction of Pre-K may be associated with changes in the demographic composition of students within a district. For example, if families sort across districts in response to Pre-K availability, treated cohorts may differ from untreated cohorts along observable characteristics that are correlated with later outcomes. To address this concern, we control for individual sex, ethnicity, income status, and English proficiency status. The results are reported in Table A3 and Table A4 for districts that provide Pre-K to both age groups simultaneously and districts that provide Pre-K for four-year-olds only, respectively.

The estimates remain stable after including these controls. For example, Column (2) of Table A3 shows that access to Pre-K increases earnings at age 27 by \$1,728, compared to the baseline estimate of \$1,661 reported in Column (1). More generally, the magnitude, sign, and statistical significance of the estimates remain largely unchanged across specifications. This suggests that compositional changes along observable dimensions are unlikely to explain our findings.[18]

Second, it is possible that cohorts exposed to Pre-K differ in the schools they attend or that the timing of Pre-K adoption is correlated with other school-level factors that affect long-run outcomes. To address these concerns, we re-estimate our main model while including school fixed effects. The estimates reported in Column (3) of Table A3 and Table A4 remain similar to the baseline results, indicating that our findings are not driven by differences across schools or changes in the schools attended by students.[19] Overall, the robustness of the estimates across specifications provides additional support for the validity of our identification strategy.

5.5 Heterogeneous effects

Our first stage results in subsection 4.2 show a larger uptake of Pre-K for eligible individuals relative to ineligible ones. Recall, an individual is considered eligible for free state-funded

[18] The results for districts that provide Pre-K for four-year-olds first and later expand access to three-year-olds are reported in Table B4 and show a similar pattern.

[19] The estimates for fourth-grade reading and math test scores become smaller and less precise when school fixed effects are included, although they remain positive.

Pre-K if they are low-income and/or have limited English proficiency. Thus, we estimate the effect of access to Pre-K by eligibility status, reported in Figure A8a and Figure A9a for districts with Pre-K for ages three and four and districts with Pre-K for age four only, respectively. In both figures, we report the effect size relative to the subgroup control mean along with the 95% confidence interval. Across both districts, the results indicate that there are no meaningful differences in the effect of Pre-K access by eligibility status.

Additionally, previous research has shown that participation in Pre-K affects individuals differently across sex (e.g., Gray-Lobe et al. (2023)) and race (e.g., Currie and Thomas (1999)). However, in our setting, We find limited evidence of systematic heterogeneity across demographic groups, as shown in Figure A8 and Figure A9. While Figure A8c shows that Black individuals who have access to Pre-K in districts with simultaneous access for three- and four-year-olds experience a larger decrease in disciplinary incidents relative to Hispanic and White individuals, the majority of the results suggest no meaningful differences across individuals of different ethnicity/race (Figure A8c and Figure A9c) or sex (Figure A8b and Figure A9b).[20]

6 Discussion

The heterogeneity in state-funded Pre-K effectiveness across districts documented above raises an important question: why does access to state-funded Pre-K generate improvements in educational and labor market outcomes in districts that provide Pre-K to both three- and four-year-olds simultaneously, but not in other districts? We consider three non-mutually exclusive explanations. First, children in different districts may face different counterfactual early childhood environments, including varying access to alternative preschool options. Second, the benefits may reflect differences in the timing and duration of exposure to Pre-K, as children in districts that provide Pre-K for both three- and four-year-olds are eligible to

[20]In Figure A7, we show that females and males are equally likely to enroll in Pre-K once it is available, while Black and Hispanic individuals are more likely than White individuals to be enrolled.

enroll at younger ages. Third, differences in program quality, resources, or implementation may contribute to the variation in long-run impacts across districts. In this section, we provide additional results to examine these explanations.

6.1 Counterfactual conditions

One possible explanation for the heterogeneity in effects is that the returns to public Pre-K depend on the counterfactual early childhood environment. If districts that provide Pre-K to both three- and four-year-olds simultaneously offer fewer, or worse quality, alternative preschool options, then the introduction of state-funded Pre-K may substantially expand access to early childhood education, generating larger benefits for children in these districts. In contrast, districts that provide Pre-K for four-year-olds only may have greater access to alternatives such as Head Start or private preschool. In such settings, the introduction of state-funded Pre-K may primarily induce substitution across programs rather than increase overall preschool participation. As a result, reduced-form estimates may be attenuated even if public Pre-K itself is effective. This possibility is particularly relevant in our setting, as we study cohorts attending kindergarten between 1995 and 2001, a period during which alternative preschool options were widely available.

In Table 1, we show that districts that provide Pre-K to both three- and four-year-olds are in fact more likely to have alternative preschool options relative to other treated districts (64% relative to 39%). Recall, a district is classified as having alternative options if at least one Head Start center or private preschool provider is located within its boundaries within the sample period. Yet, it is still possible that the quality of these alternative preschool options varies across districts, in a way that matters for the effect of public Pre-K. To further test that, we examine whether the effects of state-funded Pre-K vary by the availability of alternative early childhood programs. We estimate heterogeneous treatment effects separately for districts that provide Pre-K to both age groups and districts that provide Pre-K for four-year-olds only. For each type of district, we estimate the effect of public Pre-K separately for districts that have alternative options and those that do not. If differences in counterfactual preschool environments are responsible for the heterogeneity across district types, then comparing districts with similar access to alternative preschool options should reduce the differences in estimated treatment effects between districts that provide Pre-K to both age groups and those that primarily serve four-year-olds.

First, Figure A7 shows that, across both types of districts, public Pre-K enrollment is substantially higher in districts without alternative preschool options. These results suggest that public preschool and other programs are at least partially substitutes. For example, among districts that provide Pre-K to both three- and four-year-olds simultaneously, nearly 30% of children enroll in public Pre-K when alternative options are unavailable, compared to fewer than 10% when such options are available. However, the results in Figure A10a and Figure A10b provide little support for this explanation. In both figures, the differences between districts with and without alternative preschool options are not statistically significant. More importantly, conditioning on the availability of alternative preschool programs does not reduce the differences in estimated effects across districts that provide Pre-K for both age groups and others. Moreover, the results in Figure A10b show that access to Pre-K in districts that primarily serve four-year-olds has little effect on long-run outcomes regardless of whether alternative preschool options are available. Taken together, these findings suggest that differences in counterfactual preschool environments are unlikely to explain why the effects of Pre-K are concentrated in districts that provide access to both three- and four-year-olds.[21]

6.2 Intensity or timing of exposure

A second explanation is that impacts depend on the intensity or timing of exposure. The positive effects we observe arise in districts that provide Pre-K to both three- and four-year-

[21] We also examine whether the effect of access to Pre-K depends on peer composition, focusing on low-income and low English proficiency share in districts. However, we find no meaningful differences across districts of different peer quality, as shown in Figure A12.

olds simultaneously, which could reflect either earlier exposure (if children attend at age three) and/or longer exposure (if children participate for two years).

However, this explanation is difficult to reconcile with the data. First, take-up at age three is relatively low even in districts where Pre-K is available for that age group. For example, in districts serving both age groups simultaneously, only 9 percent of children attend Pre-K at both ages. Second, if this limited take-up were sufficient to generate the observed long-run effects, we would expect to see similar improvements in districts that expand access to three-year-olds at a later stage. Instead, we find no measurable effects following these expansions, which suggests that differences in exposure intensity or timing alone are unlikely to account for the heterogeneity in impacts.

6.3 Program quality and resources

Finally, it is possible that Pre-K effectiveness depends on the quality of the program. While we do not observe a measure of quality, we use average Pre-K funding as a proxy for program quality and organizational capacity.

Compared to districts providing Pre-K for four-year-olds only or those that expand sequentially, districts that serve both three- and four-year-olds simultaneously differ systematically in size and resources. As shown in Table 1, these districts are more likely to be urban or suburban (5% relative to 0% in sequential-expansion districts) and are substantially larger, with nearly twice as many kindergarten students in 1995 (83 students versus 40–48 in other districts).

Importantly, serving both age groups at rollout likely requires greater administrative capacity, early childhood infrastructure, and financial resources. While we do not directly observe these dimensions, differences in per-child spending suggest that such districts operate with meaningfully greater resources. As shown in Table 1, districts serving both age groups spend $13,201 per child, compared to $481–$1,510 in other district types.

To further test this hypothesis, we examine whether the effectiveness of state-funded

Pre-K varies with district resources by estimating heterogeneous treatment effects across districts with above- and below-mean Pre-K funding of about $3,100 per child between 1995-2000. The results are shown in Figure A11a and Figure A11b for districts that provide Pre-K to both three- and four-year-olds simultaneously and districts that provide Pre-K to four-year-olds only, respectively.

In districts that provide Pre-K to both age groups, we find suggestive evidence that the benefits of access to Pre-K are larger in districts with higher levels of Pre-K funding. As shown in Figure A11a, the positive effects on earnings, high school graduation, and fourth-grade test scores are larger and statistically significant only in higher-funded districts. This pattern is consistent with the idea that additional resources may improve program quality or implementation and, in turn, increase the effectiveness of Pre-K. Moreover, the corresponding estimates for lower-funded districts are smaller and statistically indistinguishable from zero. However, results in Figure A11b indicate that among districts that provide Pre-K for four-year-olds only, there is little evidence of improvements in educational or labor market outcomes even in high-funding districts, which may suggest that there are other factors, besides funding levels, that contribute to the effectiveness of the program.

At the same time, the results in Figure A11a are consistent with evidence from other settings showing that greater preschool resources are associated with better outcomes. To put this into perspective, we compare the Texas Pre-K program to programs in other states (Table A5). While these programs differ along multiple dimensions, the comparison is suggestive of a broader relationship between program resources and effectiveness. One potential channel through which resources translate into improved outcomes is class size. For example, Gormley Jr et al. (2005) indicates that class sizes in Boston's Pre-K program range from 9 to 25, with an average around 19 students, compared to approximately 22 students in Texas. Although this difference is modest, it is consistent with the idea that higher funding may enable more intensive or higher quality instruction. Consistent with this hypothesis, Watts et al. (2023) shows that higher Pre-K funding is associated with better academic achievement. More generally, Jackson and Mackevicius (2024) show that increasing public school spending has meaningful impacts on educational outcomes and college enrollment.

7 Conclusion

This paper studies the long-run effects of access to a large-scale, contemporary state-funded Pre-K program using rich administrative data from Texas that tracks individual outcomes from prekindergarten until the late twenties. Exploiting variation in program availability across districts and cohorts generated by the gradual rollout of Pre-K, we find that access substantially increases enrollment in state-funded preschool. The first-stage effects are particularly large among children who are eligible for free Pre-K and in districts where alternative preschool options are limited, indicating that state-funded Pre-K meaningfully expands participation in early childhood education.

Additional results show that in districts that provide Pre-K to both three- and four-year-olds simultaneously, access to Pre-K increases the likelihood of high-school graduation and raises earnings throughout the mid- to late-twenties. By examining intermediate term outcomes such as test scores, disciplinary incidents, and other behavioral outcomes, we show that these effects are driven by cognitive skill improvements, as access to Pre-K increases fourth grade math and reading test scores. In contrast, we find little evidence that access to Pre-K improves long-run labor market and educational outcomes in districts that primarily serve four-year-olds or in districts that expand access to three-year-olds after initially serving four-year-olds only.

Importantly, we examine several potential explanations to understand these differences. The evidence provides little support for differences in counterfactual preschool environments across district types. Even after conditioning on alternative preschool availability, differences in the effects across districts that provide Pre-K for both age groups simultaneously and other districts persist. Additionally, longer exposure or exposure duration cannot explain the results, as enrollment for three-year-olds remain less than 10% in districts that serve both age groups.

Instead, the patterns are most consistent with differences in resources and implementation capacity. Districts generating positive long-run effects tend to be larger, more urban, and substantially better funded, and within these districts, higher levels of Pre-K funding are associated with stronger educational and labor market gains. While these relationships should not be interpreted causally, they suggest that the effectiveness of large-scale Pre-K programs depends not only on expanding access but also on the resources available to support implementation.

Taken together, our findings contribute to a growing literature that seeks to understand the conditions under which early childhood education programs generate lasting benefits. While much of the evidence from earlier interventions such as Perry Preschool, Abecedarian, and Head Start points to substantial long-run returns, evidence from modern state-funded Pre-K programs has been considerably more mixed. Our results suggest that this variation may reflect differences in resources, and potentially program quality, rather than differences in access alone. As states continue to expand investments in early childhood education, understanding how program quality, funding, and institutional capacity shape long-run outcomes may be as important as expanding enrollment itself.

References

(2025). Texas education code § 29.153: Free prekindergarten for certain children. Accessed: 2026-02-02.

Anders, J., Barr, A. C., and Smith, A. A. (2023). The effect of early childhood education on adult criminality: Evidence from the 1960s through 1990s. *American Economic Journal: Economic Policy*, 15(1):37–69.

Bailey, M. J., Sun, S., and Timpe, B. (2021). Prep school for poor kids: The long-run impacts of head start on human capital and economic self-sufficiency. *American Economic Review*, 111(12):3963–4001.

Barnett, W. S. and Masse, L. N. (2007). Comparative benefit–cost analysis of the abecedarian program and its policy implications. *Economics of education review*, 26(1):113–125.

Barr, A. and Gibbs, C. R. (2022). Breaking the cycle? intergenerational effects of an antipoverty program in early childhood. *Journal of Political Economy*, 130(12):3253–3285.

Borusyak, K. and Jaravel, X. (2018). *Revisiting event study designs*. SSRN.

Campbell, F. A. and Ramey, C. T. (1994). Effects of early intervention on intellectual and academic achievement: a follow-up study of children from low-income families. *Child development*, 65(2):684–698.

Cascio, E. U. (2023). Does universal preschool hit the target?: Program access and preschool impacts. *Journal of Human Resources*, 58(1):1–42.

Chetty, R., Friedman, J. N., Hilger, N., Saez, E., Schanzenbach, D. W., and Yagan, D. (2011). How does your kindergarten classroom affect your earnings? evidence from project star. *The Quarterly journal of economics*, 126(4):1593–1660.

