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

Maya Mikdash* Jiee Zhong‡
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First draft: March 26, 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. Despite these large increases in take-up, we find heterogeneous effects of access across districts. In districts offering Pre-K to three- and four-year-olds simultaneously, access increases high school graduation, decreases juvenile crime, and increases earnings at age 23 by approximately 5 percent, though these gains do not persist into the late twenties. In contrast, districts that primarily provide access for four-year-olds show precise null effects on long- and intermediate-term outcomes. We provide suggestive evidence that the effectiveness of state-funded Pre-K depends on resource availability rather than access alone.

*We thank Chloe Gibbs, Jocelyn Wikle, Taryn Morrissey, Anamarie Auger Whitaker, and seminar participants at Miami University, MidSouth Education Policy Workshop, APPAM 2025, PPCW2026, 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.

†Louisiana State University, Department of Economics, mayamikdash@lsu.edu

‡Miami University, Department of Economics, zhongj8@miamioh.edu

1 Introduction

Early childhood education is widely viewed as a high-return public investment. Indeed, a substantial body of research suggests that high-quality early childhood programs, such as the Abecedarian Project and Perry Preschool, can yield lasting positive outcomes. These findings have motivated substantial public investment in state-funded prekindergarten (Pre-K) programs across the United States. In 2022, states spent almost $12 billion on preschool programs—more than double the inflation-adjusted average from 2002-2003 (NIEER (2025a)). Yet modern public Pre-K differs sharply from early programs in terms of funding and scale. For instance, per-child spending in the Abecedarian Project exceeded $100,000, compared to less than $8,000 per child in contemporary state-funded programs (Whitaker et al. (2026)). Moreover, these programs operate in environments where alternative preschool options are widely available. Given the recency of these programs, most existing evidence focuses on short- and intermediate-term effects, and the evidence is mixed. Moreover, these outcomes may not fully capture long-term effects (e.g., Chetty et al. (2011)). Consequently, evidence on the long-run impacts of large-scale state-funded Pre-K on labor market and educational outcomes remains limited.

In this paper, we estimate the causal effect of access to state-funded Pre-K on earnings, college enrollment, high school graduation, and intermediate educational and behavioral 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 link individuals' educational records to juvenile crime and labor market outcomes.

Our first stage results show that Pre-K availability substantially increases public Pre-K enrollment for both eligible and ineligible children. For instance, 38-60% and 5-16% of eligi-

ble and ineligible children attend state-funded Pre-K when it becomes available, respectively. Despite these significant increases in take-up, long-run impacts are modest and heterogeneous across district types. In districts offering Pre-K to both three- and four-year-olds simultaneously, access to state-funded Pre-K increases high school graduation by approximately two percentage points (3 percent), reduces juvenile crime incidents by 25 percent, and has no effect on college enrollment. We also document a 5 percent increase in earnings at age 23 in these districts, driven by both a higher likelihood of employment at 23 and higher earnings conditional on being employed, though these gains fade by the late twenties. We estimate the impact on intermediate educational outcomes, such as third grade test scores, absenteeism, grade retention, and special education, but we see no statistically significant effects. This is in line with previous evidence that shows that short-term test scores might not be a reliable indicator of long-term effects and that access to early childhood education can impact long-term outcomes through non-cognitive channels (e.g., Chetty et al. (2011), Anders et al. (2023)). In contrast, expansions limited to four-year-olds generate precisely estimated null effects on test scores, college enrollment, earnings, and behavioral outcomes.

Despite large increases in Pre-K enrollment, average long-run impacts are limited, with gains concentrated in districts that serve both three- and four-year-olds simultaneously. We examine three potential explanations for this pattern: differences in counterfactual preschool environments, differences in the timing and intensity of exposure, and differences in program resources. We find that access to public Pre-K increases enrollment much more in districts without Head Start or private preschool options, indicating that public provision partly substitutes for existing programs. However, despite these large differences in take-up, long-run impacts do not vary meaningfully across these environments. This suggests that substitution across observable preschool alternatives alone cannot account for the largely null effects.

We also find limited support for explanations based on exposure timing or intensity. Although positive effects emerge in districts that offer Pre-K to both three- and four-year-olds, take-up among three-year-olds is low, and expansions to younger cohorts in other districts

do not generate similar improvements in long-run outcomes. Instead, the pattern of results is most consistent with differences in program resources and implementation capacity across districts. Districts that serve both age groups simultaneously are larger, more likely to be urban, and spend substantially more per child relative to other districts. Taken together, these findings suggest that program effectiveness depends critically on the context and resources with which programs are implemented.

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. Second, we document substantial heterogeneity in impacts across districts, showing that positive effects are concentrated in higher-resource implementation environments. These findings help reconcile differences between the evidence from modern state-funded Pre-K programs and earlier, more intensive interventions. In doing so, our paper contributes to a large literature on the effects 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), Nores et al. (2005), Barnett and Masse (2007), Heckman et al. (2010), 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 incorporated home visits, parental services, and health support in addition to classroom instruction. As a result, average spending 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

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

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), Ludwig and Miller (2007), Deming (2009), Bailey et al. (2021), Johnson and Jackson (2019), 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 short-term effects. In short, the evidence regarding the efficacy of these programs is mixed. 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. While Gray-Lobe et al. (2023) provide important evidence on the longer-run educational outcomes, evidence on labor market impacts of large-scale state-funded Pre-K remains limited. 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.

Taken together, these findings suggest that expanding access to early childhood education in a modern setting is not sufficient to generate long-run gains. Instead, we find suggestive evidence that the effectiveness of large-scale Pre-K programs may depend on the resources

and institutional capacity with which they are implemented. These results help reconcile the mixed evidence on modern state-funded Pre-K and highlight the importance of program design in shaping the returns to early childhood investments (Whitaker et al. (2026)).

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 from a low-income family, i.e., those 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, children from different socioeconomic backgrounds may attend Pre-K together.

According to the 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 if the resources are available. This generates additional variation in access to Pre-K for three-year-olds. Consequently, although state funding for Pre-K became available in 1985, children of the same age group are differentially exposed to Pre-K depending on their district and cohort. Of course, large school districts with hundreds or thousands of eligible students began offering Pre-K immediately. For instance, the Houston Independent School District (HISD) has provided Pre-K services since the 1985-1986 school year (HISD, Department of Research and Accountability (2017)), in contrast to smaller and more rural districts.

[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)).

This institutional setting generates quasi-random variation in access to state-funded Pre-K across districts and cohorts. Because districts cross the eligibility threshold at different times based on the number of eligible children, the availability of state-funded Pre-K varied across districts and cohorts.[4] As a result, following 1985, some children became age-eligible for Pre-K (turned four or three of age) in districts where state-funded Pre-K was available for their age group, while others became age-eligible in districts where it was not. Figure 1 shows the number of districts offering Pre-K services by academic year. As shown in the figure, more than 50% 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 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 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 reported sharing facilities and classroom instruction with other agencies, including Head Start. On average, the adult-child ratio was 1:11, with 22 students per class. Most teachers had six or more years of experience, but less than half of them had received any formal training in early childhood education. In addition, unlike programs such as Head Start,

[4]According to conversations with the Texas Education Agency (TEA), 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.

[5]Source: NIEER, 2023

parent involvement was minimal. Schools primarily communicated with parents through calls or teacher-parent conferences, and home visits were 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

Our primary data source is 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. 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 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-kindergarten through grade 12.

We define treatment based on when an individual becomes age-eligible for Pre-K (turns three or four) relative to when Pre-K becomes available in their district. Specifically, an individual is considered treated if they turn four (or three) in a given academic year 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, it is unavailable for children who were not enrolled in Pre-K. Hence, we use an individual's KG district as a proxy for the district in which they become age-eligible for Pre-K. To assess the validity of this proxy, we examine the subset of students who attend Pre-K and for whom both Pre-K and KG district information are observed. Among these students, nearly 90% attend KG in the same district in which they attended Pre-K, suggesting that KG district is a reliable proxy for district of residence at the time of eligibility.

We use district-level Pre-K enrollment data to determine whether a district offers Pre-K in a given year. A district is classified as offering Pre-K if at least one student is enrolled 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 groups: (1) districts that, at $t = 0$, provide Pre-K for both four- and three-year-olds simultaneously; (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; and (3) districts that, at $t = 0$, provide Pre-K for four-year-olds only but later expand to include three-year-olds. Control districts are defined as those with zero Pre-K enrollment throughout the sample period.

As we show in Figure 1, 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 figures include both districts that began offering Pre-K in 1994 and those that had already offered it prior to the start of our data. Because we do not observe Pre-K availability before 1994, we cannot distinguish between these groups. We therefore exclude districts with positive Pre-K enrollment in 1994 from our main analysis. In these districts, prior availability is unknown, so treatment status cannot be determined for children who became age-eligible before 1994. Among the remaining districts, Pre-K availability expands gradually over time, generating variation in exposure across cohorts and districts.[6]

To examine long-run outcomes, we restrict our sample to individuals who attend kindergarten (KG) between 1995 and 2001 (1995 and 2002) when studying the effects of access at age 4 (age 3). This ensures that we observe each individual's KG district, which we use to assign treatment status, even for cohorts that became age-eligible for Pre-K prior to the start of our data. In particular, because we restrict attention to districts that begin offering Pre-K in 1995 or later, individuals who attend KG in 1995 in districts that offer Pre-K for the first time in 1995 (and became age-eligible in earlier years) are classified as untreated.

This sample construction also allows us to observe long-run earnings for all cohorts. We observe earnings through 2023, which implies that the oldest cohorts are observed up to age 33, while the youngest cohorts are observed through ages 26–27. To maintain a balanced comparison across cohorts, we focus on earnings measured up to age 27.[7]

[6] As can be seen in the figure, the total number of districts varies over time. This could be due to consolidations of small districts or the creation of new ones as communities seek detachment from existing districts.

[7] Since the last year of our sample period is 2023, we cannot observe earnings at age 27 for individuals who attend KG in 2002. However, as we show later, the estimated effects are stable across ages 23–26.

Our final sample consists of 115,372 individuals who attended KG in Texas between 1995 and 2002.

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), we observe quarterly earnings information for every individual who ever worked in Texas, which we use to calculate annual earnings at ages 23 to 27. Note that earnings information is only observed if an individual is employed in Texas. This means that quarterly earnings are missing if an individual is unemployed, is self-employed, or if they are employed in a different state. In our analysis, we consider the effect on non-zero annual earnings and on annual earnings while replacing missing values with zeros. To assess whether our second measure yields biased results due to selection, we also estimate the effect of Pre-K access on the probability of having positive earnings at each age.

In addition, we observe several measures of long-term educational achievement, including high-school graduation and college enrollment. 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). We also examine the impact on on-time high school graduation, which we define as completing high school within 13 years after KG. 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. We construct an indicator equal 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. Finally, we examine the effect on on-time college enrollment, defined as enrolling in college within 14 years of KG.

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, the Texas Assessment of Knowledge and Skills (TAKS). The TAKS requires students in grades 3 to 11 to take a reading and math multiple-choice exams. The raw scores measure the number of correctly answered questions on each exam. To measure the impact on the students' performance on TAKS, 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 grade scores as a measure of intermediate academic performance, and we report the effect on fourth grade scores in the appendix.

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 within 13 years after Pre-K. Finally, the TEA data also allow us to observe additional important behavioral outcomes, including 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 within 13 years after Pre-K. Additionally, we observe the number of criminal charges that are filed against an individual until they turn 18, which we use to measure the impact on juvenile crime. Table A1 shows a list of criminal incidents that appear in the data.

3.4 Supplementary data

In addition to the ERC data, we collect information on the availability of alternative preschool options, 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 in a given year. We then aggregate this measure to the school district level based on the set of zip codes within each district. A district is considered to have alternative preschool options if at least one of its zip codes contains a Head Start center or a private preschool provider during our study period (1995–2000). Under this definition, we find that 67.5% of school districts in our sample do not have alternative preschool options within their boundaries.

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 control group, i.e., those who attend KG in a district that never provides Pre-K in our sample period. Column (2) shows the average outcomes for individuals who attend KG in districts that provide Pre-K for both three- and four-year-olds simultaneously. Column (3) shows the average outcomes for individuals who attend KG in districts that offer Pre-K for four-year-olds only, while Column (4) shows the average outcomes for individuals who attend KG in districts that first offer Pre-K for four-year-olds only then for three-year-olds.