Currie, J. (2001). Early childhood education programs. *Journal of Economic perspectives*, 15(2):213–238.

Currie, J. and Thomas, D. (1999). Does head start help hispanic children? *Journal of public Economics*, 74(2):235–262.

De Chaisemartin, C. and d'Haultfoeuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American economic review*, 110(9):2964–2996.

Deming, D. (2009). Early childhood intervention and life-cycle skill development: Evidence from head start. *American Economic Journal: Applied Economics*, 1(3):111–134.

Duncan, G. J. and Magnuson, K. (2013). Investing in preschool programs. *Journal of economic perspectives*, 27(2):109–132.

Durkin, K., Lipsey, M. W., Farran, D. C., and Wiesen, S. E. (2022). Effects of a statewide pre-kindergarten program on children's achievement and behavior through sixth grade. *Developmental Psychology*, 58(3):470.

Fitzpatrick, M. D. (2008). Starting school at four: The effect of universal pre-kindergarten on children's academic achievement. *The BE Journal of Economic Analysis & Policy*, 8(1).

Garces, E., Thomas, D., and Currie, J. (2002). Longer-term effects of head start. *American economic review*, 92(4):999–1012.

García, J. L., Heckman, J. J., and Ronda, V. (2023). The lasting effects of early-childhood education on promoting the skills and social mobility of disadvantaged african americans and their children. *Journal of Political Economy*, 131(6):1477–1506.

Gibbs, C. R. (2026). Early learning across decades: Advances in measuring head start effectiveness. *Journal of Economic Literature*.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of econometrics*, 225(2):254–277.

Gormley Jr, W. T., Gayer, T., Phillips, D., and Dawson, B. (2005). The effects of universal pre-k on cognitive development. *Developmental psychology*, 41(6):872.

Gray-Lobe, G., Pathak, P. A., and Walters, C. R. (2023). The long-term effects of universal preschool in boston. *The Quarterly Journal of Economics*, 138(1):363–411.

Heckman, J., Moon, S. H., Pinto, R., Savelyev, P., and Yavitz, A. (2010a). Analyzing social experiments as implemented: A reexamination of the evidence from the highscope perry preschool program. *Quantitative economics*, 1(1):1–46.

Heckman, J. J., Moon, S. H., Pinto, R., Savelyev, P. A., and Yavitz, A. (2010b). The rate of return to the highscope perry preschool program. *Journal of public Economics*, 94(1-2):114–128.

Hipple, S. F. and Hammond, L. A. (2016). Bls spotlight on statistics: Self-employment in the united states.

HISD, Department of Research and Accountability (2017). Effects of hisd prekindergarten programs on kindergarten students' academic achievement, 2016–2017. Research Report ED596504, Houston Independent School District.

Internal Revenue Service (2022). SOI Tax Stats - Migration Data 2021–2022 - State-to-State Outflow [dataset]. Accessed via Internal Revenue Service [distributor], January 21, 2025.

Jackson, C. K. and Mackevicius, C. L. (2024). What impacts can we expect from school spending policy? evidence from evaluations in the united states. *American Economic Journal: Applied Economics*, 16(1):412–446.

Johnson, R. C. and Jackson, C. K. (2019). Reducing inequality through dynamic complementarity: Evidence from head start and public school spending. *American Economic Journal: Economic Policy*, 11(4):310–349.

Kline, P. and Walters, C. R. (2016). Evaluating public programs with close substitutes: The case of head start. *The Quarterly Journal of Economics*, 131(4):1795–1848.

Ludwig, J. and Miller, D. L. (2007). Does head start improve children's life chances? evidence from a regression discontinuity design. *The Quarterly journal of economics*, 122(1):159–208.

National Center for Education Statistics (2000). Private school universe survey (pss).

NIEER (2012). Texas public school prekindergarten state profile. PDF report. Part of *The State of Preschool 2012: State Preschool Yearbook*; Accessed: 2026-02-02.

NIEER (2024). Texas public school prekindergarten state profile. PDF report. Accessed: 2026-02-02; part of The State of Preschool 2024: State Preschool Yearbook, National Institute for Early Education Research.

NIEER (2025a). Executive summary, *The State of Preschool 2024 Yearbook*. Technical report, Rutgers University.

NIEER (2025b). Texas: State preschool yearbook profile, 2023–2024. State profile report.

Nores, M., Belfield, C. R., Barnett, W. S., and Schweinhart, L. (2005). Updating the economic impacts of the high/scope perry preschool program. *Educational Evaluation and Policy Analysis*, 27(3):245–261.

of Policy Planning, T. D. and Evaluation (1992). Texas evaluation study of prekindergarten programs: Preliminary findings. ERIC Document ED363405, Texas Education Agency. Accessed: 2026-02-02.

Office of Head Start (2026). Program information report (pir).

Pages, R., Lukes, D. J., Bailey, D. H., and Duncan, G. J. (2020). Elusive longer-run impacts of head start: Replications within and across cohorts. *Educational Evaluation and Policy Analysis*, 42(4):471–492.

Ramey, C. T. and Campbell, F. A. (1984). Preventive education for high-risk children: cognitive consequences of the carolina abecedarian project. *American journal of mental deficiency*, 88(5):515–523.

Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of econometrics*, 225(2):175–199.

Texas Education Agency (n.d.). Prekindergarten tuition. Accessed: 2026-02-02.

US Census Bureau (2023). American Community Survey (ACS): Public Use Microdata Sample (PUMS), 2019–2023. Distributed by Inter-university Consortium for Political and Social Research, Ann Arbor, MI. Accessed January 21, 2025.

Watts, T. W., Jenkins, J. M., Dodge, K. A., Carr, R. C., Sauval, M., Bai, Y., Escueta, M., Duer, J., Ladd, H., Muschkin, C., et al. (2023). Understanding heterogeneity in the impact of public preschool programs. *Monographs of the Society for Research in Child Development*, 88(1):7–182.

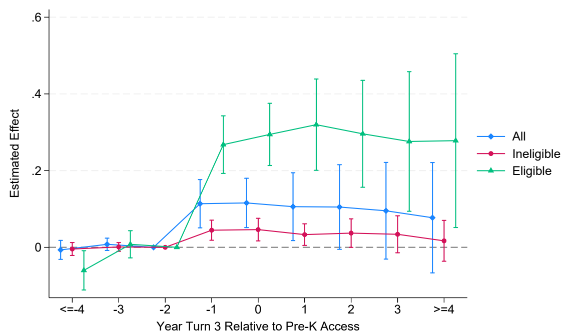
Whitaker, A. A., Burchinal, M., Jenkins, J. M., Bailey, D. H., Watts, T. W., Duncan, G. J., Hart, E. R., and Peisner-Feinberg, E. (2026). Why are preschool programs becoming less effective? *Journal of Policy Analysis and Management*, 45(1):e70031.

Wong, V. C., Cook, T. D., Barnett, W. S., and Jung, K. (2008). An effectiveness-based evaluation of five state pre-kindergarten programs. *Journal of Policy Analysis and Management: The Journal of the Association for Public Policy Analysis and Management*, 27(1):122–154.

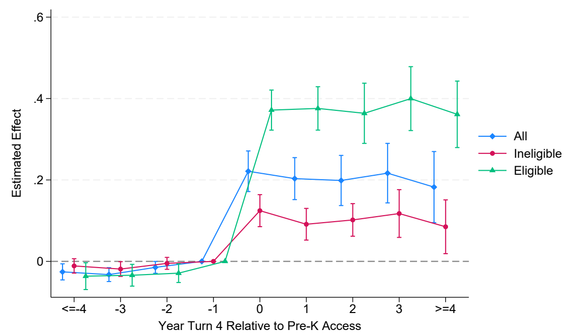
8 Figures and Tables

Figure 1: The Effect of Access to Pre-K on Pre-K Enrollment

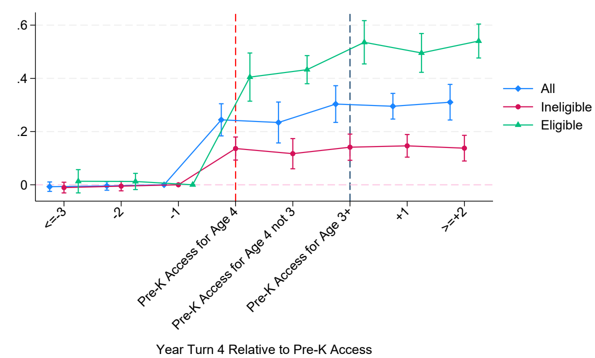
(a) Districts with Pre-K Access for Age 3+



(b) Districts with Pre-K Access for Age 4



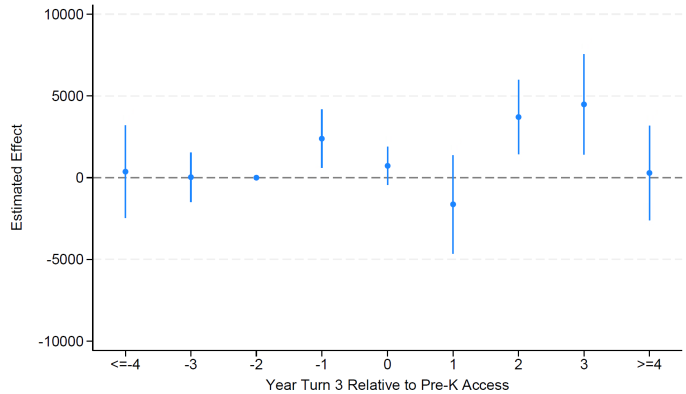
(c) Districts with Pre-K Access for Age 4 then 3+



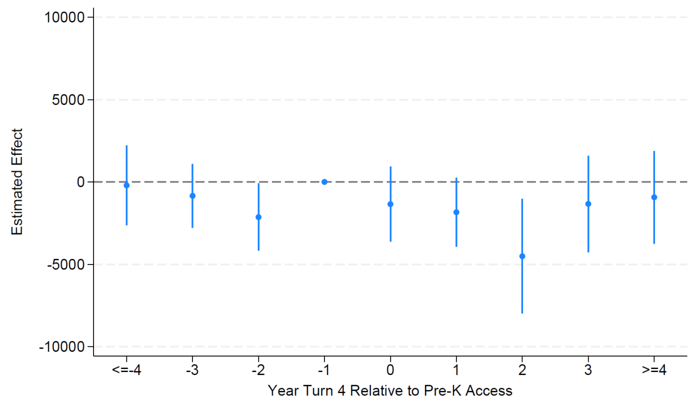
Notes: These figures show the effect of access to Pre-K on Pre-K enrollment, by district type, using Sun and Abraham (2021). Sub-figure (a) shows the effect of access to Pre-K in districts that provide Pre-K for three- and four-year-olds simultaneously, sub-figure (b) shows the effect of access to Pre-K in districts that provide Pre-K for four-year-olds only, and sub-figure (c) shows the effect of access to Pre-K in districts where Pre-K is available for age four first then age three. In districts with Pre-K access for age 3+, treatment is defined based on when an individual turns three relative to when Pre-K becomes available in their district. The omitted category is t = -2. In all other districts, treatment is defined based on when an individual turns 4 relative to when Pre-K becomes available, and the omitted category is t = -1. In each figure, we report the coefficient estimates in addition to the 95% confidence intervals. Standard errors are clustered at the school district level.

Figure 2: The Effect of Access to Pre-K on Positive Earnings at Age 27

(a) Districts with Pre-K Access for Age 3+



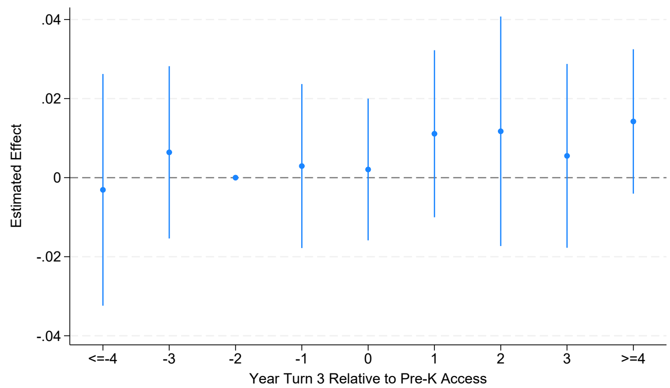
(b) Districts with Pre-K Access for Age 4



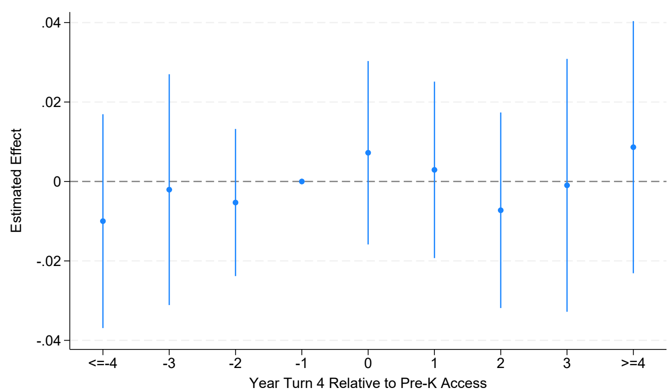
Notes: These figures show the effect of access to Pre-K on earnings, conditional on having non-zero earnings, at age 27, by treatment type, using Sun and Abraham (2021). Sub-figure (a) shows the effect of access to Pre-K in districts that provide Pre-K for three- and four-year-olds simultaneously, and sub-figure (b) shows the effect of access to Pre-K in districts that provide Pre-K for four-year-olds only. In districts with Pre-K access for age 3+, treatment is defined based on when an individual turns three relative to when Pre-K becomes available in their district. The omitted category is t = -2. In districts with Pre-K access for age 4 only, treatment is defined based on when an individual turns 4 relative to when Pre-K becomes available in their district, and the omitted category is t = -1. The results for districts where Pre-K is available for age four first then age three are reported in Figure B2. In each figure, we report the coefficient estimates along with 95% confidence intervals. Standard errors are clustered at the school district level.