Importantly, 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 65-77 percent of the districts are considered "rural." Across all districts, 40 to 45 percent of students are considered economically disadvantaged, based on KG enrollment in 1994, and between 4 and 7 percent are considered to have limited English proficiency.

The most notable differences across district types are average 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 enrollment in KG of 83 (Column (2)), relative to 36-47 in other districts (Column (1),(3) and (4)). They are also slightly more likely to be considered as urban or suburban.[8] Additionally, average Pre-K funding is substantially higher in these districts relative to the others. While per-child Pre-K funding in 1995 was almost $4,000 in districts that offer Pre-K for both age groups at the same time (Column (2)), other districts with Pre-K access spent between $1,300 and $2,200.[9]

First, all individuals in our sample attend KG at age 5.[10] 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 less likely to be White relative to other districts (74-77 percent compared to 81-83 percent). Individuals in the control group are slightly less likely to be Black relative to other groups (3 percent relative to 4-6 percent), and districts that offer Pre-K for three- and four-year-olds at the same time have slightly more Hispanic individuals (19 percent relative to 13-15 percent). 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 37 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)) at the same time are more likely to have individuals with limited English proficiency compared to all other groups.

Turning to our main outcomes of interest, average annual earnings between ages 25-27 for individuals in the control group is $25,670 (Column (1)), compared to $23,910 for those

[8]In the data, districts in our sample 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.

[9]Pre-K funding is not consistently available throughout our sample period, so the numbers in Table 1 correspond to Pre-K funding in 1995.

[10]Individuals who attend KG at an older age are only a small fraction of the sample (<5%) and are excluded from our analysis.

who attend KG in districts that provide Pre-K for three- and four-year-olds (Column (2)), $25,090 for those who attend KG in districts that provide Pre-K for four-year-olds (Column (3)), and $24,890 for those who attend KG in districts that provide Pre-K for four-year-olds then three-year-olds (Column (4)). Conditional on having positive earnings, average earnings between ages 25-27 is similar across all groups, which is around $39,000. Moreover, the likelihood of having positive earnings between ages 25-27 is similar across all groups as well (between 58 and 62 percent).

In terms of educational outcomes, individuals across all districts have similar 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, which is similar to the likelihood of graduating high school for individuals in districts that provide Pre-K for four-year-olds (Column (3)). In fact, the likelihood of enrollment in college is slightly higher for individuals in the control group relative to others (65 percent vs 60-62 percent), with very 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 73 percent, and the likelihood of on-time college enrollment being 56 and 58%.

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 13 years of Pre-K and less than one crimes committed before age 18. 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 medical or health service and less than one therapy service.

4 Empirical Strategy

To estimate the impact of access to 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, where we compare the outcomes of individuals who turn four (three) in districts where Pre-K is available to the outcomes of those who turn four (three) in districts where Pre-K is not available, controlling for district fixed effects 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 were heterogeneous across groups and/or over time (Borusyak and Jaravel (2018), De Chaisemartin and d'Haultfoeuille (2020), Goodman-Bacon (2021), Sun and Abraham (2021)).In our setting, treatment effects may vary across cohorts and over time, for example if program quality differs across districts or evolves over time. To address this concern, we use the interaction-weighted estimator of Sun and Abraham (2021) as our main estimation method. We describe the estimation procedure in three steps.

Step 1. Using Sun and Abraham (2021), we estimate the dynamic treatment effects on the treated 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 in year 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 equals one if district d initiates Pre-K in period e, where $e \in \{1995,2000\}$. This indicates that for every district d that provides

Pre-K in period e, we observe at least one treated and one untreated cohort.[11]

As mentioned in section 2, we exclude districts that offer Pre-K in 1994 (our first sample period), which are considered the "always-treated" districts. Hence, for individuals turning four (three) in a district "d" where Pre-K is available, the comparison group comprises of individuals who turn four (three) in the same district but before Pre-K became available (any period $l<e$), or individuals who turn four (three) in districts where Pre-K is not available before year 2000 (also referred to as the "never-treated" districts).

In all our estimations, we control for district fixed effects (α_d) to account for district time-invariant characteristics in addition to time 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 in the relevant period l, Sun and Abraham (2021) 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 the weighted average of the estimates from Step 1 with weight estimates from Step 2. The specification of the interaction-weighted estimator is

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

The validity of 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

[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.

outcomes of individuals who have no access to Pre-K. Under (2), we assume that the presence of Pre-K increases the likelihood that four-year-olds (or three-year-olds) enroll in Pre-K in their district. In the next section, 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.

5 Results

5.1 Tests of identifying assumptions

The validity of our approach relies on two main assumptions: (1) a strong first stage, i.e., the availability of Pre-K triggers enrollment for individuals who turn four (or three) of age, and (2) parallel trends. In our context, the parallel trends assumption implies that in the absence of Pre-K, the outcomes of treated cohorts would have evolved similarly to those of untreated cohorts. While we cannot directly test for parallel trends, we estimate dynamic difference-in-differences models for our outcomes of interest to provide empirical evidence that supports this assumption. We will discuss these results extensively in the next subsections (subsection 5.2, subsection 5.3), but in summary, all of our event-study graphs show no evidence of divergence in our outcomes of interest across treatment and control pre-treatment (Figure 4-Figure 8).

To show that the policy change is relevant, and that the existence of Pre-K does indeed affect Pre-K enrollment, we estimate the effect of availability of Pre-K on the likelihood of Pre-K enrollment using Equation 1. The results are reported in Figure 2, where we show the coefficient estimates that represent the impact of access to Pre-K on enrollment in Pre-K for each period. As shown in the figures, the likelihood of enrolling in Pre-K is zero in the pre-periods, consistent with how we construct the treatment variable. After t=0, across all three types of districts, we observe an increase in Pre-K enrollment of treated cohorts relative

to untreated ones.

We also estimate the impact on enrollment by eligibility status in Table 2 (Figure A1). Overall, enrollment for both eligible and ineligible children increases, but the increase is larger for eligible children. For instance, in Panel A of Table 2, column (2) and (3) show that after Pre-K becomes available for three- and four-year-olds in a district, enrollment increases by about 6 and 36 percent for ineligible and eligible children, respectively. Similarly, Panel B shows that after Pre-K becomes available in districts providing Pre-K for four-year-olds only, 14 percent and 48 percent of ineligible and eligible children enroll in Pre-K. Finally, Panel C shows that enrollment increases by 15 percent and 52 percent for ineligible and eligible children, respectively, after Pre-K becomes available for four-year-olds in the third type of districts, but enrollment remains steady even after accessibility expanded to include three-year-olds.

We also examine whether access to public Pre-K causes non-random attrition between individuals who have access to Pre-K at age four (or three) and those who do not, which could potentially cause selection bias. For instance, if access to Pre-K improves short-term outcomes, some parents may respond by moving their children to private schools, in which case subsequent outcomes would be unobserved in our data, which cover only students enrolled in Texas public schools. 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. Ever exiting the Texas Public School System is defined as equal to 1 if an individual exits 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.

5.2 Effect on labor market outcomes

We begin by examining the impact of access to Pre-K on labor market outcomes. We do so separately for each type of district. As a reminder, where Pre-K is available for individuals who are three-years-old, treatment is defined as turning 3 in a district where Pre-K is available. To observe the outcomes of interest (earnings between 23-27) for all individuals, our sample is individuals who attend KG between 1995-2002.[12] As described in section 3, earnings are observed for individuals who are employed within the state. Hence, we first estimate the effect on annual earnings where we replace missing earnings with zero. In addition, we estimate the effect on the likelihood of having positive earnings and on non-zero earnings at each age.

We begin by estimating the event-study models using Equation 1, where the outcome variable is annual earnings at age 26. We focus on earnings at age 26 since they are the latest observed earnings for all of the cohorts in our sample. The results are shown in Figure 3, 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. 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 as those who turn four (or three) in a district where Pre-K is unavailable.

Second, in districts providing Pre-K for 3+ year-olds, point estimates are positive for some treated cohorts, but none are statistically distinguishable from zero (Figure 3a). Similarly, Figure 3b and Figure 3c show no effect of access to Pre-K on individuals who turn 4 (or 3) in districts where Pre-K is available. The event-study graphs for earnings at ages 25, 26 and 27 show a similar pattern Figure A3. Additionally, we show the event-study graphs where

[12]Earnings at age 27 are not observed for the youngest cohort, those who are 3 years-old in 2000, since our data end in 2023, but the results are consistent for all other ages.

the outcome variables are the likelihood of having positive earnings at age 26 (Figure 4) and total non-zero earnings at age 26 (Figure 5). Both sets of graphs show no evidence of diverging pre-trends and no evidence of an effect of access to Pre-K on the likelihood of having positive earnings or on positive earnings at age 26. Again, the event study graphs for the outcomes of interests at ages 25, 26, and 27 show a similar pattern (Figure A2 and Figure A4).

We then estimate the average treatment effects (ATE) of access to Pre-K on earnings at ages 23-27 using Equation 3, and we report the results in Table 3, Table 4, and Table 5 for districts that provide Pre-K for 3+ year-olds, districts that provide Pre-K for 4 year-olds only, and districts that provide Pre-K for 4 year-olds first and then 3 year-olds, respectively. For the third type of districts, we estimate the effect of Pre-K availability for 4 year-olds only and the effect of expanding Pre-K to 3 year-olds separately as shown in the table.

Beginning with the first type of districts, those that provide Pre-K for both age groups at the same time, the results are consistent with the patterns we observe in the event-study graphs. Specifically, column (4) of Table 3 shows that access to Pre-K has no impact on annual earnings (Panel A), the likelihood of having positive earnings (Panel B), or on positive earnings at age 26 (Panel C). However, column (1) shows that access to Pre-K in these districts has positive impacts at age 23. For instance, Panel A shows that access to Pre-K increases annual earnings by about $990, which is a 5 percent increase relative to the outcome mean ($18,776), and this effect is statistically significant at the 1 percent level. Additionally, Panel B shows that the likelihood of having positive earnings increases by about 0.02 percentage points (3 percent) as a result of access to Pre-K. Conditional on having positive earnings, earnings increase by about $811, which is a 3 percent increase relative to the control mean (Panel C).

However, the increase in earnings does not persist beyond age 23. As columns (2)-(5) show, the estimated effects are no longer statistically distinguishable from zero at later ages. For example, if we look at average annual earnings between ages 25 and 27 (column (6)), we

see statistically insignificant effects of access to Pre-K. Importantly, we are able to rule out any increases in positive earnings that are larger than 5 percent.[13] Similarly, access to Pre-K does not affect the likelihood of having positive earnings (Panel B) nor positive earnings between ages 25-27 (Panel C).

Next, we examine the impact of access to Pre-K at age 4 on earnings. Consistent with the evidence in the event-study graphs, the results in Table 4 show that access to Pre-K in districts providing Pre-K for four-year-olds only has no statistically significant effects on annual earnings (Panel A), the likelihood of having positive earnings (Panel B), nor on positive earnings (Panel C) at any age between 23 and 27. This is also true for the third type of districts, those that provide Pre-K for sequentially for four-year-olds then three-year-olds (Table 5). Again, our estimates are precise enough to rule out economically meaningful changes in our outcomes of interest. For instance, Table 4 column (6) in Panel A shows that we can reject any increases in average earnings between ages 25 and 27 that are larger than 3 percent.

Overall, the results indicate that access to Pre-K at age 3 in districts serving both age groups leads to a statistically significant 5 percent increase in earnings at age 23. It also increases the likelihood of having positive earnings by about 3 percent, which can be interpreted as an increase in labor market participation at this age. Conditional on working, earnings increase by about 3 percent. This indicates that the increase in annual earnings is driven by both more people working and by higher earnings among those who work at age 23. However, this increase is not persistent at later ages. In contrast, access to Pre-K in districts that provide Pre-K for four-year-olds only and districts that provide Pre-K for four-year-olds first then three-year-olds does not have statistically significant impacts on earnings between ages 23-27, and across all our results, the coefficient estimates are precise enough to rule out even small changes in earnings.

[13]The upper 95% confidence interval relative to the control mean of $25,670 is 5 percent.