Figure 3: The Effect of Access to Pre-K on Having Positive Earnings at Age 27

(a) Districts with Pre-K Access for Age 3+



(b) Districts with Pre-K Access for Age 4

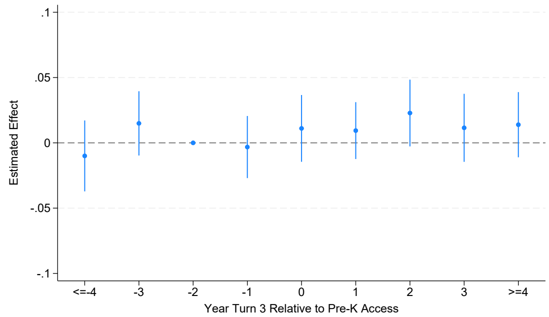


Notes: These figures show the effect of access to Pre-K on the likelihood of having non-zero earnings at age 27, by treatment type, using Sun and Abraham (2021). Sub-figure (a) shows the effect of access to Pre-K in districts that provide Pre-K for three- and four-year-olds simultaneously, and sub-figure (b) shows the effect of access to Pre-K in districts that provide Pre-K for four-year-olds only. In districts with Pre-K access for age 3+, treatment is defined based on when an individual turns three relative to when Pre-K becomes available in their district. The omitted category is t = -2. In districts with Pre-K access for age 4 only, treatment is defined based on when an individual turns 4 relative to when Pre-K becomes available in their district, and the omitted category is t = -1. The results for districts where Pre-K is available for age four first then age three are reported in Figure B2. In each figure, we report the coefficient estimates along with 95% confidence intervals. Standard errors are clustered at the school district level.

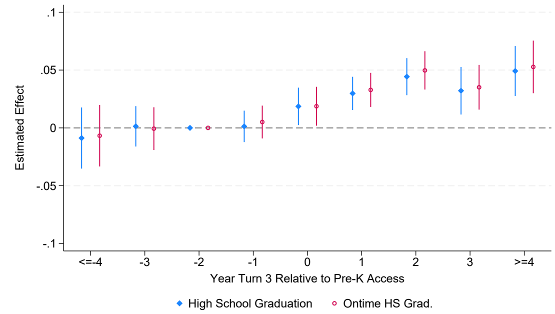
Figure 4: The Effect of Access to Pre-K on College Enrollment and High School Graduation

Districts with Pre-K Access for Age 3+

(a) College Enrollment

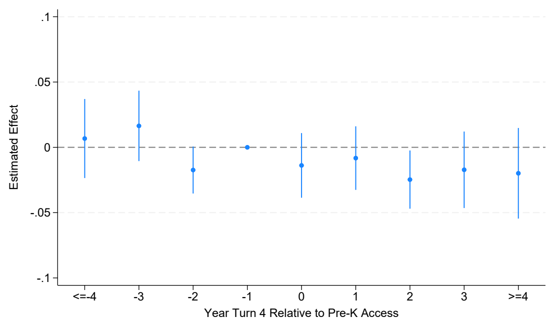


(b) High School Graduation

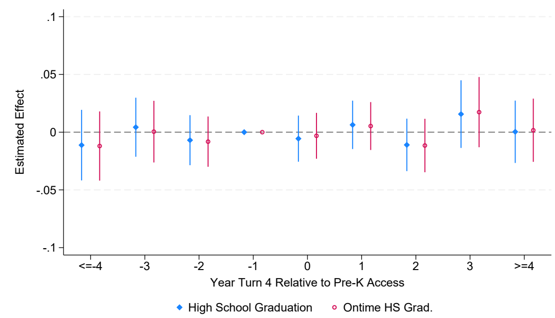


Districts with Pre-K Access for Age 4

(c) College Enrollment



(d) High School Graduation

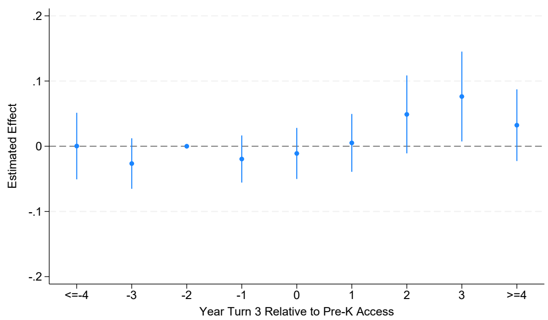


Notes: These figures show the effect of access to Pre-K on college enrollment and high school graduation, by treatment type, using Sun and Abraham (2021). College enrollment is equal to 1 if an individual ever enrolls in any post-secondary institution, including a Texas public university, private university, community college, health institute, career school, or out-of-state university. High school graduation refers to graduating from a Texas public school. On-time high school graduation is defined as graduating from high school within 13 years after kindergarten. The first row shows the effect of access to Pre-K in districts that provide Pre-K for ages 3 and 4 simultaneously, while the second row shows the effect of access to Pre-K in districts that provide Pre-K for age 4 only. In districts with Pre-K access for age 3+, treatment is defined based on when an individual turns three relative to when Pre-K becomes available in their district. The omitted category is t = -2. In districts with Pre-K access for age 4 only, treatment is defined based on when an individual turns 4 relative to when Pre-K becomes available in their district, and the omitted category is t = -1. The results for districts where Pre-K is available for age four first then age three are reported in Figure B3. In each figure, we report the coefficient estimates along with 95% confidence intervals. Standard errors are clustered at the school district level.

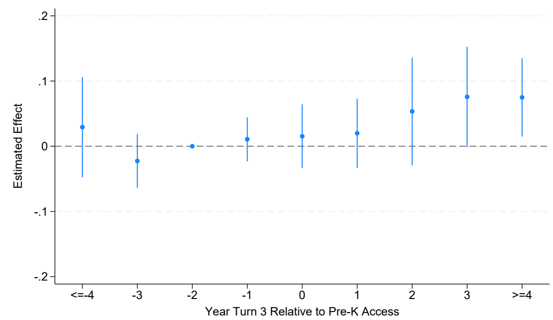
Figure 5: The Effect of Access to Pre-K on Fourth Grade Test Scores

Districts with Pre-K Access for Age 3+

(a) Reading

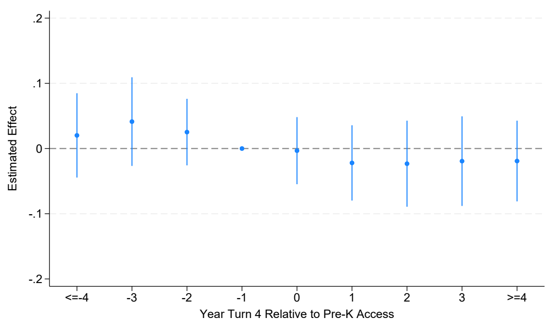


(b) Math

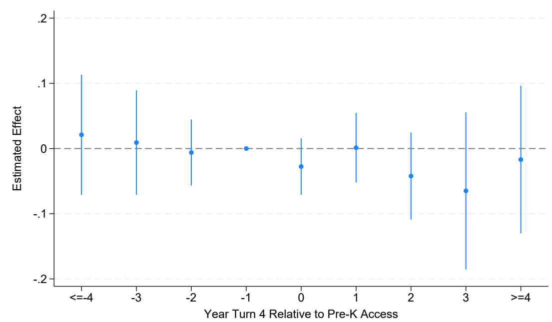


Districts with Pre-K Access for Age 4

(c) Reading



(d) Math



Notes: These figures show the effect of access to Pre-K on fourth grade test scores, by treatment type, using Sun and Abraham (2021). Test scores are standardized by cohort-subject such that the mean is zero and the standard deviation is one. The first row shows the effect of access to Pre-K in districts that provide Pre-K for ages 3 and 4 simultaneously, while the second row shows the effect of access to Pre-K in districts that provide Pre-K for age 4 only. In districts with Pre-K access for age 3+, treatment is defined based on when an individual turns three relative to when Pre-K becomes available in their district. The omitted category is t = -2. In districts with Pre-K access for age 4 only, treatment is defined based on when an individual turns 4 relative to when Pre-K becomes available in their district, and the omitted category is t = -1. The results for districts where Pre-K is available for age four first then age three are reported in Figure B4. In each figure, we report the coefficient estimates along with 95% confidence intervals. Standard errors are clustered at the school district level.

Table 1: Summary Statistics

	Control	Districts with Pre-K: For 3+	For 4 only	For 4 then 3+
	(1)	(2)	(3)	(4)
District Characteristics				
Rural	0.64	0.54	0.66	0.65
Urban or Suburban	0.02	0.04	0.03	0.00
Enrollment in KG in 1995	35.66	78.85	39.79	47.26
Other Preschool Options Available	0.41	0.64	0.39	0.42
Per Child Pre-K Funding ($)	-	13201.44	1509.58	481.69
Per Student Funding in 1995 ($)	19605.29	18578.59	17472.15	18576.84
Number of Districts	195.00	87.00	118.00	31.00
Student Characteristics				
Male	0.506	0.501	0.507	0.505
White	0.798	0.733	0.766	0.820
Hispanic	0.140	0.174	0.146	0.118
Black	0.051	0.069	0.065	0.052
Economically Disadvantaged	0.304	0.237	0.352	0.371
Special Education Participation	0.063	0.055	0.066	0.084
Limited English Proficiency	0.034	0.105	0.055	0.045
Educational Outcomes				
Grade 3 Reading Score	0.202	0.201	0.139	0.150
Grade 3 Math Score	0.200	0.217	0.134	0.137
Grade 4 Reading Score	0.203	0.205	0.154	0.162
Grade 4 Math Scores	0.182	0.199	0.131	0.116
High School Graduation	0.725	0.701	0.723	0.710
On-Time High School Graduation	0.719	0.693	0.717	0.702
Any College Enrollment	0.640	0.618	0.623	0.602
Community College Enrollment	0.556	0.524	0.544	0.542
Public University Enrollment	0.293	0.302	0.289	0.264
Any College Graduation	0.359	0.349	0.344	0.319
Behavioral Outcomes				
No. Disciplinary Incidents	3.185	3.132	3.076	3.710
No. Crime	0.108	0.099	0.092	0.102
No. Grade Retention	0.232	0.263	0.250	0.273
Days Absent from School	73.583	66.973	72.088	78.645
Years of Special Education	0.080	0.073	0.124	0.110
Years of Disability	1.221	1.141	1.272	1.302
No. Therapy Services Received	0.540	0.555	0.552	0.603
Earnings Outcomes				
Positive Earnings Age 25 ($10,000)	3.629	3.633	3.583	3.640
Positive Earnings Age 26 ($10,000)	4.057	4.081	3.979	4.041
Positive Earnings Age 27 ($10,000)	4.583	4.575	4.407	4.402
Have Positive Earnings Age 25	0.637	0.601	0.642	0.635
Have Positive Earnings Age 26	0.630	0.593	0.636	0.626
Have Positive Earnings Age 27	0.620	0.584	0.629	0.618
Annual Earnings Age 25 ($10,000)	2.313	2.182	2.300	2.311
Annual Earnings Age 26 ($10,000)	2.557	2.420	2.531	2.530
Annual Earnings Age 27 ($10,000)	2.843	2.673	2.772	2.718
Observations	43,576	43,632	33,654	11,431

Notes: This table shows the means for district characteristics, individual characteristics, and outcomes of interest by district type. Column (1) shows averages for individuals in the control districts, i.e., those that never provide Pre-K during our sample period. Columns (2), (3), and (4) show the averages for individuals in districts that provide Pre-K for ages 3 and 4 simultaneously, for age 4 only, and for age 4 then age 3, respectively. Student characteristics and outcomes are reported for cohorts attending kindergarten between 1994–2001. Enrollment in KG is based on 1995 enrollment. Per-child Pre-K funding is calculated using funding data from 1995 to 2000 across districts. Per-child total funding is average district spending per student in K-12.

Table 2: The Effect of Access to Pre-K on Pre-K Enrollment

		By Eligibility Status	
	All	Ineligible	Eligible
	(1)	(2)	(3)
Panel A: Districts with Pre-K Access for 3+			
	0.102**	0.035**	0.289***
	(0.050)	(0.017)	(0.067)
Observations	87207	62168	25032
Panel B: Districts with Pre-K Access for 4			
	0.205***	0.104***	0.375***
	(0.031)	(0.022)	(0.030)
Observations	77229	51497	25724
Panel C: Districts with Pre-K Access for 4 then 3+			
Panel C1: Access for 4			
	0.239***	0.127***	0.419***
	(0.033)	(0.022)	(0.033)
Panel C2: Access for 3+			
	0.303***	0.142***	0.524***
	(0.027)	(0.020)	(0.029)
Observations	55006	37258	17741

Notes: This table shows the effect of access to Pre-K on Pre-K enrollment by treatment type using Equation 3. In column (1), we estimate the effect of enrollment for the entire sample. In columns (2) and (3), we estimate the effect of access to Pre-K on enrollment by eligibility for free Pre-K status. An individual is eligible for free Pre-K if they are economically disadvantaged or have limited English proficiency in KG, and ineligible otherwise. Standard errors are clustered at the school district level and are reported in parentheses.* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3: The Effect of Access to Pre-K on Earnings

	Control Mean	**Districts with Access to Pre-K for Age 3+**		Districts with Access to Pre-K for Age 4	
		N	Effect	N	Effect
Panel A: Positive Earnings (excluding 0s)					
Age 23	27,910	54,951	745.194*	50,338	-429.552
			(426.243)		(707.953)
Age 24	31,960	54,383	1,321.899**	49,839	-699.941
			(537.575)		(712.605)
Age 25	36,290	53,967	-1,280.787	49,367	-206.652
			(3,694.723)		(772.449)
Age 26	40,570	53,344	1,027.279**	48,874	-681.716
			(522.235)		(853.090)
Age 27	45,830	52,519	1,661.459**	48,194	-1,993.185**
			(661.061)		(955.057)
Panel B: Have Positive Earnings					
Age 23	0.647	87,207	0.002	77,229	0.006
			(0.007)		(0.011)
Age 24	0.643	87,207	0.008	77,229	0.009
			(0.007)		(0.011)
Age 25	0.637	87,207	-0.002	77,229	0.000
			(0.007)		(0.011)
Age 26	0.630	87,207	0.010	77,229	0.002
			(0.007)		(0.009)
Age 27	0.620	87,207	0.008	77,229	0.002
			(0.008)		(0.011)
Panel C: Earnings (including 0s)					
Age 23	18,050	87,207	378.522	77,229	-59.411
			(271.355)		(618.641)
Age 24	20,540	87,207	918.596**	77,229	-108.094
			(368.806)		(610.068)
Age 25	23,130	87,207	-1,030.074	77,229	-72.572
			(2,310.373)		(613.946)
Age 26	25,570	87,207	671.054	77,229	-281.604
			(434.960)		(677.012)
Age 27	28,430	87,207	1,005.026**	77,229	-964.451
			(476.119)		(826.374)

Notes: This table shows the effect of access to Pre-K on labor market outcomes using Equation 3. Panel A reports the effect of access to Pre-K on positive earnings (conditional on having non-zero earnings) for ages 23-27, while Panels B and C report the effect of access to Pre-K on the likelihood of having positive earnings and earnings (including zeros) for ages 23-27, respectively. The control mean refers to average outcomes for individuals in districts that never provide Pre-K in our sample period. We report the effects separately for districts that provide Pre-K for ages three and four simultaneously and for districts that provide Pre-K for age four only. Standard errors clustered at the school district level are reported in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

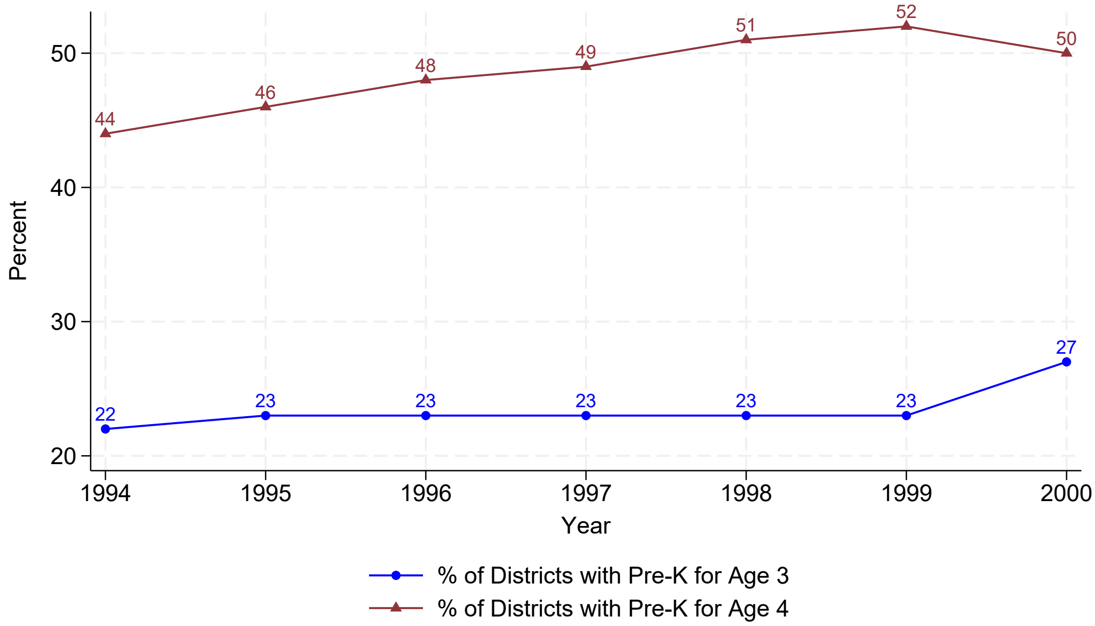
Table 4: The Effect of Access to Pre-K on Educational and Behavioral Outcomes

		Districts with Access to Pre-K for Age 3+		Districts with Access to Pre-K for Age 4	
	Control Mean	N	Effect	N	Effect
Panel A: Long-term Educational Outcomes					
High School Graduation	0.725	87,207	0.029***	77,229	0.001
			(0.006)		(0.009)
On-Time High School Graduation	0.719	87,207	0.032***	77,229	0.002
			(0.006)		(0.009)
Any College Enrollment	0.640	87,207	0.011	77,229	-0.017
			(0.010)		(0.011)
On-Time College Enrollment	0.564	87,207	0.010	77,229	-0.020**
			(0.009)		(0.009)
Any College Graduation	0.359	87,207	0.003	77,229	-0.012
			(0.008)		(0.011)
Panel B: Intermediate Educational Outcomes					
Grade 3 Reading	0.202	74,546	0.025	67,625	-0.027
			(0.021)		(0.021)
Grade 3 Math	0.200	73,082	0.023	66,263	-0.037
			(0.021)		(0.029)
Missing Grade 3 Reading	0.123	87,207	-0.021*	77,229	-0.013
			(0.011)		(0.009)
Missing Grade 3 Math	0.141	87,207	-0.027**	77,229	-0.013
			(0.013)		(0.011)
Grade 4 Reading	0.203	72,124	0.022	65,370	-0.017
			(0.020)		(0.021)
Grade 4 Math	0.182	72,124	0.042*	65,370	-0.030
			(0.023)		(0.031)
Missing Grade 4 Reading	0.153	87,207	-0.032***	77,229	-0.017*
			(0.012)		(0.009)
Missing Grade 4 Math	0.153	87,207	-0.032***	77,229	-0.017*
			(0.012)		(0.009)
Panel C: Behavioral Outcomes					
Disciplinary Incidents	3.185	87,207	0.074	77,229	0.441**
			(0.134)		(0.182)
Crime Incidents	0.108	87,207	-0.013	77,229	0.001
			(0.014)		(0.008)
Grade Retention	0.232	87,207	-0.026	77,229	0.018
			(0.017)		(0.014)

Notes: This table shows the effect of access to Pre-K on educational and behavioral outcomes using Equation 3. Panel A reports the effect of access to Pre-K on long-term educational outcomes, Panel B reports the effect on intermediate educational outcomes, and Panel C reports the effect on behavioral outcomes. An individual is considered to have graduated from high school if they have ever graduated from a public high school in Texas. On-time high school graduation is defined as graduating from a Texas public high school within 13 years of KG. On-time college enrollment is defined as enrollment within 13 years of KG. Third and fourth-grade test scores are normalized by cohort-subject such that the mean is zero and the standard deviation is one. A test score is missing if an individual does not take the test. Panel C shows the effect on the number of any disciplinary incidents within 13 years post Pre-K and criminal incidents between 5 and 12 years after KG. Grade retention refers to the number of repeated grades after Pre-K. The control mean is the average for individuals in districts that never provided Pre-K in our sample period. We report the effects separately for districts that provide Pre-K for ages three and four simultaneously and for districts that provide Pre-K for age four only. Standard errors clustered at the school district level are reported in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Appendix A: Additional Figures and Tables

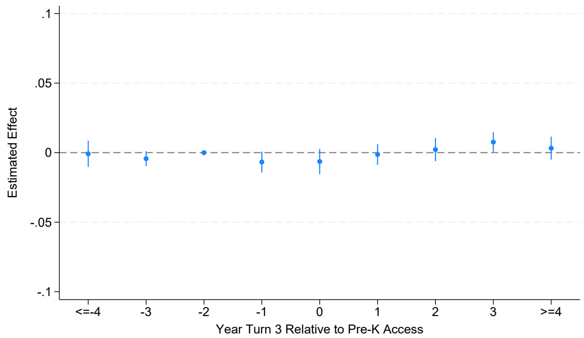
Figure A1: Pre-K Availability in School Districts by Year



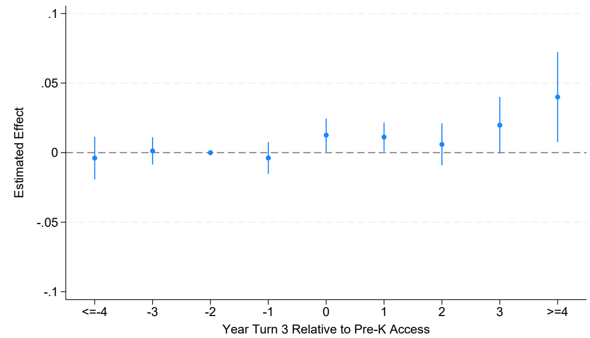
Notes: This figure shows the share of districts offering Pre-K to 3-year-olds and 4-year-olds separately over time.

Figure A2: The Correlation between Access to Pre-K and Individual Characteristics in Districts with Pre-K Access for Age 3+

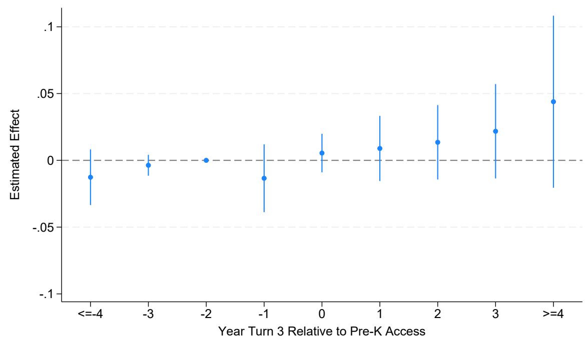
(a) Black



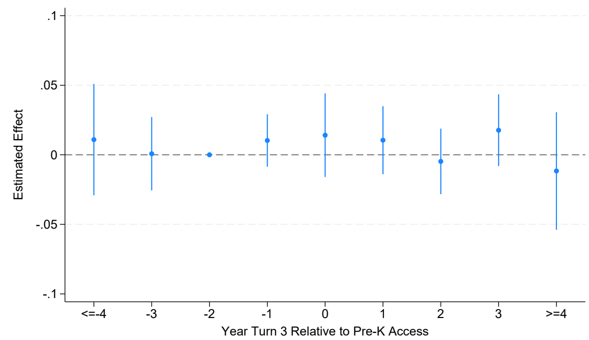
(b) Hispanic



(c) Limited English Proficiency



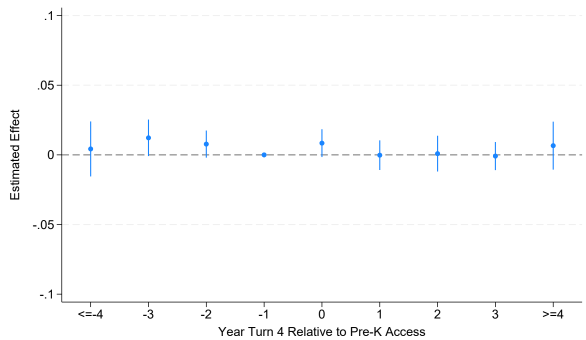
(d) Low Income



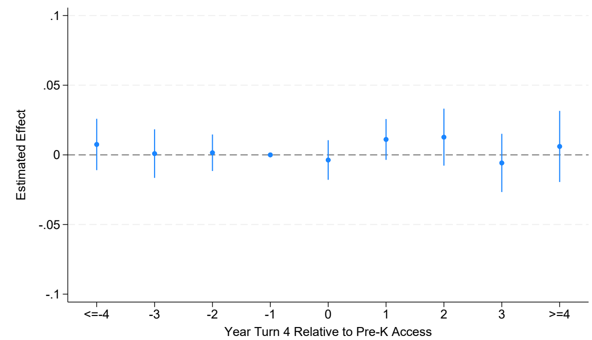
Notes: These figures show the correlation between access to Pre-K and observable individual characteristics by estimating NIEER (2025a) using each observable characteristic as the dependent variable. Each sub-figure reports the coefficient estimates and the 95% confidence intervals for the indicated characteristic. Standard errors are clustered at the school district level.

Figure A3: The Correlation between Access to Pre-K and Individual Characteristics in Districts with Pre-K Access for Age 4

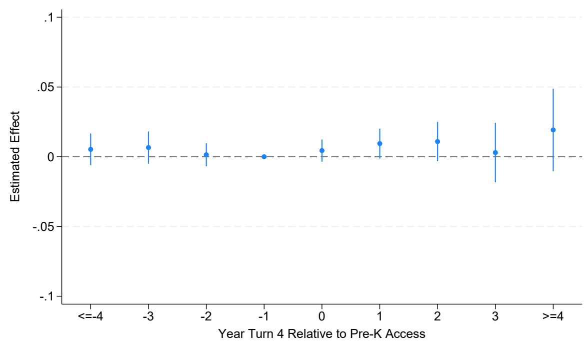
(a) Black



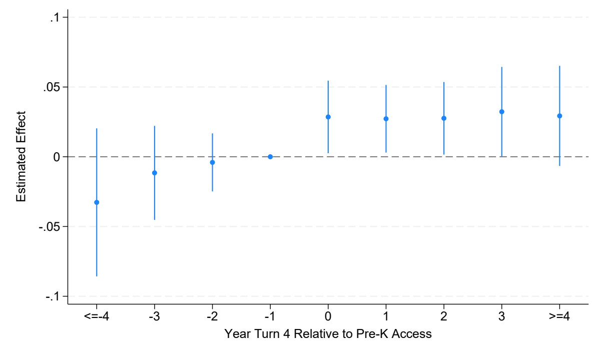
(b) Hispanic



(c) Limited English Proficiency



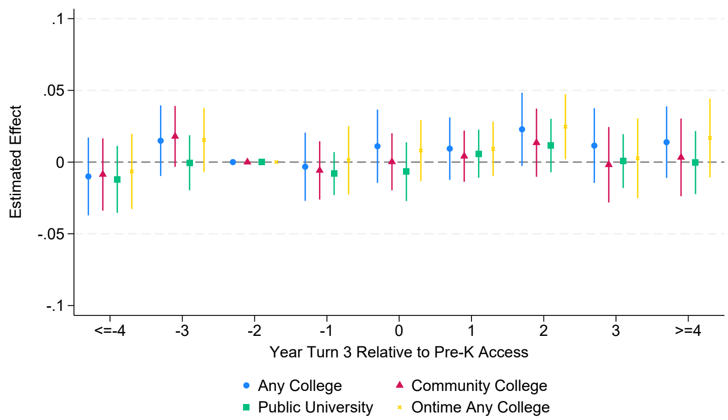
(d) Low Income



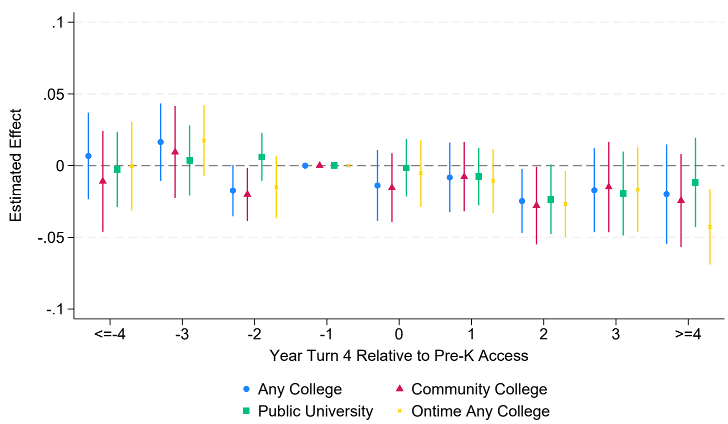
Notes: These figures show the correlation between access to Pre-K and observable individual characteristics by estimating Sun and Abraham (2021) using each observable characteristic as the dependent variable. Each sub-figure reports the coefficient estimates and the 95% confidence intervals for the indicated characteristic. Standard errors are clustered at the school district level.