5.3 Effect on long-term educational outcomes

Next, we estimate the effect of access to Pre-K on long-term educational outcomes, including college enrollment and high-school completion. We begin by using Equation 1 to show the dynamic treatment effects for our outcomes of interest. We report the results for college enrollment in Figure A5, and we show that there is (1) no evidence of diverging pre-trends and (2) no evidence of an affect on the likelihood of college enrollment in all three district types. Similarly, Figure 7 shows that across all three types of districts, there is no evidence in diverging pre-trends for the probability of ever graduating high school and the probability of graduating high school on time (13 years after Pre-K). However, Figure 7a shows that for districts that provide Pre-K for 3 and 4 year-olds at the same time, the likelihood of ever graduating high school and on time graduation increases for those who turn 3 after Pre-K becomes available in their district.

We formally estimate the average treatment effects using Equation 3, and we report the coefficient estimates along with the standard errors in Table 6. For districts that provide Pre-K for four-year-olds before providing it for three-year-olds, we estimate the effect of access at four and the effect of access at both ages, separately (Panel C).

In line with the event-study graphs, the results in this table show that access to Pre-K at age 3 in districts that provide Pre-K for both age groups simultaneously increases the likelihood of graduating high school by about 2 percentage points, and the coefficient is statistically significant at the 1 percent level. Relative to the outcome mean of 0.73, this means that access to Pre-K at age 3 increases the likelihood of high school graduation by about 3 percent. The results also show that on time graduation improves: the likelihood of graduating high-school on time increase by 3 percentage points (3 percent relative to the outcome mean), and the coefficient is statistically significant at the 1 percent level.

However, for districts that provide Pre-K for four-year-olds only and for four- and three-year olds at separately, access to Pre-K does not affect the likelihood of high-school graduation, as expected given the event-study graphs. The coefficients are precisely estimated and

economically small. For instance, column (1) of Panel B shows that we can reject increases (decreases) in the likelihood ever graduating high school that are larger than 2 percent (3 percent) relative to the control mean.[14] In fact, columns (1) and (2) in Panel C show that in districts where Pre-K is available for 4 then 3-year-olds, availability for 4-year-olds initially decrease the likelihood of high school graduation and the likelihood of on time high school graduation, but the effect sizes are small and marginally significant, and they do not translate into subsequent negative effects on other longer-term educational or labor market outcomes.

Additionally, across all three panels, we show that access to Pre-K, whether at age 3 or age 4, has no statistically significant effects on the likelihood of any college enrollment (Column (3)), enrollment in community college (Column (4)), or enrollment in a public university (Column (5)), nor does it an effect on the timing of college enrollment (Column (6)). Importantly, the estimates are precise enough that we can reject relatively small changes. For instance, column (3)) in Panel A shows that we can reject increases in college enrollment that are larger than 4 percent.[15]

5.4 Effect on intermediate outcomes

We use 3rd grade test scores, grade retention, and a number of behavioral outcomes to examine the effect of Pre-K access on intermediate outcomes. Our findings show that access to Pre-K of any type does not improve 3rd grade reading and math test scores (Columns (7) and (8) in Table 6. The event study graphs (Figure 8) corroborate these findings and show that there is no divergence in the pre-trends across treatment and control districts. In fact, the graphs suggest that there may be a decrease in reading scores for the first two treated cohorts in districts where Pre-K is available for 3+ years-old and four-year-olds only, but the coefficients are imprecisely estimated. Additionally, the results are consistent when using

[14]The upper and lower 95% confidence intervals are +0.02 and -0.03, respectively, relative to the control mean of 0.733.

[15]The 95% upper confidence interval is 4 percent relative to the control mean of 0.65.

fourth-grade test scores instead, as shown in Figure A6. Similarly, access to Pre-K has no effect on the frequency of grade retention between K-12 (Table 7).

We also find no effect of access to Pre-K on absenteeism, or the number of absent days (Column (4) of Table 7), the number of years an individual received special education (Column (5)), disability services (Column (6)), medical services (Column (7)), or therapy services (Column (8)). While we find no impact as well on the number of disciplinary incidents between KG and 12th grade, we find that access to Pre-K at age 3 decreases the number of criminal incidents filed against an individual before age 18. Specifically, Column (2) of Panel A (Table 7) shows that access to Pre-K at age 3 decreases the number of criminal incidents before 18 by about 25 % relative to the control mean of 0.1. Importantly, this is only true for districts that provide Pre-K for individuals who turn 3 and 4 at the same time. This is consistent with previous papers that show that early childhood education can improve non-cognitive outcomes for exposed individuals (Gray-Lobe et al. (2023)). It is also consistent with our earlier results in subsection 5.3, where we show that only access to Pre-K in districts providing Pre-K for both age groups simultaneously has positive impacts on high-school graduation and early labor market outcomes.

Overall, the only consistent positive effects appear in districts that provide Pre-K for three- and four-year-olds simultaneously. In these districts, access at age three increases high school graduation, increases on-time graduation, and reduces the number of juvenile criminal incidents. Consistent with these changes, we show a 5 percent increase in earnings at age 23, although this effect does not persist into the mid-to-late twenties. Notably, these gains occur in the absence of detectable improvements in third-grade (or fourth-grade) test scores or college enrollment, suggesting that early access at age three may operate through non-cognitive and/or persistence-related channels rather than sustained changes in measured academic achievement.

In contrast, we find no evidence of economically meaningful impacts in districts that provide Pre-K for four-year-olds only or that phase in access sequentially (four-year-olds

first, then three-year-olds). Across these settings, access to Pre-K does not affect test scores, high school completion, college enrollment, earnings, or behavioral outcomes. Moreover, the estimates are sufficiently precise to rule out small improvements in these outcomes. Taken together, the results suggest that any long-run benefits of access to Pre-K in this context are concentrated among districts that provide Pre-K beginning at age three, while expansions limited to four-year-olds do not appear to generate measurable effects.

5.5 Heterogeneous effects

To assess whether access to public Pre-K has differential impacts by eligibility status, we re-estimate our main specifications (Equation 3) separately for eligible and ineligible individuals. Across district types, access to Pre-K increases public Pre-K attendance for both eligible and ineligible children, though as we have shown in section 5, the magnitude of take-up differs substantially across both groups. In particular, in all three types of districts, eligible students exhibit much larger increases in attendance than ineligible students, and these differences are statistically significant. For instance, Table 8 shows that 38 percent of eligible children attend Pre-K, compared to only 6 percent of ineligible students. Thus, eligibility status strongly predicts take-up of the program.

We report the effects by eligibility status in Table 8. Column (1) shows the estimates for the entire sample, as shown in previous tables. Columns (2) and (3) show the effects by eligibility status, while Column (4) shows the p-value where we test the statistical difference between the coefficients in Columns (2) and (3).[16] Despite the differences in attendance, we find little evidence that the impacts on educational, behavioral, or labor market outcomes differ by eligibility status. In districts offering Pre-K beginning at age three, some outcomes are statistically significant for ineligible students but not the other; however, the p-values in column (4) suggest that the effects are not statistically different across both groups. Similarly, in districts offering Pre-K for four-year-olds only or districts that phase out access

[16]The null hypothesis is H_0: $\beta_{ineligible}$-$\beta_{eligible}$=0. We do so using a regular bootstrap method with 1,000 repetitions, with standard errors clustered at the district level.

sequentially, the estimated impacts on test scores, high school completion, college enrollment, earnings, and most behavioral outcomes do not differ between eligible and ineligible students.

Overall, the heterogeneity analysis suggests that although eligible students are more likely to enroll in public Pre-K when it becomes available, the reduced-form effects on later outcomes appear broadly similar across eligible and ineligible students.

Previous research has also 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)). We test whether the effects of access to Pre-K vary across sex and race in our context. However, there are no statistically significant differences in the effects of access to Pre-K by group.

6 Discussion

The pattern of results raises a central question: why does access to public Pre-K generate largely null effects, with only modest and nonpersistent gains concentrated in districts that serve both three- and four-year-olds simultaneously? We consider three non-mutually exclusive explanations: differences in counterfactual preschool environments, differences in the timing or intensity of exposure, and differences in program quality.

Counterfactual conditions

A natural explanation is that the effects of public Pre-K depend on the counterfactual preschool environment. If children in untreated districts already have access to Head Start or private preschool, the introduction of public Pre-K may primarily induce substitution across programs rather than expand exposure to early learning. In this case, reduced-form estimates would be attenuated even if public Pre-K itself is effective. Because our analysis focuses on cohorts attending kindergarten between 1995 and 2002—a period during which alternative preschool options were widely available—this concern is particularly relevant. Accordingly, we test whether the impacts of public Pre-K differ between districts with and

without access to alternative early childhood programs.

In principle, we expect larger impacts of access to state-funded Pre-K in districts with limited access to alternative preschool programs, where its introduction represents a meaningful expansion in early childhood education. In contrast, in districts with access to Head Start or private preschool, public Pre-K is more likely to substitute for existing programs, leading to smaller estimated effects if these alternatives are of comparable quality.

While the ERC data do not indicate whether individuals are enrolled in any non-state-funded Pre-K programs, we rely on supplementary data on Head Start and private Pre-K to proxy for the counterfactual conditions at the district level. Specifically, we merge school districts with information on the presence of Head Start centers or private preschool providers within their zip codes. We then estimate the average treatment effects separately for districts with and without alternative preschool options, and report the results in Table 11–Table 13. Columns (2) and (3) present the estimated effects for districts with and without alternatives, respectively, while column (4) reports p-values testing for differences between these estimates.

Across all three tables, public Pre-K enrollment increases substantially more in districts without alternative preschool options. For example, column (3) of Table 11 shows that enrollment rises by 32 percent in districts without Head Start or private preschool, compared to only 5 percent in districts where such alternatives are available. Similar patterns emerge across other district types (Table 12 and Table 13), indicating that public Pre-K at least partially substitutes for existing programs.

Despite these large differences in take-up, long-run impacts do not vary meaningfully across these environments. This evidence suggests that substitution across preschool programs, such as Head Start and private preschool, cannot account for the predominantly null effects. Even in settings where public Pre-K represents a substantial expansion in access to early childhood education, we do not observe corresponding improvements in educational or labor market outcomes.

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-olds simultaneously, which could reflect either earlier exposure (if children attend at age three) 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 and only 9 percent of children attend Pre-K in both years. 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. Taken together, these patterns suggest that differences in exposure intensity or timing alone are unlikely to account for the heterogeneity in impacts.

Program quality and resources

Taken together, the evidence is most consistent with differences in program resources and implementation capacity across districts. While we cannot causally attribute the differences in effects across district types to differences in program quality, the patterns we document suggest that impacts emerge only in settings that are consistent with higher-resourced and potentially higher-quality implementation environments.

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 1994 (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 nearly $4,000 per child in 1995, compared to $1,300–$2,200 in other district types. These differences point to variation in the intensity and quality of program implementation across districts.

To put this into perspective, we compare the Texas Pre-K program to programs in other states (Table A3). 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) show 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.

Taken together, these results suggest that expanding access to Pre-K in low-resource settings may be insufficient to generate long-run gains, and that program effectiveness depends critically on the resources and institutional capacity with which it is implemented.

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. Exploiting variation in program availability across districts and cohorts, we find that access to Pre-K substantially increases enrollment. In particular, we show that the availability of state-funded Pre-K

increases enrollment by 14-33 percent of age-eligible children. The increase in enrollment is mainly driven by eligible children (low-income and English learners) and is higher in districts where Head Start and private preschool options are unavailable, suggesting that state-funded Pre-K is a substitute for these alternative options.

Despite the increase in enrollment, long-term impacts are modest and heterogeneous across districts. Positive effects are concentrated in districts that offer Pre-K to both three- and four-year-olds simultaneously, where access increases high school graduation, reduces juvenile crime, and raises earnings at age 23, though these gains do not persist into the late twenties. In contrast, we find largely precise null effects of access to Pre-K in other district types, including those that offer Pre-K only to four-year-olds and those that introduce Pre-K for four-year-olds before expanding access to three-year-olds. Opposite to previous studies, further analyses show that the effects do not vary by eligibility status, sex, race, or socioeconomic status.