Figure A4: The Effect of Access to Pre-K on College Enrollment Outcomes

(a) Districts with Pre-K Access for Age 3+



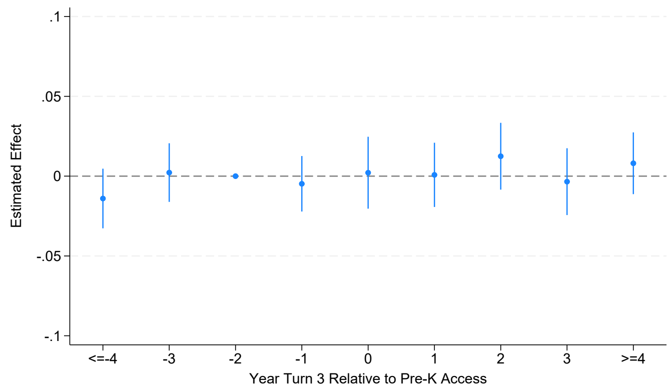
(b) Districts with Pre-K Access for Age 4



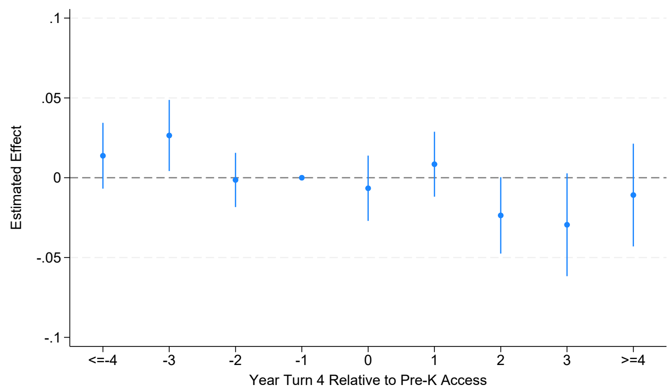
Notes: These figures show the effect of access to Pre-K on college enrollment outcomes, by treatment type, using Sun and Abraham (2021). Any college enrollment is equal to 1 if an individual enrolls in any type of college. On-time college enrollment is defined as enrolling in college exactly 13 years after kindergarten. In districts with Pre-K access for age 3+, treatment is defined based on when an individual turns three relative to when Pre-K becomes available in their district. The omitted category is t = -2. In districts with Pre-K access for age 4 only, treatment is defined based on when an individual turns 4 relative to when Pre-K becomes available in their district, and the omitted category is t = -1. In each figure, we report the coefficient estimates along with 95% confidence intervals. Standard errors are clustered at the school district level.

Figure A5: The Effect of Access to Pre-K on College Graduation

(a) Districts with Pre-K Access for Age 3+



(b) Districts with Pre-K Access for Age 4

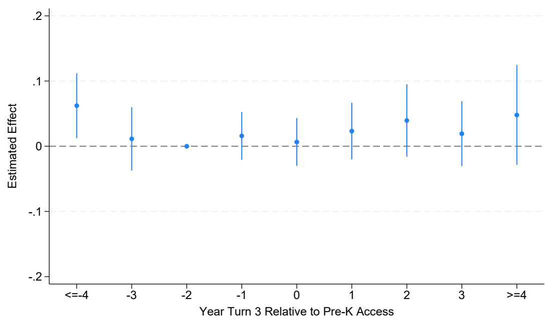


Notes: These figures show the effect of access to Pre-K on college graduation, by treatment type, using Sun and Abraham (2021). College graduation is equal to one if an individual ever graduates college. In districts with Pre-K access for age 3+, treatment is defined based on when an individual turns three relative to when Pre-K becomes available in their district. The omitted category is t = -2. In districts with Pre-K access for age 4 only, treatment is defined based on when an individual turns 4 relative to when Pre-K becomes available in their district, and the omitted category is t = -1. In each figure, we report the coefficient estimates along with 95% confidence intervals. Standard errors are clustered at the school district level.

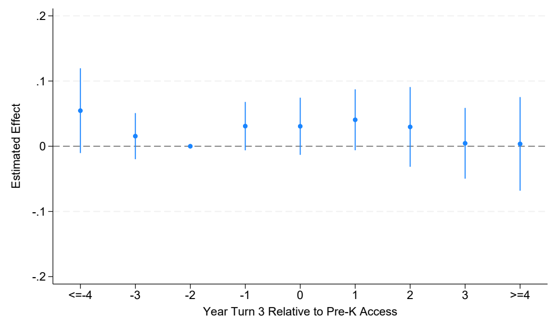
Figure A6: The Effect of Access to Pre-K on Third Grade Test Scores

Districts with Pre-K Access for Age 3+

(a) Reading

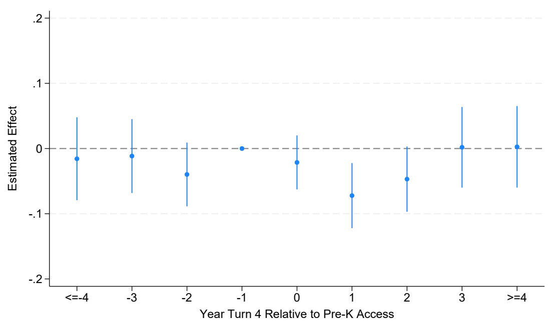


(b) Math

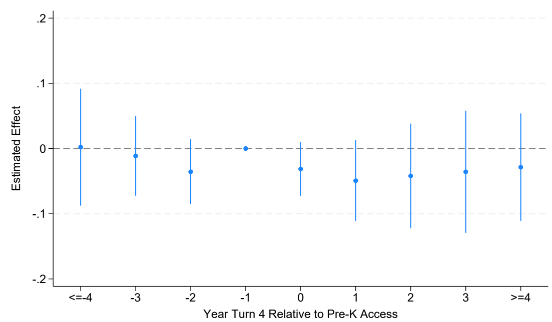


Districts with Pre-K Access for Age 4

(c) Reading

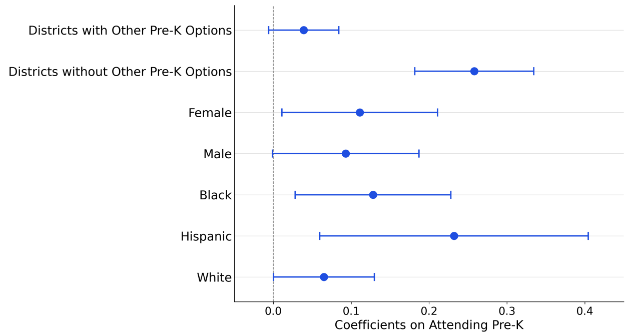


(d) Math

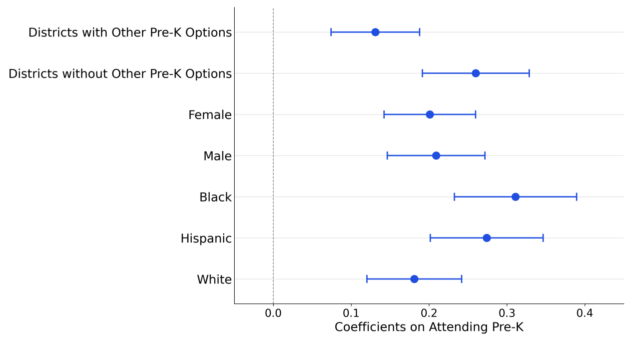


Notes: These figures show the effect of access to Pre-K on third grade test scores, by treatment type, using Sun and Abraham (2021). Test scores are standardized by cohort-subject such that the mean is zero and the standard deviation is one. In districts with Pre-K access for age 3+, treatment is defined based on when an individual turns three relative to when Pre-K becomes available in their district. The omitted category is t = -2. In districts with Pre-K access for age 4 only, treatment is defined based on when an individual turns 4 relative to when Pre-K becomes available in their district, and the omitted category is t = -1. The results for districts where Pre-K is available for age four first then age three are reported in Figure B5. In each figure, we report the coefficient estimates along with 95% confidence intervals. Standard errors are clustered at the school district level.

Figure A7: The Effect of Access to Pre-K on Pre-K Enrollment by Group



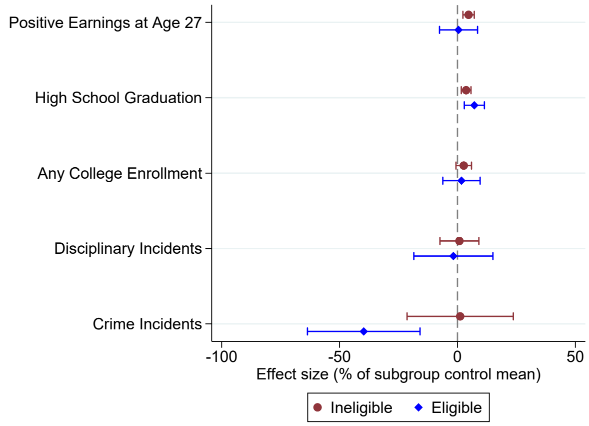
(a) Districts with Pre-K Access for Age 3



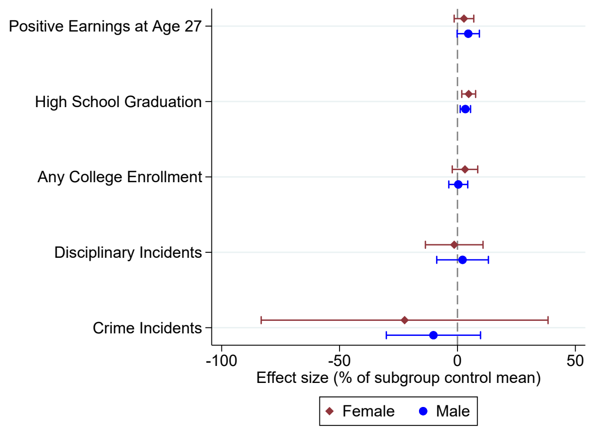
(b) Districts with Pre-K Access for Age 4

Notes: These figures show heterogeneous treatment effects of access to Pre-K on Pre-K enrollment by the availability of alternative preschool options, sex, and ethnicity. We estimate Sun and Abraham (2021) separately for each subgroup, and we report the coefficient estimates with their 95% confidence intervals. In all regressions, standard errors are clustered at the school district level.

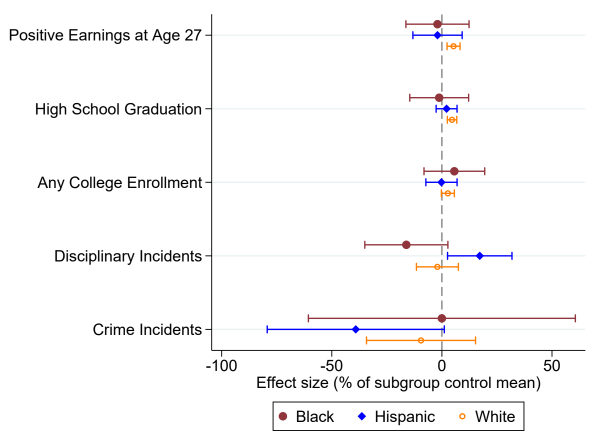
Figure A8: Heterogeneous Effects in Districts with Access for Age 3+



(a) By Eligibility Status



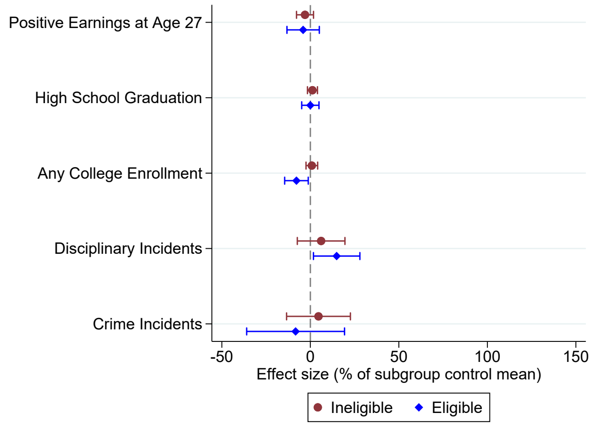
(b) By Sex



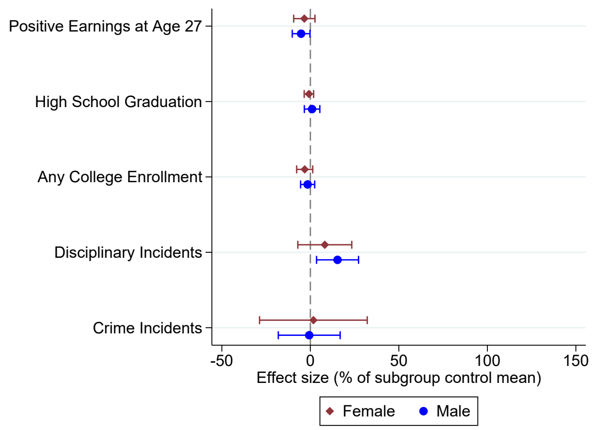
(c) By Ethnicity

Notes: These figures show heterogeneous treatment effects of access to Pre-K by eligibility status, sex and ethnicity in districts that provide Pre-K for both three- and four-year-olds simultaneously. We estimate Sun and Abraham (2021) separately for each subgroup, and we report the coefficient estimates with their 95% confidence intervals, divided by the control mean for each subgroup. In all regressions, standard errors are clustered at the school district level.

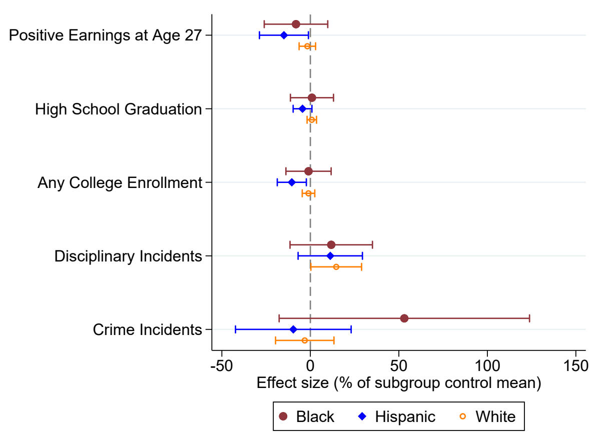
Figure A9: Heterogeneous Effects in Districts with Access for Age 4



(a) By Eligibility Status



(b) By Sex

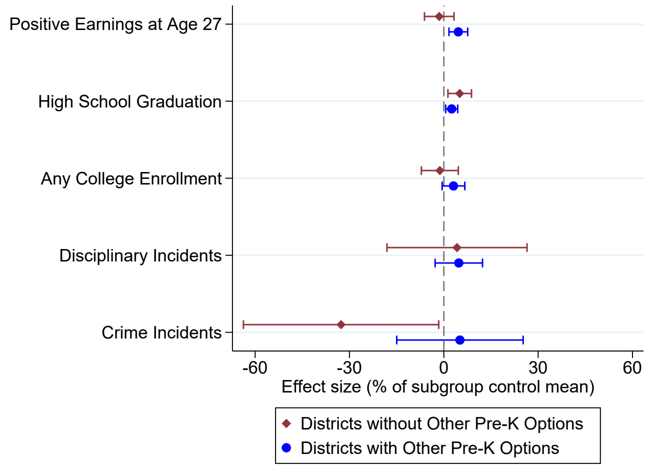


(c) By Ethnicity

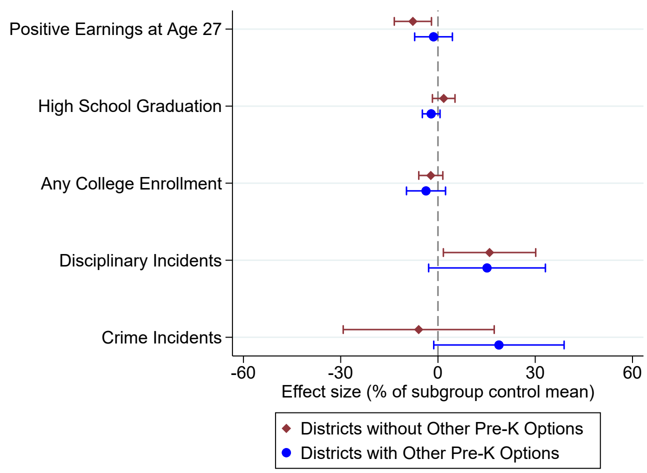
Notes: These figures show heterogeneous treatment effects of access to Pre-K by eligibility status, sex and ethnicity in districts that provide Pre-K for four-year-olds only. We estimate Sun and Abraham (2021) separately for each subgroup, and we report the coefficient estimates with their 95% confidence intervals, divided by the control mean for each subgroup. In all regressions, standard errors are clustered at the school district level.

Figure A10: Heterogeneous Effects by Availability of Other Pre-K Options

(a) Districts with Pre-K Access for Age 3+



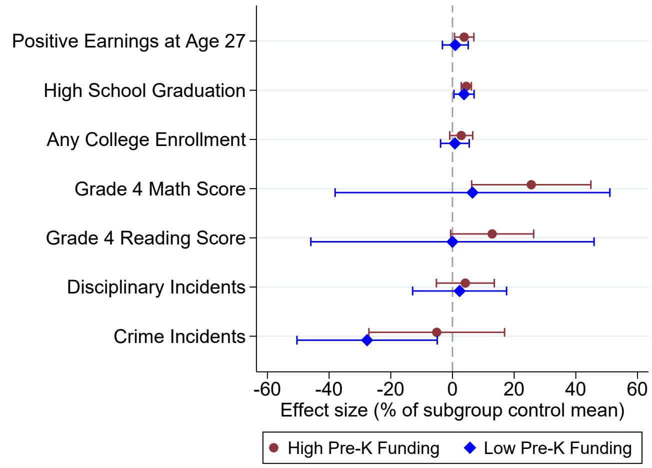
(b) Districts with Pre-K Access for Age 4



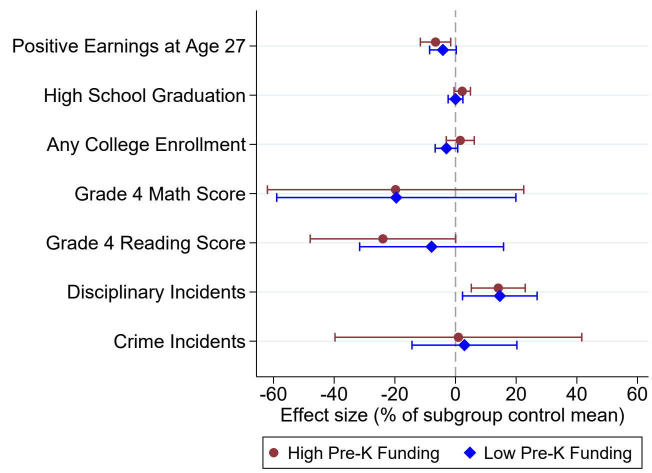
Notes: These figures show heterogeneous treatment effects of access to Pre-K by alternative Pre-K options availability status for each districts. We estimate Sun and Abraham (2021) separately for each subgroup, and we report the coefficient estimates with their 95% confidence intervals, divided by the control mean for each subgroup. Alternative Pre-K options availability is defined at the district level. A district is defined as having alternative Pre-K options if it has at least one Head Start or private preschool center between 1995 and 2000. In all regressions, standard errors are clustered at the school district level.

Figure A11: Heterogeneous Effects by Pre-K Funding

(a) Districts with Pre-K Access for Age 3+



(b) Districts with Pre-K Access for Age 4

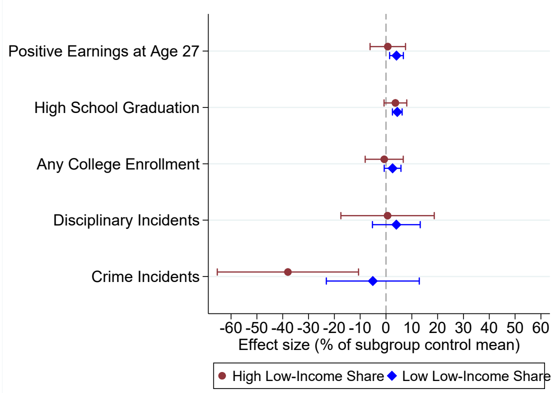


Notes: These figures show heterogeneous treatment effects of access to Pre-K by the amount of available Pre-K funds in a given district. In any given year, a district is classified as a high Pre-K funding district if it has more than average Pre-K funds and low Pre-K funding otherwise. We estimate Sun and Abraham (2021) separately for each subgroup, and we report the coefficient estimates with their 95% confidence intervals, divided by the control mean for each subgroup. In all regressions, standard errors are clustered at the school district level.

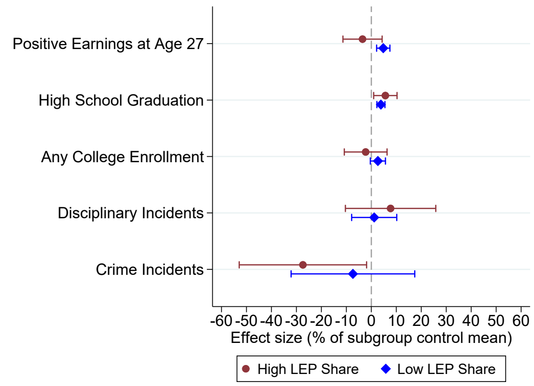
Figure A12: Heterogeneous Effects by Peer Composition

Districts with Pre-K Access for Age 3+

(a) By Low-Income Share

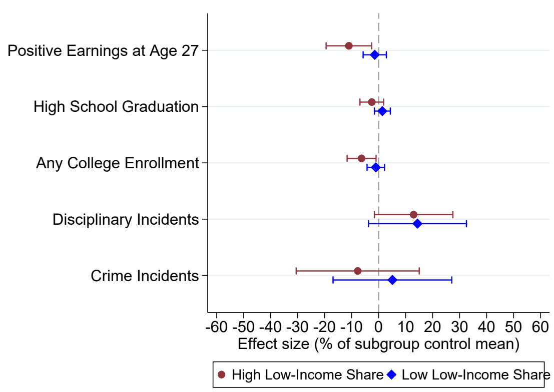


(b) By Low English Proficiency Share

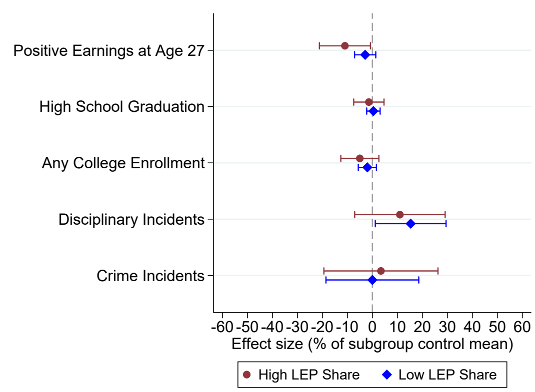


Districts with Pre-K Access for Age 4

(c) By Low-Income Share



(d) By Low English Proficiency Share



Notes: These figures show heterogeneous treatment effects of access to Pre-K by district peer composition, focusing on the share of low-income and low English proficiency individuals. For any given district, high low-income share (or low English proficiency share) is equal to 1 if more than 50 percent of the individuals in that district are classified as low-income (or low English proficient) (correct version: equals 1 if a district's share of low-income students exceeds the average for the full sample. The full sample includes four groups: the control group, districts that provide Pre-K to children age 3 and older, districts that provide Pre-K only to 4-year-olds, and districts that provide Pre-K to 4-year-olds plus transportation for children age 3 and older.). We estimate Sun and Abraham (2021) separately for each subgroup, and we report the coefficient estimates with their 95% confidence intervals, divided by the control mean for each subgroup. In all regressions, standard errors are clustered at the school district level.

Table A1: List of Criminal Incidents

CODE	TRANSLATION
1	PERMANENT REMOVAL BY TEACHER FROM CLASS
2	CONDUCT PUNISHABLE AS A FELONY 37.006(A)(2)(A)
4	POSSESSED, SOLD, USED OR WAS UNDER THE INFLUENCE OF MARIHUANA OR OTHER CONTROLLED SUBSTANCE
5	POSSESSED, SOLD, USED OR WAS UNDER THE INFLUENCE OF AN ALCOHOLIC BEVERAGE
6	ABUSE OF A VOLATILE CHEMICAL
7	PUBLIC LEWDNESS OR INDECENT EXPOSURE
8	RETALIATION AGAINST SCHOOL EMPLOYEE
9	TITLE 5 FELONY - OFF CAMPUS AND NOT AT SCHOOL SPONSORED ACTIVITY
10	NON TITLE 5 FELONY CONDUCT-NOT ON CAMPUS OR AT SCHOOL SPONSORED ACTIVITY
11	BROUGHT FIREARM TO SCHOOL - TEC37.007(e) OR UNLAWFUL CARRY OF A HANDGUN
12	UNLAWFUL CARRY OF AN ILLEGAL KNIFE UNDER PENAL CODE 46.02-TEC 37.007(a)(1)
13	UNLAWFUL CARRYING OF A CLUB UNDER PENAL CODE 46.02 - TEC 37.007(a)(1)
14	CONDUCT CONTAINING THE ELEMENTS OF AN OFFENSE RELATING TO PROHIBITED WEAPONS
16	ARSON
17	MURDER, CAPITAL MURDER, CRIMINAL ATTEMPT TO COMMIT MURDER OR CAPITAL MURDER
18	INDECENCY WITH A CHILD
19	AGGRAVATED KIDNAPPING
22	CRIMINAL MISCHIEF
26	TERRORISTIC THREAT - TEC SECTION 37.006(A)(1) OR 37.007(B)
27	ASSAULT UNDER PENAL CODE SECT. 22.01(A)(1) AGAINST A DISTRICT EMPLOYEE/VOLUNTEER
28	ASSAULT UNDER PENAL CODE SECT 22.01(A)(1) OTHER THAN SCHOOL DISTRICT EMPLOYEE/VOLUNTEER
29	AGGRAVATED ASSAULT (PC SECT 22.02) AGAINST SCHOOL DISTRICT EMPLOYEE/VOLUNTEER
30	AGGRAVATED ASSAULT (PC SECT 22.02) AGAINST SOMEONE OTHER THAN DISTRICT EMPLOYEE
31	SEXUAL OR AGGRAVATED ASSAULT AGAINST A SCHOOL DISTRICT EMPLOYEE/VOLUNTEER
32	SEXUAL OR AGGRAVATED SEXUAL ASSAULT AGAINST SOMEONE O/T DISTRICT EMPLOYEE/VOLUNTEER
35	FALSE ALARM/FALSE REPORT - TEC SECTION 37.006(A)(1) AND 37.007(B)
36	FELONY CONTROLLED SUBSTANCE VIOLATION - TEC SECTION 37.007(A)(3)
37	FELONY ALCOHOL VIOLATION - TEC SECTION 37.007(A)(3)
46	AGGRAVATED ROBBERY - TEC 37.007(a)(2)(F), TEC 37.006 (c)-(d)
47	MANSLAUGHTER
48	CRIMINALLY NEGLIGENT HOMICIDE
49	ENGAGES IN DEADLY CONDUCT
50/52	USED, EXHIBITED OR POSSESSED A NON-ILLEGAL KNIFE PER STUDENT CODE CONDUCT
51	USED, EXHIBITED, OR POSSESSED A FIREARM OFF-CAMPUS BUT W/IN 300FT OF SCHOOL
53	ENGAGED IN CONDUCT THAT OFFENSES ARE SPEC IN TEC OFF-CAMPUS BUT W/IN 300FT
54	ENGAGE IN CONDUCT PUNISH AS FELONY OFF-CAMPUS BUT W/IN 300FT