Our findings show that access alone is not sufficient to generate meaningful long-run gains. Rather, the effectiveness of state-funded Pre-K appears to depend on the context in which it is implemented. We show that districts with positive impacts tend to be larger, slightly more urban, and better resourced, suggesting that differences in implementation capacity may play a central role in shaping outcomes. While we do not causally identify program quality, the patterns we document are consistent with a broader interpretation that resources and institutional capacity may be key determinants of program effectiveness.

In contrast to highly intensive programs such as Perry Preschool or Abecedarian, contemporary public Pre-K programs operate at scale and with substantially fewer resources. These results help reconcile the mixed evidence on modern state-funded Pre-K programs and the more uniformly positive findings from earlier, smaller-scale interventions.

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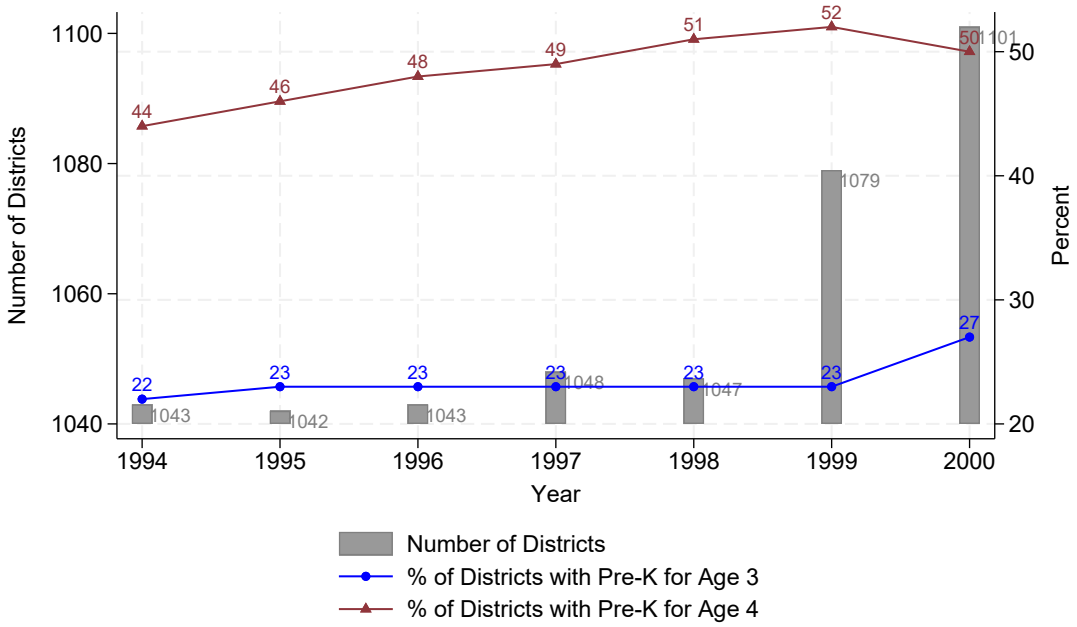
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8 Figures and Tables

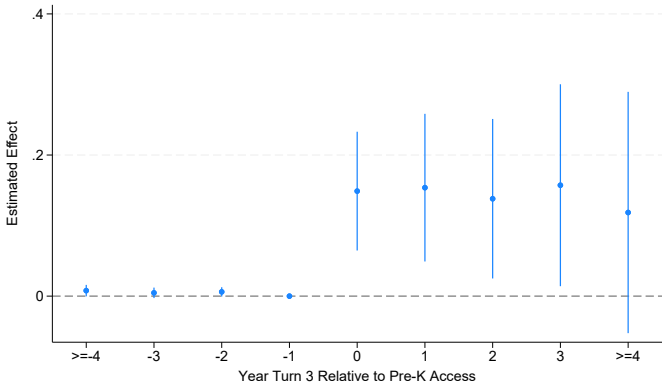
Figure 1: Pre-K Availability by Year



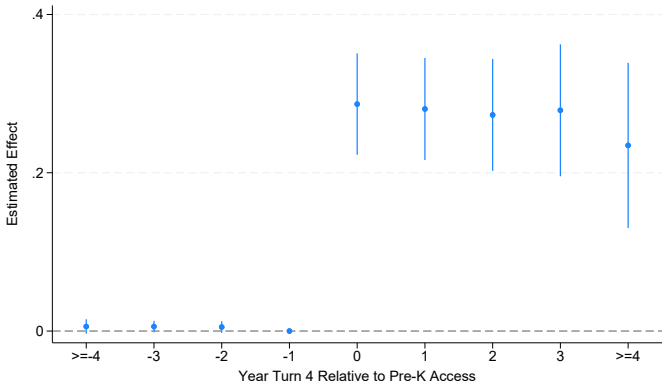
Notes: This figure shows the percentage of districts where Pre-K is available for 3 and 4-year-olds separately. The gray bars indicate the total number of districts per year. Note that our analysis excludes districts with Pre-K availability in 1994 since *when* Pre-K was initially offered in these districts is unknown.

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

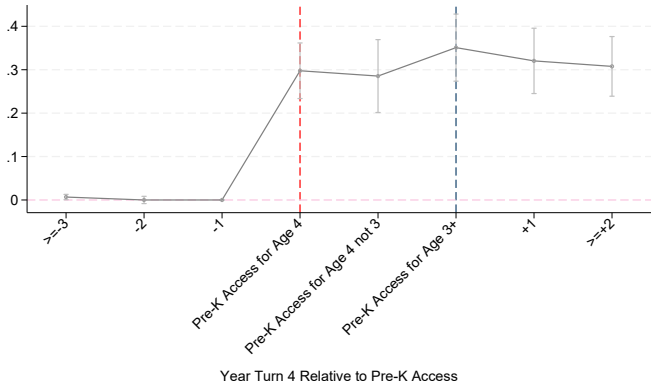
(a) Pre-K Available to 3+ Year-Olds



(b) Pre-K Available to 4 Year-Olds



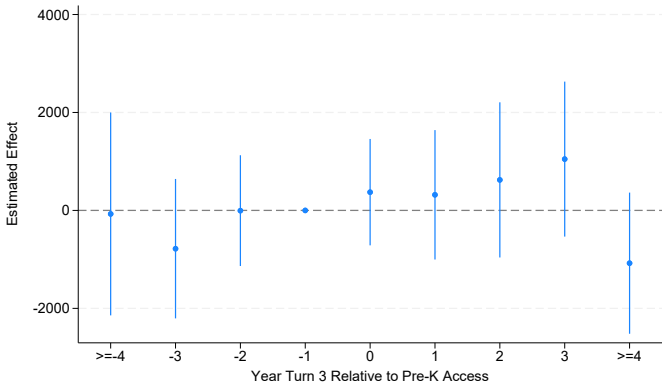
(c) Pre-K Available to 4 then 3+ Year-Olds



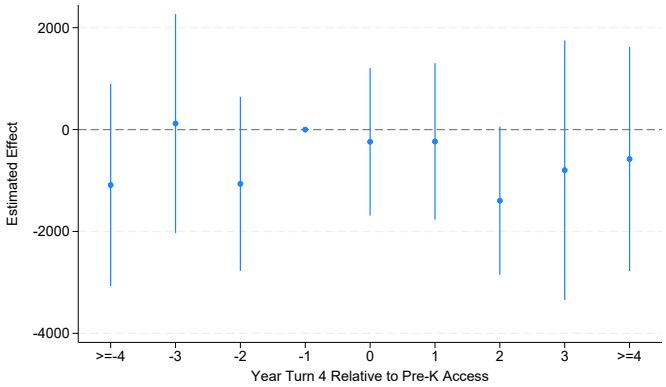
Notes: These figures show the effect of access to Pre-K on Pre-K enrollment, by treatment type, using our main estimation equation (Equation 1). Sub-figure (a) shows the effect of Pre-K availability for ages 3 and 4 on Pre-K enrollment, sub-figure (b) shows the effect of access to Pre-K at age 4 on Pre-K enrollment, and sub-figure (c) shows the effect of access to Pre-K in districts where Pre-K is available for age 4 first then age 3 on Pre-K enrollment. Enrollment is defined at any age (3 or 4). 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 3: The Effect of Access to Pre-K on Annual Earnings at Age 26

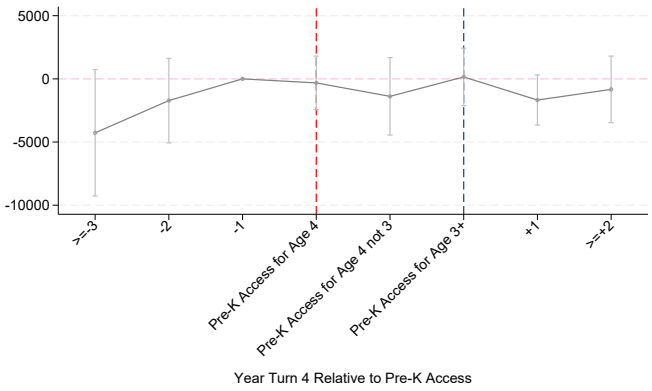
(a) Pre-K Available to 3+ Year-Olds



(b) Pre-K Available to 4 Year-Olds



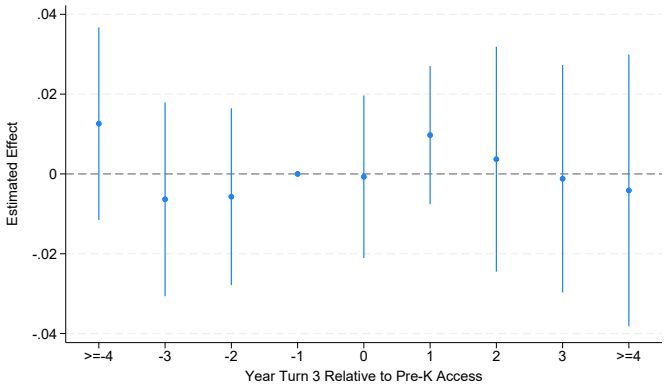
(c) Pre-K Available to 4 then 3+ Year-Olds



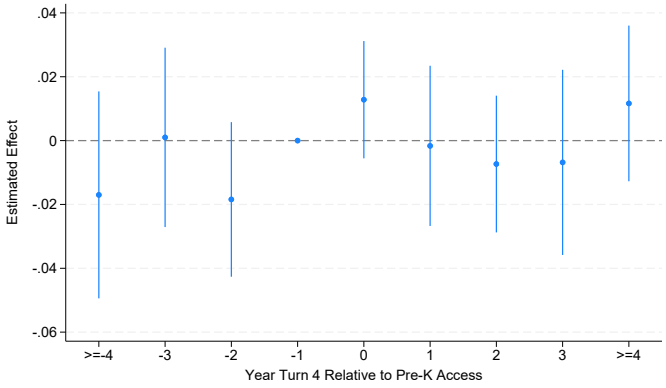
Notes: These figures show the effect of access to Pre-K on annual earnings at age 26, by treatment type, using our main estimation equation (Equation 1). Missing earnings are replaced by zero. Sub-figure (a) shows the effect of Pre-K availability for ages 3 and 4, sub-figure (b) shows the effect of access to Pre-K at age 4 only, and sub-figure (c) shows the effect of access to Pre-K in districts where Pre-K is available for age 4 first then age 3. 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 4: The Effect of Access to Pre-K on Having Positive Earnings at Age 26

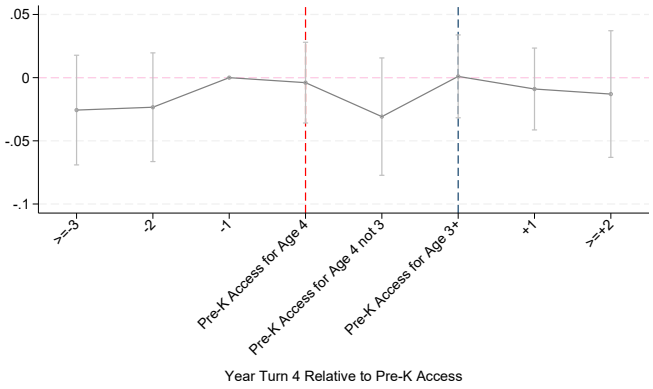
(a) Pre-K Available to 3+ Year-Olds



(b) Pre-K Available to 4 Year-Olds



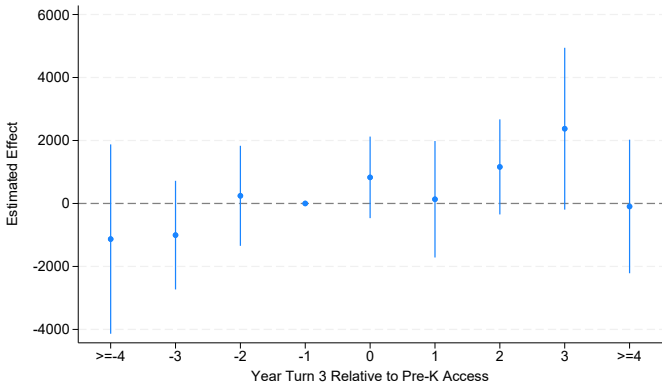
(c) Pre-K Available to 4 then 3+ Year-Olds