Table A2: The Effect of Access to Pre-K on Attrition

	(1) Ever Drop Out	(2) Ever Exit Texas Public School System
Panel A: Districts with Access to Pre-K for 3+		
	-0.003	0.028
	(0.006)	(0.019)
Observations	73524	73524
Panel B: Districts with Access to Pre-K for 4		
	0.004	0.003
	(0.004)	(0.016)
Observations	67911	67911
Panel C: Districts with Access to Pre-K for 4 then 3+		
Panel C1: Access for 4		
	0.004	-0.004
	(0.006)	(0.015)
Panel C2: Access for 3+		
	0.000	-0.007
	(0.005)	(0.019)
Observations	49234	49234
Control mean	0.03	0.18

Notes: This table shows the effect of access to Pre-K on the likelihood of ever dropping out and the likelihood of exiting the Texas Public School System. According to Texas law, individuals are allowed to drop out of school at age 17 or 18. Ever dropping out is defined as 1 if an individual is observed to drop out between 2000 and 2013. Ever exiting the system is equal to 1 if an individual exits between 1995 and 2015. Exiting the system includes switching to a private school, homeschooling, or an out-of-state school. Standard errors are clustered at the school district level and reported in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table A3: Robustness of the Effect of Access to Pre-K in Districts with Access for Age 3+

	Control Mean	(1) Main	(2) + Individual Controls	(3) + School FE
Positive Earnings at Age 27	45,829.35	1,661.459**	1,728.035***	1,164.628**
		(661.061)	(586.936)	(591.507)
High School Graduation	0.725	0.029***	0.032***	0.029***
		(0.006)	(0.006)	(0.006)
Any College Enrollment	0.640	0.011	0.016*	0.006
		(0.010)	(0.009)	(0.009)
Disciplinary Incidents	3.185	0.074	0.037	0.237*
		(0.134)	(0.127)	(0.140)
Crime Incidents	0.108	-0.013	-0.011	-0.009
		(0.014)	(0.015)	(0.015)
Grade 4 Reading Score	0.203	0.022	0.035*	0.005
		(0.020)	(0.019)	(0.021)
Grade 4 Math Score	0.182	0.042*	0.049**	0.028
		(0.023)	(0.022)	(0.022)

Notes: This table shows the robustness of the main estimates to controlling for observable characteristics (column (2)) and school fixed effects (column (3)). Individual characteristics in column (2) include indicator variables for English proficiency status, low-income, and race. Standard errors clustered at the school-district level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Robustness of the Effect of Access to Pre-K in Districts with Access for Age 4

	Control Mean	(1) Main Model	(2) + Individual Controls	(3) + School FE
Positive Earnings at Age 27	45,829.35	-1,993.185**	-1,776.428*	-2,086.316**
		(955.057)	(956.304)	(944.827)
High School Graduation	0.725	0.001	0.005	0.002
		(0.009)	(0.009)	(0.009)
Any College Enrollment	0.640	-0.017	-0.010	-0.014
		(0.011)	(0.011)	(0.012)
Disciplinary Incidents	3.185	0.441**	0.427**	0.440**
		(0.182)	(0.179)	(0.181)
Crime Incidents	0.108	0.001	0.003	0.001
		(0.008)	(0.009)	(0.008)
Grade 4 Reading Score	0.203	-0.017	-0.007	-0.007
		(0.021)	(0.021)	(0.023)
Grade 4 Math Score	0.182	-0.030	-0.022	-0.017
		(0.031)	(0.030)	(0.032)

Notes: This table shows the robustness of the main estimates to controlling for observable characteristics (column (2)) and school fixed effects (column (3)). Individual characteristics in column (2) include indicator variables for English proficiency status, low-income, and race. Standard errors clustered at the school-district level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Per Child Funding and Effect Sizes Across State-Funded Pre-K Programs

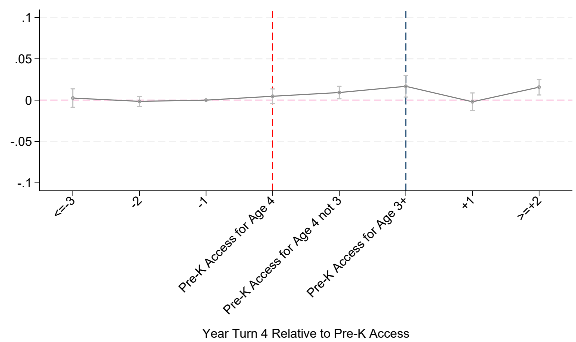
	Program				
	Texas (Pre-K for 3+)	Texas (Pre-K for 4 only)	Tennessee	Boston	Georgia
Universal	No	No	No	Yes	Yes
Funding amount ($/child)	13,201	1,510	2,226	8,890	3,721
	Our paper	Our paper	Durkin et al. (2022)	Gray-Lobe et al. (2023)	Fitzpatrick (2008)
Math test scores	23%*	-16%	-0.119%**	-11%	+1%
Reading test scores	11%	-8%	-0.062%	+6%	+2%
High school graduation	+4%***	0.1%	-	+9%**	-
College enrollment	+2%	-3%	-3%	+18%***	-
Earnings (age 27)	+4%***	-4%	-	-	-

Notes: We retrieve funding information for the 2001-2002 academic year from the NIEER 2003 state yearbooks for the state-funded Pre-K programs in other states. In 2001-2002, the state of Massachusetts operated a targeted Pre-K program that cost $4,826 per child. However, Boston has had its own universal Pre-K program, with funding reaching $13,000 per child in 2020 ($8,890 in 2001 USD) according to Gray-Lobe et al. (2023). Third-grade test scores are reported in Durkin et al. (2022), Gray-Lobe et al. (2023), and our paper, while Fitzpatrick (2008) focus on fourth-grade test scores.

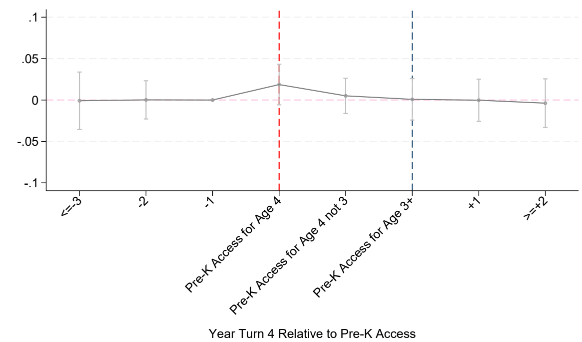
Appendix B: Results for Districts with Access for Age 4 then 3+

Figure B1: The Correlation between Access to Pre-K and Observable Individual Characteristics in Districts with Access for Age 4 then 3+

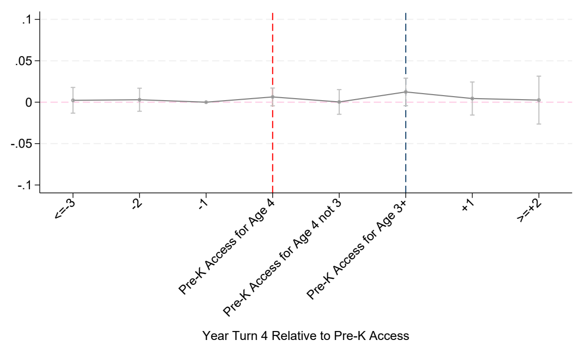
(a) Black



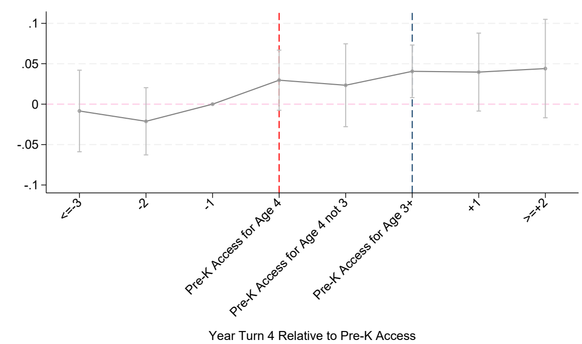
(b) Hispanic



(c) Limited English Proficiency



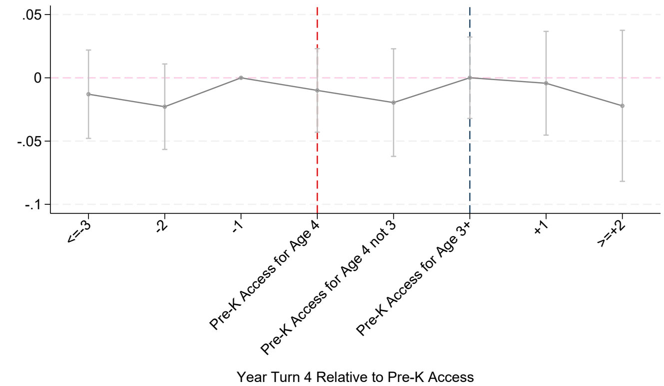
(d) Low Income



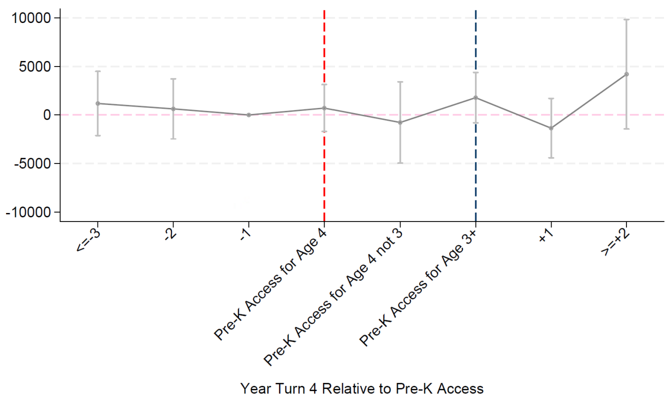
Notes: These figures show the correlation between access to Pre-K and observable individual characteristics by estimating Sun and Abraham (2021) using each observable characteristic as the dependent variable. In all estimations, t = 0 is defined as the period when Pre-K becomes accessible for four-year-olds in a given district. The coefficient estimate between the dotted vertical red and black lines correspond to those who turn four after t = 0 but before access expands to three-year-olds. The omitted category is t = -1. Each sub-figure reports the coefficient estimates and the 95% confidence intervals for the indicated characteristic. Standard errors are clustered at the school district level.

Figure B2: The Effect of Access to Pre-K on Earnings at Age 27 in Districts with Access for Age 4 then 3+

(a) Have Positive Earnings



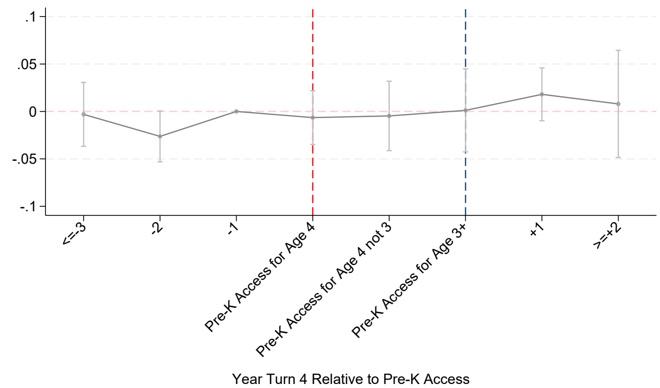
(b) Positive Earnings



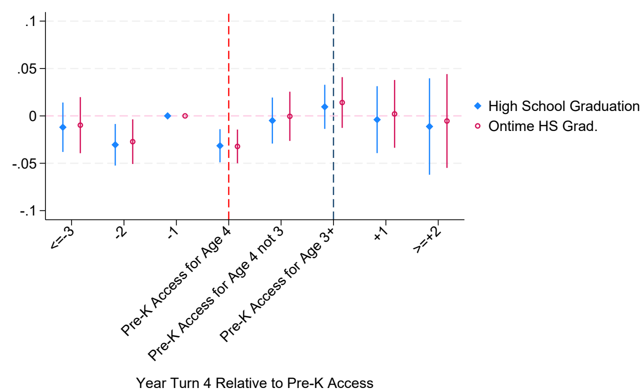
Notes: These figures show the effect of access to Pre-K on the likelihood of having positive earnings and on positive earnings (excluding 0s) at age 27 in districts that provide Pre-K for age 4 then age 3, using Sun and Abraham (2021). In all estimations, t = 0 is defined as the period when Pre-K becomes accessible for four-year-olds in a given district. The coefficient estimate between the dotted vertical red and black lines correspond to those who turn four after t = 0 but before access expands to three-year-olds. The omitted category is t = -1. In each figure, we report the coefficient estimates along with 95% confidence intervals. Standard errors are clustered at the school district level.