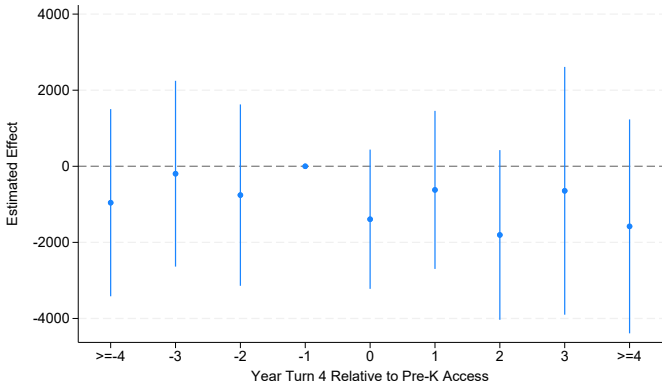
Notes: These figures show the effect of access to Pre-K on the likelihood of having non-zero earnings at age 26, by treatment type, using our main estimation equation (Equation 1). Sub-figure (a) shows the effect of Pre-K availability for ages 3 and 4, sub-figure (b) shows the effect of access to Pre-K at age 4 only, and sub-figure (c) shows the effect of access to Pre-K in districts where Pre-K is available for age 4 first then age 3. 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 5: The Effect of Access to Pre-K on Positive Earnings at Age 26

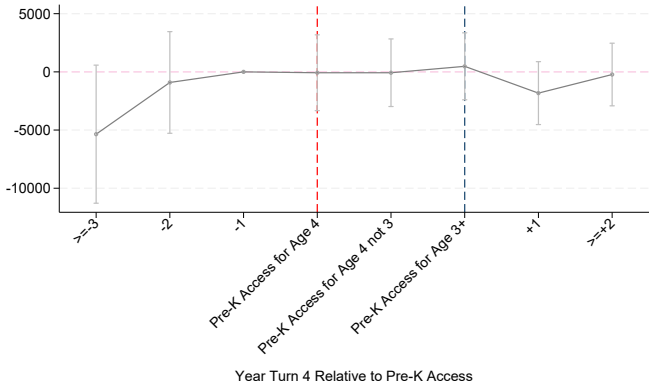
(a) Pre-K Available to 3+ Year-Olds



(b) Pre-K Available to 4 Year-Olds



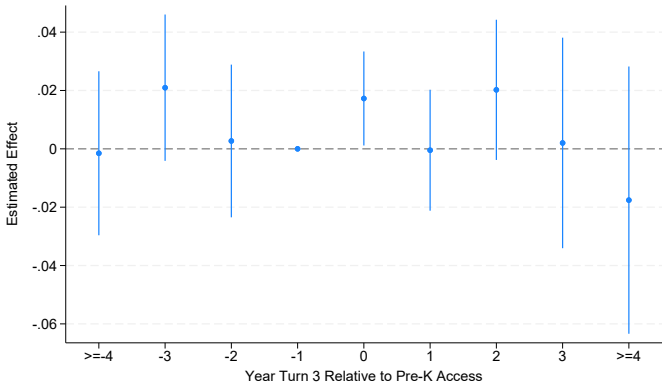
(c) Pre-K Available to 4 then 3+ Year-Olds



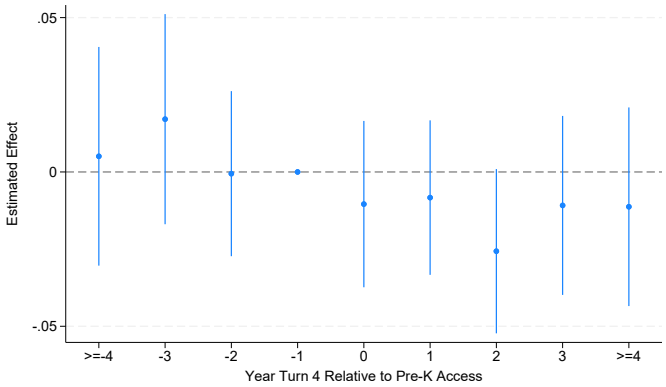
Notes: These figures show the effect of access to Pre-K on earnings, conditional on having non-zero earnings, at age 26, by treatment type, using our main estimation equation (Equation 1). Sub-figure (a) shows the effect of Pre-K availability for ages 3 and 4, sub-figure (b) shows the effect of access to Pre-K at age 4 only, and sub-figure (c) shows the effect of access to Pre-K in districts where Pre-K is available for age 4 first then age 3. 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 6: The Effect of Access to Pre-K on Any College Enrollment

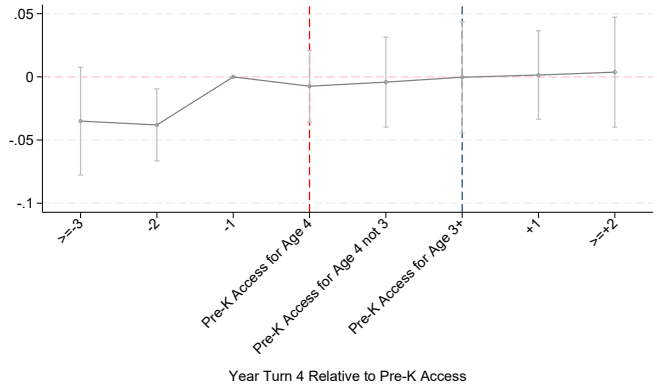
(a) Pre-K Available to 3+ Year-Olds



(b) Pre-K Available to 4 Year-Olds



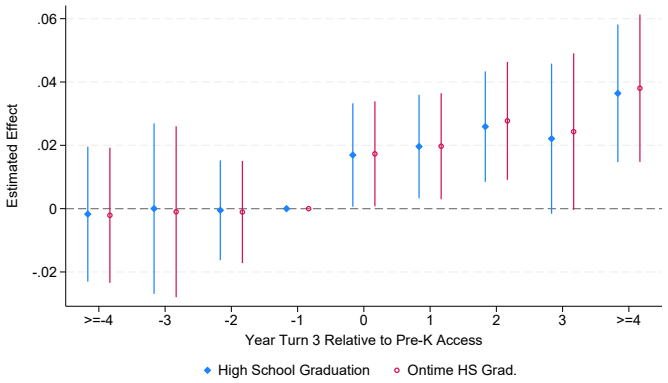
(c) Pre-K Available to 4 then 3+ Year-Olds



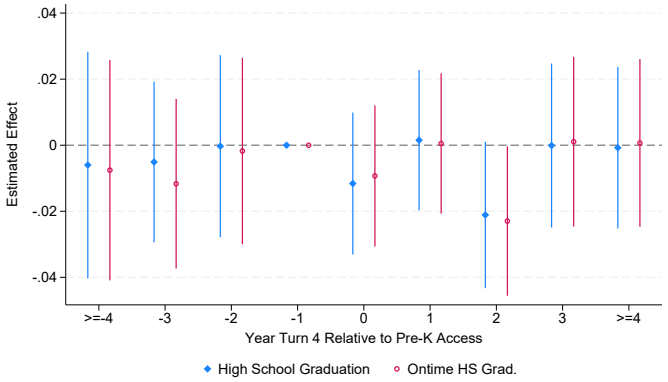
Notes: These figures show the effect of access to Pre-K on any college enrollment (enrolled in any college from 2006 to 2021), by treatment type, using our main estimation equation (Equation 1). Sub-figure (a) shows the effect of Pre-K availability for ages 3 and 4, sub-figure (b) shows the effect of access to Pre-K at age 4 only, and sub-figure (c) shows the effect of access to Pre-K in districts where Pre-K is available for age 4 first then age 3. 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 7: The Effect of Access to Pre-K on High School Graduation

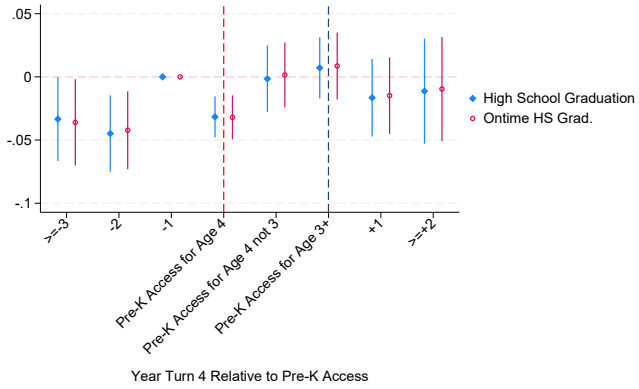
(a) Pre-K Available to 3+ Year-Olds



(b) Pre-K Available to 4 Year-Olds



(c) Pre-K Available to 4 then 3+ Year-Olds

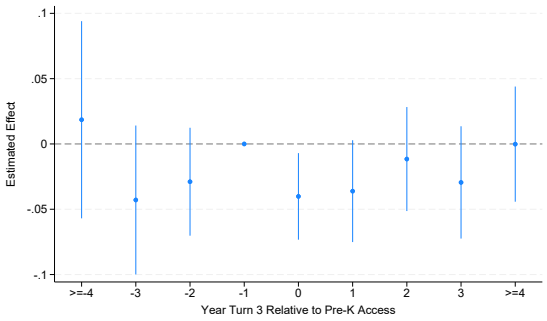


Notes: These figures show the effect of access to Pre-K on the likelihood of high-school graduation and the likelihood of on-time high-school graduation, by treatment type, using our main estimation equation (Equation 1).High school graduation refers to graduating from a Texas public school from 2006 to 2023. On-time high school graduation is defined as graduating from high school in 13 years after Kindergarten. Sub-figure (a) shows the effect of Pre-K availability for ages 3 and 4, sub-figure (b) shows the effect of access to Pre-K at age 4 only, and sub-figure (c) shows the effect of access to Pre-K in districts where Pre-K is available for age 4 first then age 3. In each figure, we report the coefficient estimates in addition to the 95% confidence intervals. Standard errors are clustered at the school district level.

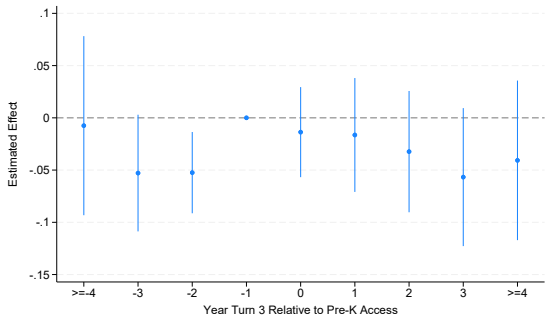
Figure 8: The Effect of Access to Pre-K on Third Grade Test Scores

Pre-K Available to 3+ Year-Olds

(a) Reading

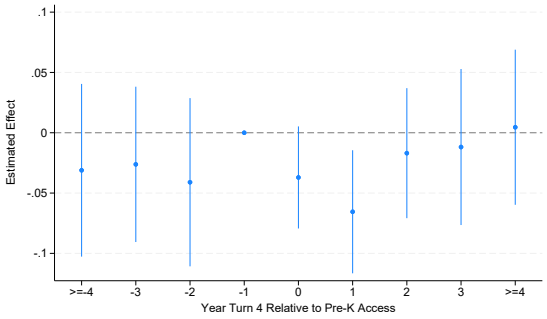


(b) Math

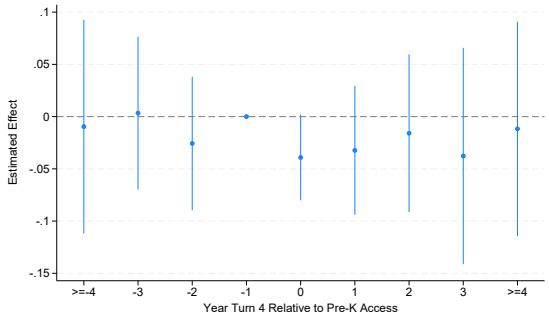


Pre-K Available to 4 Year-Olds

(c) Reading

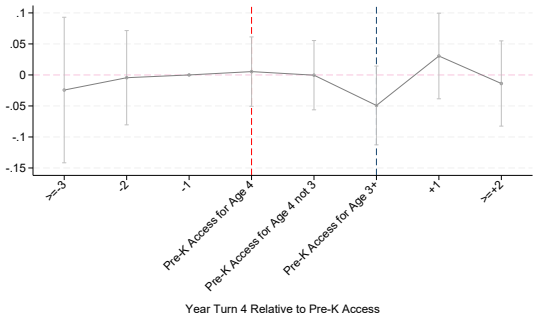


(d) Math

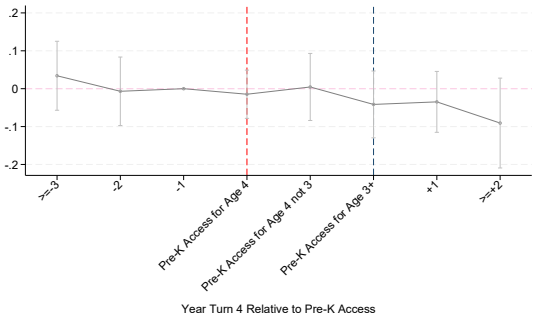


Pre-K Available to 4 then 3+ Year-Olds

(e) Reading



(f) Math



Notes: These figures show the effect of access to Pre-K on third grade test scores, by treatment type, using our main estimation equation (Equation 1). Test scores are standardized by cohort-subject such that the mean is zero and the standard deviation is one. Sub-figures (a) and (b) show the effect of Pre-K availability for ages 3 and 4, sub-figures (c) and (d) show the effect of access to Pre-K at age 4 only, and sub-figures (e) and (f) show the effect of access to Pre-K in districts where Pre-K is available for age 4 first then age 3. In each figure, we report the coefficient estimates in addition to the 95% confidence intervals. Standard errors are clustered at the school district level.

Table 1: Summary Statistics by District Type

	(1) Control	(2) Pre-K 3+	(3) Pre-K 4	(4) Pre-K 4 then 3+
District Characteristics				
Rural	0.77	0.68	0.71	0.65
Urban or Suburban	0.03	0.05	0.03	0.00
Average Pre-K funding	0	3,992	2,211	1,329
Enrollment in KG in 1994	36.43	83.16	40.82	47.77
Economically Disadvantaged in KG in 1994	0.43	0.40	0.45	0.45
Limited English Proficiency in KG in 1994	0.04	0.07	0.05	0.07
Number of Districts	148	57	95	31
Student Characteristics				
KG Age	5.000	5.000	5.000	5.000
Male	0.506	0.501	0.507	0.505
White	0.829	0.742	0.769	0.815
Hispanic	0.129	0.189	0.155	0.121
Black	0.029	0.043	0.056	0.053
Economically Disadvantaged	0.297	0.247	0.369	0.376
Special Education Participation	0.067	0.060	0.071	0.086
Limited English Proficiency	0.035	0.125	0.056	0.048
Educational Outcomes				
Grade 3 Reading Score	0.183	0.185	0.109	0.117
Grade 3 Math Score	0.192	0.207	0.108	0.097
High School Graduation	0.733	0.709	0.730	0.710
Any College Enrollment	0.648	0.615	0.624	0.604
Community College Enrollment	0.569	0.525	0.552	0.552
Public University Enrollment	0.297	0.297	0.289	0.262
On Time High School Graduation	0.727	0.705	0.724	0.707
On Time Any College Enrollment	0.581	0.566	0.559	0.556
On Time Community College Enrollment	0.483	0.468	0.471	0.501
On Time Public University Enrollment	0.199	0.223	0.197	0.205
Behavioral Outcomes				
Total No. Disciplinary Incidents	3.078	3.312	3.264	3.833
Total No. Crime	0.104	0.100	0.093	0.102
Total No. Grade Retention	0.222	0.261	0.256	0.273
Total Days Absent from School	74.613	69.698	75.465	81.171
Total Years of Special Education Participation	0.085	0.089	0.138	0.123
Total Years of Disability	1.241	1.196	1.309	1.288
Total Medical and Physical Health Service Received	0.030	0.032	0.028	0.060
Total Therapy Service Received	0.554	0.582	0.568	0.597
Earnings Outcomes				
Average Annual Earnings Ages 25-27 ($10,000)	2.567	2.391	2.509	2.489
Positive Earnings Ages 25-27 ($10,000)	3.975	3.919	3.823	3.871
Have Positive Earnings Ages 25-27	0.609	0.576	0.618	0.601
Observations	38,977	35,868	28,942	11,585

Notes: This table shows the means for district characteristics, individual characteristics, and outcomes of interest by district type. "Control" includes districts that never provide Pre-K in our sample period (between 1995 and 2000). Student characteristics, educational outcomes, behavioral outcomes, and earnings are reported for the individuals in our sample analysis (cohorts that attend KG between 1995-2002). Average Pre-K funding is the average state spending per child enrolled in 1995.

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

	(1) All Sample	(2) Ineligible for Free Pre-K	(3) Eligible for Free Pre-K
Panel A: Pre-K Available to 3+			
Pre-K 3+	0.143**	0.056**	0.375***
	(0.062)	(0.022)	(0.069)
Observations	73523	51879	21643
Panel B: Pre-K Available to 4			
Pre-K 4	0.271***	0.143***	0.480***
	(0.036)	(0.026)	(0.031)
Observations	67919	45148	22770
Panel C: Pre-K Available to 4 then 3+			
Panel C1: Access to Pre-K Available to 4			
Pre-K 4	0.292***	0.153***	0.515***
	(0.036)	(0.023)	(0.033)
Panel C2: Access to Pre-K Available to 3+			
Pre-K 3+	0.326***	0.159***	0.571***
	(0.035)	(0.024)	(0.030)
Observations	49240	33289	15950

Notes: This table shows the effect of access to Pre-K on Pre-K enrollment by treatment type using Equation 3. An individual is eligible for free Pre-K if they are economically disadvantaged or have limited English proficiency in KG, and ineligible otherwise. Note that in panel A, the summation of the number of observations in columns (2) and (3) does not equal the number of observations in column (1) because Stata dropped a singleton observation (a district with only one eligible student for free Pre-K). 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: Effect of Access to Pre-K for 3+ Year-Olds on Earnings

	(1) Age 23	(2) Age 24	(3) Age 25	(4) Age26	(5) Age 27	(6) Ages 25-27
Panel A: Annual Earnings						
Access to Pre-K for 3+	989.526***	450.577	202.000	257.111	658.156	372.423
	(298.794)	(425.229)	(548.755)	(538.847)	(517.142)	(436.150)
Observations	73523	73523	73523	73523	73523	73523
Control Mean	18776	21549	24380	27045	25595	25670
Effect Size (%)	5.3	2.1	0.8	1.0	2.6	1.5
Panel B: Have Positive Earnings						
Access to Pre-K for 3+	0.021**	0.010	0.003	0.001	0.010	0.005
	(0.008)	(0.009)	(0.011)	(0.011)	(0.009)	(0.010)
Observations	73523	73523	73523	73523	73523	73523
Control Mean	0.65	0.65	0.64	0.63	0.55	0.61
Effect Size (%)	3.2	1.5	0.5	0.2	1.8	0.8
Panel C: Positive Earnings						
Access to Pre-K for 3+	810.589*	425.764	570.688	880.216	462.468	606.815
	(425.390)	(410.778)	(530.213)	(730.732)	(1,043.257)	(584.086)
Observations	46799	46365	46009	45421	39399	50720
Control Mean	28838	33273	37961	42607	46632	39750
Effect Size (%)	2.8	1.3	1.5	2.1	1.0	1.5

Notes: This table shows the effect of access to Pre-K at age 3+ on labor market outcomes using Equation 3. Panel A shows the effect on annual earnings, where missing earnings are replaced by zero. Panel B shows the effect on the likelihood of having positive earnings, while Panel C shows the effect on earnings conditional on observing earnings (positive earnings). Each column represents these measures at a different age from 23 to 27. Column (6) reports the effect on average outcomes between ages 25-27. 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 4: Effect of Access to Pre-K for 4 Year-Olds on Earnings

	(1) Age 23	(2) Age 24	(3) Age 25	(4) Age26	(5) Age 27	(6) Ages 25-27
Panel A: Annual Earnings						
Access to Pre-K for 4	-170.316	-268.116	-295.341	-649.149	-767.580	-570.690
	(677.319)	(651.222)	(657.997)	(738.593)	(752.677)	(682.767)
Observations	67919	67919	67919	67919	67919	67919
Control Mean	18776	21549	24380	27045	25595	25670
Effect Size (%)	-0.9	-1.2	-1.2	-2.4	-3.0	-2.2
Panel B: Have Positive Earnings						
Access to Pre-K for 4	0.008	0.008	-0.000	0.002	0.000	0.001
	(0.012)	(0.011)	(0.011)	(0.009)	(0.012)	(0.009)
Observations	67919	67919	67919	67919	67919	67919
Control Mean	0.65	0.65	0.64	0.63	0.55	0.61
Effect Size (%)	1.2	1.2	0.0	0.3	0.0	0.2
Panel C: Positive Earnings						
Access to Pre-K for 4	-683.246	-900.897	-538.774	-1,208.183	-2,140.139**	-1,152.257
	(733.580)	(734.180)	(814.692)	(925.217)	(975.686)	(800.335)
Observations	44676	44271	43847	43381	37597	48520
Control Mean	28838	33273	37961	42607	46632	39750
Effect Size (%)	-2.4	-2.7	-1.4	-2.8	-4.6	-2.9

Notes: This table shows the effect of access to Pre-K at age 3+ on labor market outcomes using Equation 3. Panel A shows the effect on annual earnings, where missing earnings are replaced by zero. Panel B shows the effect on the likelihood of having positive earnings, while Panel C shows the effect on earnings conditional on observing earnings (positive earnings). Each column represents these measures at a different age from 23 to 27. Column (6) reports the effect on average outcomes between ages 25-27. 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 5: Effect of Access to Pre-K for 4 then 3+ Year-Olds on Earnings

	(1) Age 23	(2) Age 24	(3) Age 25	(4) Age 26	(5) Age 27	(6) Ages 25-27
Panel A1: Access to Pre-K for 4 on Annual Earnings						
Access to Pre-K for 4	-321.126	45.963	-638.175	-851.842	-207.542	-565.853
	(677.940)	(933.800)	(947.836)	(1,000.100)	(1,165.021)	(987.067)
Effect Sizes (%)	-1.710	0.213	-2.618	-3.150	-0.811	-2.204
Panel A2: Access to Pre-K for 3+ on Annual Earnings						
Access to Pre-K for 3+	-177.282	302.316	-588.253	-784.994	1,121.056	-84.064
	(656.871)	(885.914)	(788.866)	(834.987)	(835.666)	(730.478)
Effect Sizes (%)	-0.944	1.403	-2.413	-2.903	4.380	-0.327
Control Mean	18776	21549	24380	27045	25595	25670
Observations	49240	49240	49240	49240	49240	49240
Panel B1: Access to Pre-K for 4 on Having Any Earnings						
Access to Pre-K for 4	-0.020	-0.002	-0.016	-0.017	-0.012	-0.015
	(0.015)	(0.017)	(0.020)	(0.018)	(0.016)	(0.017)
Effect Sizes (%)	-3.077	-0.308	-2.500	-2.698	-2.182	-2.459
Panel B2: Access to Pre-K for 3+ on Having Any Earnings						
Access to Pre-K for 3+	-0.008	0.007	-0.008	-0.007	-0.003	-0.006
	(0.017)	(0.017)	(0.018)	(0.017)	(0.016)	(0.016)
Effect Sizes (%)	-1.231	1.077	-1.250	-1.111	-0.545	-0.984
Control Mean	0.65	0.65	0.64	0.63	0.55	0.61
Observations	49240	49240	49240	49240	49240	49240
Panel C1: Access to Pre-K for 4 on Positive Earnings						
Access to Pre-K for 4	433.141	318.675	-51.317	-74.683	665.338	59.926
	(770.947)	(1,173.653)	(991.933)	(1,035.074)	(1,195.992)	(866.242)
Effect Sizes (%)	1.502	0.958	-0.135	-0.175	1.427	0.151
Panel C2: Access to Pre-K for 3+ on Positive Earnings						
Access to Pre-K for 3+	57.407	223.821	-394.389	-525.527	2,270.646*	-89.401
	(752.781)	(1,131.244)	(929.734)	(1,109.090)	(1,226.695)	(834.431)
Effect Sizes (%)	0.199	0.673	-1.039	-1.233	4.869	-0.225
Control Mean	28838	33273	37961	42607	46632	39750
Observations	32070	31834	31605	31220	26910	34942