Figure B3: The Effect of Access to Pre-K on College Enrollment and High School Graduation in Districts with Access for Age 4 then 3+

(a) College Enrollment



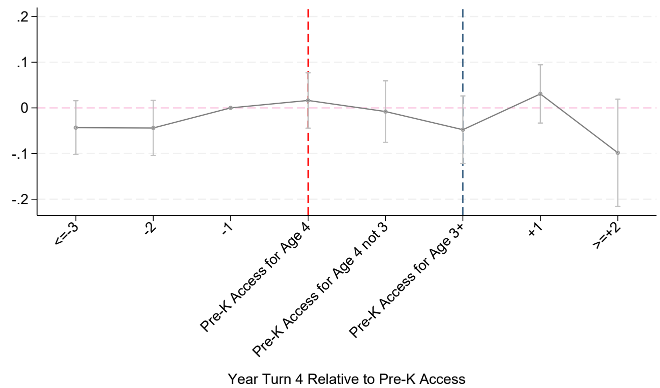
(b) High School Graduation



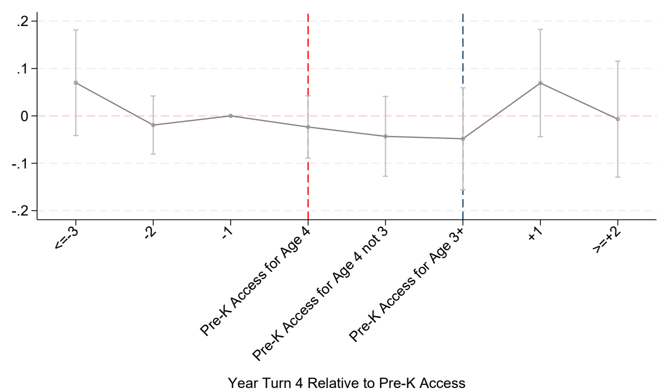
Notes: These figures show the effect of access to Pre-K on the likelihood of college enrollment (sub-figure (a)) and high school graduation (sub-figure (b)) in districts that provide Pre-K for age 4 then age 3, using Sun and Abraham (2021). In all estimations, t = 0 is defined as the period when Pre-K becomes accessible for four-year-olds in a given district. The coefficient estimate between the dotted vertical red and black lines correspond to those who turn four after t = 0 but before access expands to three-year-olds. The omitted category is t = -1. On-time high school graduation is defined as graduating high school within 13 years of kindergarten. In each figure, we report the coefficient estimates along with 95% confidence intervals. Standard errors are clustered at the school district level.

Figure B4: The Effect of Access to Pre-K on Fourth Grade Test Scores in Districts with Access for Age 4 then 3+

(a) Reading



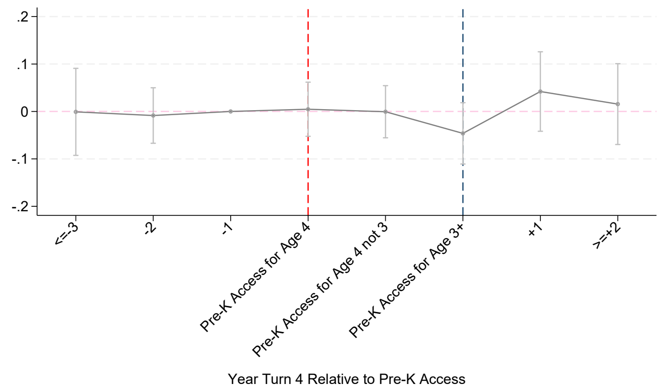
(b) Math



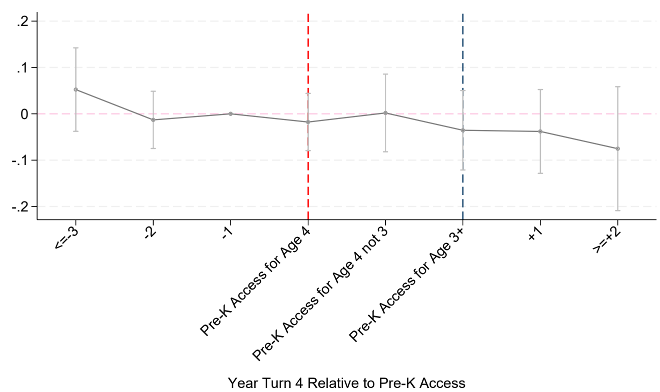
Notes: These figures show the effect of access to Pre-K on fourth grade test scores in districts that provide Pre-K for age 4 then age 3, using Sun and Abraham (2021). Test scores are normalized by subject-cohort such that the mean is zero and the standard deviation is 1. In all estimations, t = 0 is defined as the period when Pre-K becomes accessible for four-year-olds in a given district. The coefficient estimate between the dotted vertical red and black lines correspond to those who turn four after t = 0 but before access expands to three-year-olds. The omitted category is t = -1. In each figure, we report the coefficient estimates along with 95% confidence intervals. Standard errors are clustered at the school district level.

Figure B5: The Effect of Access to Pre-K on Third Grade Test Scores in Districts with Access for Age 4 then 3+

(a) Reading



(b) Math



Notes: These figures show the effect of access to Pre-K on third grade test scores in districts that provide Pre-K for age 4 then age 3, using Sun and Abraham (2021). Test scores are normalized by subject-cohort such that the mean is zero and the standard deviation is 1. In all estimations, t = 0 is defined as the period when Pre-K becomes accessible for four-year-olds in a given district. The coefficient estimate between the dotted vertical red and black lines correspond to those who turn four after t = 0 but before access expands to three-year-olds. The omitted category is t = -1. In each figure, we report the coefficient estimates along with 95% confidence intervals. Standard errors are clustered at the school district level.

Table B1: The Effect of Access to Pre-K on Earnings in Districts with Access for Age 4 then 3+

	Age 23	Age 24	Age 25	Age 26	Age 27
Panel A: Positive Earnings (excluding 0s)					
Access for 4	423.008	267.119	-225.070	-142.528	-33.145
	(780.445)	(1,225.722)	(981.341)	(1,032.016)	(1,286.790)
Access for 3+	653.220	-111.528	240.737	583.557	1,541.391
	(968.278)	(1,078.920)	(963.833)	(1,251.669)	(1,297.102)
Control Mean	27,910	31,960	36,290	40,570	45,830
Observations	35,554	35,272	35,021	34,622	34,088
Panel B: Have Positive Earnings					
Access for 4	-0.019	-0.001	-0.015	-0.019	-0.015
	(0.015)	(0.017)	(0.020)	(0.019)	(0.017)
Access for 3+	-0.023	-0.002	-0.015	-0.019	-0.009
	(0.018)	(0.018)	(0.018)	(0.017)	(0.018)
Control Mean	0.647	0.643	0.637	0.630	0.620
Observations	55,006	55,006	55,006	55,006	55,006
Panel C: Earnings (including 0s)					
Access for 4	-337.040	56.435	-692.059	-919.398	-733.777
	(694.208)	(975.231)	(954.383)	(1,018.647)	(1,215.135)
Access for 3+	-287.162	-279.989	-380.234	-569.443	508.329
	(708.397)	(800.962)	(814.329)	(821.278)	(1,068.701)
Control Mean	18,050	20,540	23,130	25,570	28,430
Observations	55,006	55,006	55,006	55,006	55,006

Notes: This table shows the effect of access to Pre-K in districts that provide Pre-K for 4-year-olds first then 3+ year-olds on labor market outcomes using Equation 3. Panel A shows the effect on earnings conditional on observing positive earnings. Panel B shows the effect on the likelihood of having positive earnings. Panel C shows the effect on annual earnings, where missing earnings are replaced by zero. In each panel, the first row reports the coefficient estimates of providing Pre-K for age 4, while the second row shows the coefficient estimate after the district expands to age 3. Each column represents these outcomes at a different age, from 23 to 27. In all regressions, standard errors are clustered at the school district level and reported in parentheses.* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table B2: The Effect of Access to Pre-K on Educational Outcomes in Districts with Access for Age 4 then 3+

	(1) High School Graduation	(2) On-Time High School	(3) Any College	(4) Community College	(5) Public University	(6) On-Time College	(7) Any College Graduation	(8) Community College Graduation	(9) Public University Graduation
Access for 4	-0.018**	-0.016*	-0.006	-0.009	-0.008	-0.017	-0.008	-0.004	-0.019**
	(0.008)	(0.009)	(0.015)	(0.013)	(0.008)	(0.013)	(0.012)	(0.007)	(0.009)
Access for 3+	-0.002	0.004	0.009	0.010	0.003	0.000	-0.011	-0.012	-0.022**
	(0.015)	(0.015)	(0.017)	(0.017)	(0.010)	(0.016)	(0.016)	(0.014)	(0.010)
Control Mean	0.725	0.719	0.640	0.556	0.293	0.564	0.359	0.142	0.211
Observations	55,006	55,006	55,006	55,006	55,006	55,006	55,006	55,006	55,006

Notes: This table shows the effect of access to Pre-K on a set of educational outcomes using Equation 3 for districts that provide Pre-K sequentially, first for 4 year-olds and then for 3+ year-olds. High school graduation refers to graduating from Texas public schools from 2003 to 2023. On-time graduation is equal to one if an individual graduates from high school 13 years after kindergarten, and zero otherwise. College enrollment indicates enrollment in any college between 2006 to 2021. On-time college is equal to 1 if an individual enrolls in college 13 years after kindergarten, and 0 otherwise. College graduation indicates graduation from any college between 2006 to 2024. In all regression equations, standard errors are clustered at the district level and reported in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table B3: The Effect of Access to Pre-K on Behavioral Outcomes in Districts with Access for Age 4 then 3+

	(1) Disciplinary Incidents	(2) Crime Incidents	(3) Grade Retention	(4) Days Absent	(5) Years of Special Ed.	(6) Years of Disability	(7) Medical/Health Services	(8) Therapy Services
Access for 4	-0.412	-0.033	-0.014	-0.176	0.003	-0.006	-0.022	-0.040
	(0.299)	(0.023)	(0.020)	(1.498)	(0.015)	(0.062)	(0.013)	(0.046)
Access for 3+	-0.645**	-0.019	-0.016	-2.331	-0.022	-0.130	-0.028	-0.119**
	(0.315)	(0.019)	(0.021)	(2.485)	(0.026)	(0.097)	(0.023)	(0.051)
Control Mean	3.185	0.108	0.232	73.583	0.080	1.221	0.028	0.540
Observations	55,006	55,006	55,006	55,006	55,006	55,006	55,006	55,006

Notes: This table shows the effect of access to Pre-K on a set of educational outcomes using Equation 3 for districts that provide Pre-K sequentially, first for 4 year-olds and then for 3+ year-olds. Columns (1)–(4) report effects on the total number of disciplinary incidents, grade retention, days absent between K-12, and criminal incidents within 5-12 years post KG. Columns (5)–(6) report effects on years receiving special education and disability services. Columns (7)–(8) report effects on the number of medical/health services and therapy services received during K–12. Standard errors clustered at the district level are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B4: Robustness of the Effect of Access to Pre-K in Districts with Access for Age 4 then 3+

	(1) All	(2) + Individual Controls	(3) + School FE
Panel A: Positive Earnings at Age 27			
Access for 4	-33.145	359.115	51.322
	(1,286.790)	(1,317.421)	(1,312.679)
Access for 3+	1,541.391	2,128.139*	1,657.663
	(1,297.102)	(1,273.306)	(1,244.327)
Control Mean	45,829.35	45,829.35	45,829.35
Observations	34,088	33,663	34,086
Panel B: High School Graduation			
Access for 4	-0.018**	-0.020**	-0.019**
	(0.008)	(0.010)	(0.008)
Access for 3+	-0.002	0.000	-0.006
	(0.015)	(0.016)	(0.015)
Control Mean	0.725	0.725	0.725
Observations	55,006	54,309	54,999
Panel C: Any College Enrollment			
Access for 4	-0.006	-0.002	-0.005
	(0.015)	(0.016)	(0.015)
Access to Pre-K for 3+	0.009	0.015	0.007
	(0.017)	(0.018)	(0.017)
Control Mean	0.640	0.640	0.640
Observations	55,006	54,309	54,999
Panel D: Disciplinary Incidents			
Access for 4	-0.412	-0.454	-0.411
	(0.299)	(0.314)	(0.301)
Access for 3+	-0.645**	-0.732**	-0.607*
	(0.315)	(0.306)	(0.315)
Control Mean	3.185	3.185	3.185
Observations	55,006	54,309	54,999
Panel E: Crime Incidents			
Access for 4	-0.033	-0.025	-0.032
	(0.023)	(0.023)	(0.023)
Access for 3+	-0.019	-0.013	-0.017
	(0.019)	(0.018)	(0.019)
Control Mean	0.108	0.108	0.108
Observations	55,006	54,309	54,999
Panel F: Grade 4 Reading Score			
Access for 4	0.004	0.018	0.007
	(0.030)	(0.029)	(0.030)
Access for 3+	-0.038	-0.020	-0.030
	(0.036)	(0.035)	(0.036)
Control Mean	0.203	0.203	0.203
Observations	46,561	45,952	46,557
Panel G: Grade 4 Math Score			
Access for 4	-0.033	-0.019	-0.029
	(0.032)	(0.033)	(0.032)
Access for 3+	0.005	0.022	0.014
	(0.047)	(0.047)	(0.047)
Control Mean	0.182	0.182	0.182
Observations	46,561	45,952	46,557

Notes: This table reports robustness estimates for the effects of access to Pre-K in districts with Pre-K access for age 4 then 3+. Column (1) reports the baseline specification, column (2) adds controls including limited English proficiency, eligibility for free or reduced meals, and race, and column (3) adds school fixed effects. Standard errors clustered at the school-district level are reported in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.