Notes: This table shows the effect of access to Pre-K at age 3+ on labor market outcomes using Equation 3. Panel A shows the effect on annual earnings, where missing earnings are replaced by zero. Panel B shows the effect on the likelihood of having positive earnings, while Panel C shows the effect on earnings conditional on observing earnings (positive earnings). Each column represents these measures at a different age from 23 to 27. Column (6) reports the effect on average outcomes between ages 25-27. 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 6: Effect of Access to Pre-K on Educational Outcomes

	(1) Ever Graduated High School	(2) On Time HS Graduation	(3) Any College	(4) Community College	(5) Public University	(6) On Time College	(7) Grade 3 Reading Score	(8) Grade 3 Math Score
Panel A: Access to Pre-K for 3+								
Pre-K 3+	0.024***	0.025***	0.004	0.004	-0.008	0.002	-0.023	-0.032
	(0.008)	(0.008)	(0.012)	(0.012)	(0.008)	(0.017)	(0.014)	(0.025)
Effect Sizes (%)	3.3	3.4	0.6	0.7	-1.2	0.3	-12.6	-16.7
Observations	73523	73523	73523	73523	73523	73523	62188	60655
Panel B: Access to Pre-K for 4								
Pre-K 4	-0.006	-0.006	-0.013	-0.012	-0.009	-0.014	-0.025	-0.027
	(0.009)	(0.009)	(0.011)	(0.011)	(0.010)	(0.009)	(0.022)	(0.032)
Effect Sizes (%)	-0.8	-0.8	-2.0	-2.1	-1.4	-2.4	-13.7	-14.1
Observations	67919	67919	67919	67919	67919	67919	59860	58299
Panel C: Pre-K available to 4 then 3+								
Panel C1: Access to Pre-K 4								
Pre-K 4	-0.017**	-0.015*	-0.006	-0.009	-0.006	-0.009	0.003	-0.005
	(0.008)	(0.008)	(0.014)	(0.013)	(0.008)	(0.012)	(0.025)	(0.036)
Effect Sizes (%)	-2.3	-2.1	-0.9	-1.6	-0.9	-1.5	1.6	-2.6
Panel C2: Access to Pre-K 3+								
Pre-K 3+	-0.007	-0.005	0.002	0.003	-0.006	-0.004	-0.011	-0.056
	(0.013)	(0.014)	(0.018)	(0.016)	(0.012)	(0.016)	(0.025)	(0.042)
Effect Sizes (%)	-1.0	-0.7	0.3	0.5	-0.9	-0.7	-6.0	-29.2
Observations	49240	49240	49240	49240	49240	49240	43436	42313
Control Mean	0.733	0.727	0.648	0.569	0.648	0.581	0.183	0.192

Notes: This table shows the effect of access to Pre-K on a set of educational outcomes using Equation 3. Panel A shows the effect of access at age 3+, Panel B shows the effect of access at age 4, while Panel C shows the effect of access at age 4 then age 3+ in districts that provide Pre-K sequentially for these two age groups. High school graduation refers to graduating from Texas public schools from 2006 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 from 2006 to 2021. On-time college is equal to 1 if an individual enrolls in college 13 years after Kindergarten, and 0 otherwise. Test scores are standardized by cohort-subject such that the mean equals 0 and the standard deviation is 1. 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 7: Effect of Access to Pre-K on Behavioral Outcomes

	(1) Total Disciplinary Incidents	(2) Total Crime	(3) Total Grade Retention	(4) Days Absent	(5) Years of Special Ed.	(6) Years of Disability	(7) Total Medical Service	(8) Total Therapy Service
Panel A: Access to Pre-K for 3+ year-olds								
Pre-K 3+	-0.150	-0.026***	-0.011	-0.733	0.014	0.011	-0.005	0.033
	(0.153)	(0.009)	(0.016)	(1.245)	(0.017)	(0.045)	(0.009)	(0.038)
Effect Sizes(%)	-4.9	-25.0	-5.0	-1.0	16.5	0.9	-16.7	6.0
Observations	73523	73523	73523	73523	73523	73523	73523	73523
Panel B: Access to Pre-K 4								
Pre-K 4	0.280	-0.007	0.018	1.591	0.013	0.097*	0.004	0.079**
	(0.173)	(0.009)	(0.014)	(1.741)	(0.021)	(0.057)	(0.010)	(0.032)
Effect Sizes(%)	9.1	-6.7	8.1	2.1	15.3	7.8	13.3	14.3
Observations	67919	67919	67919	67919	67919	67919	67919	67919
Panel C: Access to Pre-K for 4 then 3+								
Panel C1: Access to Pre-K 4								
Pre-K 4	-0.465	-0.031	-0.015	-0.444	0.002	-0.020	-0.021	-0.045
	(0.293)	(0.023)	(0.020)	(1.470)	(0.015)	(0.062)	(0.013)	(0.046)
Effect Sizes(%)	-15.1	-29.8	-6.8	-0.6	2.4	-1.6	-70.0	-8.1
Panel C2: Access to Pre-K 3+								
Pre-K 3+	-0.522	-0.017	-0.027	-1.842	-0.012	-0.106	-0.030	-0.108**
	(0.328)	(0.020)	(0.019)	(2.126)	(0.026)	(0.087)	(0.023)	(0.044)
Effect Sizes(%)	-17.0	-16.3	-12.2	-2.5	-14.1	-8.5	-100.0	-19.5
Observations	49240	49240	49240	49240	49240	49240	49240	49240
Control Mean	3.078	0.104	0.222	74.613	0.085	1.241	0.030	0.554

Notes: This table shows the effect of access to Pre-K on a set of behavioral outcomes using Equation 3. Panel A shows the effect of access at age 3+, Panel B shows the effect of access at age 4, while Panel C shows the effect of access at age 4 then age 3+ in districts that provide Pre-K sequentially for these two age groups. Columns (1)-(4) report the effect on the total number of disciplinary incidents, criminal incidents, grade retention, and days absent between K-12. Columns (5)-(6) report the effect on the total number of years an individual received special education or disability services between K-12, and columns (7) and (8) report the effect on the total number of medical services and therapy services, respectively, between K-12. 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 8: Effect of Access to Pre-K for 3+ on Ineligible vs Eligible

	(1) All	(2) Ineligible Students	(3) Eligible Students	(4) p-value
Attend Public Pre-K	0.143**	0.056**	0.375***	0.000
	(0.062)	(0.022)	(0.069)	
	73523	51879	21643	
3rd Grade Reading Scores	-0.023	-0.016	0.039	0.213
	(0.014)	(0.016)	(0.043)	
	62188	45222	16966	
High School Graduation	0.024***	0.029***	0.022	0.758
	(0.008)	(0.007)	(0.016)	
	73523	51879	21643	
Any College Enrollment	0.004	0.008	0.025	0.481
	(0.012)	(0.009)	(0.019)	
	73523	51879	21643	
Av. Earnings Ages 25-27	372.423	720.878	377.635	0.764
	(436.150)	(524.927)	(693.588)	
	73523	51879	21643	
Disciplinary Incidents	-0.150	-0.242*	-0.353	0.858
	(0.153)	(0.134)	(0.530)	
	73523	51879	21643	
Crime	-0.026***	-0.026***	-0.022	0.856
	(0.009)	(0.008)	(0.018)	
	73523	51879	21643	
Grade Retention	-0.011	-0.027***	0.003	0.454
	(0.016)	(0.008)	(0.052)	
	73523	51879	21643	

Notes: This table shows the effect of access to Pre-K at age 3+ for eligible and ineligible individuals, separately. A child is considered eligible if they are low-income or have limited English proficiency. We test the difference between the coefficients reported in columns (2) and (3) and report the corresponding p-value in column (4). 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 9: Effect of Access to Pre-K for 4 on Ineligible vs Eligible

	(1) All	(2) Ineligible Students	(3) Eligible Students	(4) p-value
Attend Public Pre-K	0.271***	0.143***	0.480***	0.000
	(0.036)	(0.026)	(0.031)	
	67919	45148	22770	
3rd Grade Reading Scores	-0.025	-0.001	-0.045	0.412
	(0.022)	(0.021)	(0.046)	
	59860	40460	19400	
High School Graduation	-0.006	-0.011	0.013	0.172
	(0.009)	(0.010)	(0.016)	
	67919	45148	22770	
Any College Enrollment	-0.013	-0.001	-0.017	0.399
	(0.011)	(0.012)	(0.016)	
	67919	45148	22770	
Av. Earnings Ages 25-27	-570.690	-156.764	-436.386	0.817
	(682.767)	(824.808)	(832.809)	
	67919	45148	22770	
Disciplinary Incidents	0.280	0.028	0.610*	0.149
	(0.173)	(0.143)	(0.355)	
	67919	45148	22770	
Crime	-0.007	0.005	-0.031	0.160
	(0.009)	(0.009)	(0.022)	
	67919	45148	22770	
Grade Retention	0.018	-0.004	0.043*	0.074
	(0.014)	(0.011)	(0.026)	
	67919	45148	22770	

Notes: This table shows the effect of access to Pre-K at age 4 for eligible and ineligible individuals, separately. A child is considered eligible if they are low-income or have limited English proficiency. We test the difference between the coefficients reported in columns (2) and (3) and report the corresponding p-value in column (4). 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 10: Effect of Access to Pre-K for 4 then 3+ on Ineligible vs Eligible

	(1) All	(2) Ineligible Students	(3) Eligible Students	(4) p-value
Access to Pre-K for 4 on Attend Pre-K	0.292***	0.153***	0.515***	0.126
	(0.036)	(0.023)	(0.033)	
Access to Pre-K for 3+ on Attend Pre-K	0.326***	0.159***	0.571***	0.000
	(0.035)	(0.024)	(0.030)	
	49240	33289	15950	
Access to Pre-K 4 on 3rd Grade Reading	0.003	0.010	0.007	0.955
	(0.025)	(0.030)	(0.052)	
Access to Pre-K 3+ on 3rd Grade Reading	-0.011	-0.001	-0.006	0.933
	(0.025)	(0.027)	(0.047)	
	43436	29775	13661	
Access to Pre-K 4 on High School Graduation	-0.017**	-0.017	-0.007	0.769
	(0.008)	(0.012)	(0.023)	
Access to Pre-K 3+ on High School Graduation	-0.007	-0.013	0.011	0.493
	(0.013)	(0.015)	(0.028)	
	49240	33289	15950	
Access to Pre-K 4 on Any College Enrollment	-0.006	-0.007	0.019	0.453
	(0.014)	(0.019)	(0.023)	
Access to Pre-K 3+ on Any College Enrollment	0.002	-0.007	0.036	0.234
	(0.018)	(0.024)	(0.024)	
	49240	33289	15950	
Access to Pre-K 4 on Av. Earnings Ages 25-27	-565.853	-981.081	1,018.992	0.287
	(987.067)	(1,048.483)	(1,519.330)	
Access to Pre-K 3+ on Av. Earnings Ages 25-27	-84.064	-612.321	2,088.685**	0.069
	(730.478)	(838.548)	(1,001.804)	
	49240	33289	15950	
Access to Pre-K 4 on Disciplinary Incidents	-0.465	-0.085	-1.317**	0.109
	(0.293)	(0.420)	(0.517)	
Access to Pre-K 3+ on Disciplinary Incidents	-0.522	-0.427	-0.760	0.664
	(0.328)	(0.376)	(0.560)	
	49240	33289	15950	
Access to Pre-K 4 on Crime	-0.031	-0.003	-0.085*	0.162
	(0.023)	(0.027)	(0.049)	
Access to Pre-K 3+ on Crime	-0.017	-0.006	-0.039	0.484
	(0.020)	(0.023)	(0.037)	
	49240	33289	15950	
Access to Pre-K 4 on Grade Retention	-0.015	-0.041***	0.014	0.232
	(0.020)	(0.016)	(0.038)	
Access to Pre-K 3+ on Grade Retention	-0.027	-0.049*	-0.003	0.388
	(0.019)	(0.025)	(0.039)	
	49240	33289	15950	

Notes: This table shows the effect of access to Pre-K at age 4 and 3+ for eligible and ineligible individuals, separately. A child is considered eligible if they are low-income or have limited English proficiency. We test the difference between the coefficients reported in columns (2) and (3) and report the corresponding p-value in column (4). 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 11: Effect of Access to Pre-K for 3+ by Alternative Options Availability

		Head Start/Private Pre-K		
	All Sample	Available	Unavailable	p-value
	(1)	(2)	(3)	(4)
Attend Public Pre-K	0.143**	0.052**	0.315***	0.017
	(0.062)	(0.024)	(0.040)	
	73523	35977	37546	
3rd Grade Reading Scores	-0.023	-0.047**	-0.015	0.511
	(0.014)	(0.020)	(0.028)	
	62188	30138	32050	
High School Graduation	0.024***	0.030***	0.028**	0.955
	(0.008)	(0.010)	(0.012)	
	73523	35977	37546	
Any College Enrollment	0.004	-0.009	0.027*	0.114
	(0.012)	(0.014)	(0.014)	
	73523	35977	37546	
Av. Earnings btw Ages 2527	372.423	997.328*	213.234	0.543
	(436.150)	(546.658)	(733.971)	
	73523	35977	37546	
Disciplinary Incidents	-0.150	-0.366**	0.055	0.381
	(0.153)	(0.170)	(0.265)	
	73523	35977	37546	
Crime	-0.026***	-0.037***	-0.025	0.684
	(0.009)	(0.013)	(0.016)	
	73523	35977	37546	
Grade Retention	-0.011	-0.036***	0.025	0.178
	(0.016)	(0.013)	(0.018)	
	73523	35977	37546	

Notes: This table shows the effect of access to Pre-K at age 3+ for districts where Head Start or private Pre-K are available and those where Head Start and private Pre-K are unavailable, separately. We test the difference between the coefficients reported in columns (2) and (3) and report the corresponding p-value in column (4). 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 12: Effect of Access to Pre-K for 4 by Alternative Options Availability

		Head Start/Private Pre-K		
	All Sample	Available	Unavailable	p-value
	(1)	(2)	(3)	(4)
Attend Public Pre-k	0.271***	0.166***	0.334***	0.086
	(0.036)	(0.033)	(0.035)	
	67919	24889	42843	
3rd Grade Reading Scores	-0.025	-0.047	-0.008	0.477
	(0.022)	(0.029)	(0.030)	
	59860	21886	37814	
High School Graduation	-0.006	-0.030***	0.007	0.066
	(0.009)	(0.010)	(0.012)	
	67919	24889	42843	
Any College Enrollment	-0.013	-0.026	-0.007	0.558
	(0.011)	(0.021)	(0.013)	
	67919	24889	42843	
Av. Earnings btw Ages 2527	-570.690	-168.896	-1,135.962	0.480
	(682.767)	(788.697)	(783.593)	
	67919	24889	42843	
Disciplinary Incidents	0.280	0.393	0.272	0.785
	(0.173)	(0.303)	(0.208)	
	67919	24889	42843	
Crime	-0.007	0.013	-0.012	0.276
	(0.009)	(0.013)	(0.012)	
	67919	24889	42843	
Grade Retention	0.018	-0.015	0.040***	0.044
	(0.014)	(0.014)	(0.013)	
	67919	24889	42843	

Notes: This table shows the effect of access to Pre-K at age 4 for districts where Head Start or private Pre-K are available and those where Head Start and private Pre-K are unavailable, separately. We test the difference between the coefficients reported in columns (2) and (3) and report the corresponding p-value in column (4). 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 13: Effect of Access to Pre-K for 4 then 3+ by Alternative Options Availability

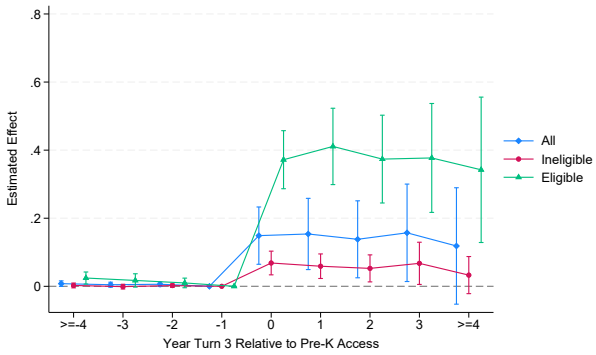
		Head Start/Private Pre-K		
	All Sample	Available	Unavailable	p-value
	(1)	(2)	(3)	(4)
Access to Pre-K for 4 on Attend Public Pre-k	0.292***	0.213***	0.344***	0.126
	(0.036)	(0.019)	(0.039)	
Access to Pre-K for 3+ on Attend Public Pre-k	0.326***	0.238***	0.394***	0.060
	(0.035)	(0.032)	(0.024)	
	49240	19589	29651	
Access to Pre-K for 4 on 3rd Grade Reading	0.003	0.035	-0.019	0.438
	(0.025)	(0.032)	(0.033)	
Access to Pre-K for 3+ on 3rd Grade Reading	-0.011	0.021	-0.031	0.404
	(0.025)	(0.032)	(0.033)	
	43436	17233	26203	
Access to Pre-K for 4 on High School Graduation	-0.017**	-0.016*	-0.019	0.912
	(0.008)	(0.008)	(0.011)	
Access to Pre-K for 3+ on High School Graduation	-0.007	-0.003	-0.012	0.839
	(0.013)	(0.022)	(0.015)	
	49240	19589	29651	
Access to Pre-K for 4 on Any College Enrollment	-0.006	0.016	-0.023	0.197
	(0.014)	(0.011)	(0.021)	
Access to Pre-K for 3+ on Any College Enrollment	0.002	0.044**	-0.029*	0.031
	(0.018)	(0.019)	(0.016)	
	49240	19589	29651	
Access to Pre-K for 4 on Av. Earnings Ages 25-27	-565.853	-417.836	-1,334.534	0.721
	(987.067)	(1,441.655)	(1,222.581)	
Access to Pre-K for 3+ on Av. Earnings Ages 25-27	-84.064	211.740	-612.359	0.711
	(730.478)	(1,248.826)	(801.095)	
	49240	19589	29651	
Access to Pre-K for 4 on Disciplinary Incidents	-0.465	-0.844***	-0.049	0.316
	(0.293)	(0.209)	(0.273)	
Access to Pre-K for 3+ on Disciplinary Incidents	-0.522	-1.458***	0.182	0.163
	(0.328)	(0.335)	(0.281)	
	49240	19589	29651	
Access to Pre-K for 4 on Crime	-0.031	-0.013	-0.039	0.605
	(0.023)	(0.034)	(0.028)	
Access to Pre-K for 3+ on Crime	-0.017	-0.031	-0.003	0.572
	(0.020)	(0.031)	(0.027)	
	49240	19589	29651	
Access to Pre-K for 4 on Grade Retention	-0.015	-0.019	-0.008	0.830
	(0.020)	(0.013)	(0.033)	
Access to Pre-K for 3+ on Grade Retention	-0.027	-0.067***	0.001	0.141
	(0.019)	(0.021)	(0.027)	
	49240	19589	29651	

Notes: This table shows the effect of access to Pre-K at age 4 and 3+ for districts where Head Start or private Pre-K are available and those where Head Start and private Pre-K are unavailable, separately. We test the difference between the coefficients reported in columns (2) and (3) and report the corresponding p-value in column (4). In all regressions, standard errors are clustered at the school district level and reported in parentheses.* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

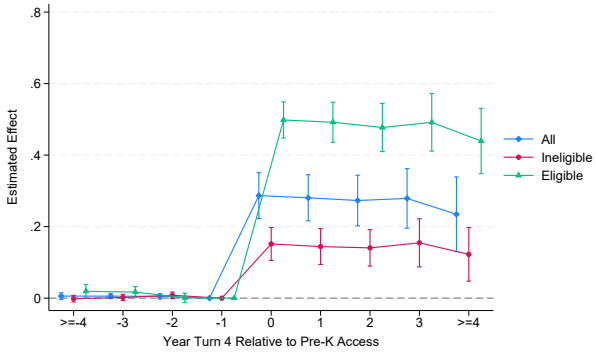
Appendix A

Figure A1: The Effect of Access to Pre-K on Pre-K Enrollment

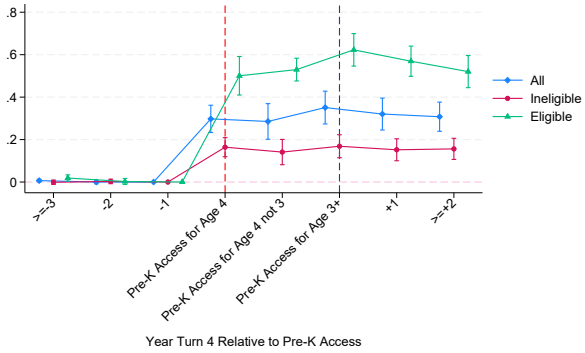
(a) Pre-K Available to 3+ Year-Olds



(b) Pre-K Available to 4 Year-Olds



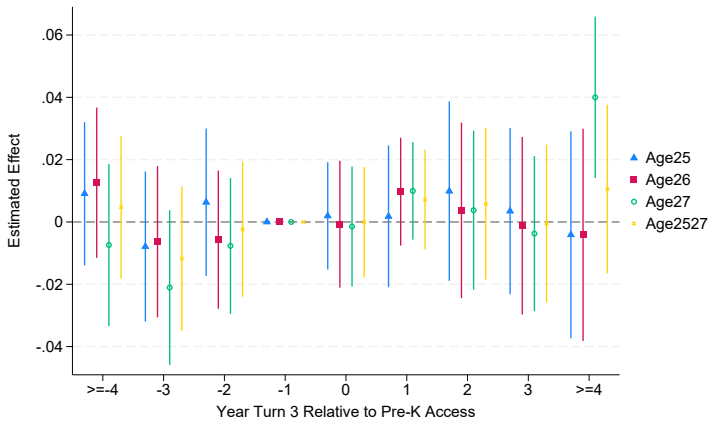
(c) Pre-K Available to 4 then 3+ Year-Olds



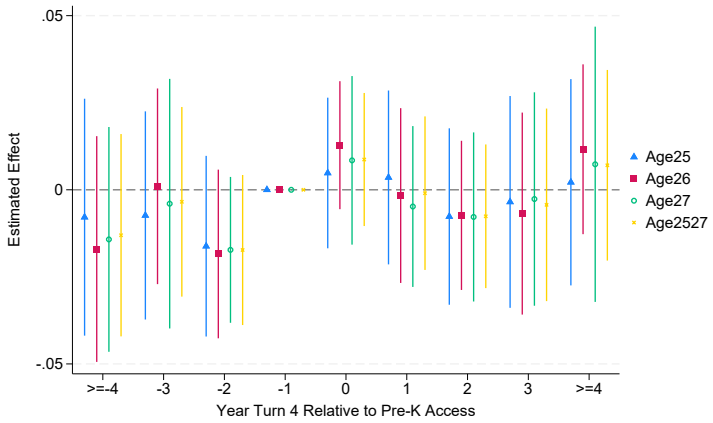
Notes: The figures show the effect of access to Pre-K on Pre-K enrollment, by the Pre-K types, using our main estimation equation (Equation 1). In each figure, we report the coefficient estimate in addition to the 95% confidence intervals. Standard errors are clustered at the school district. Subfigure (a) shows the effect of access to Pre-K that are available for 3+ year-olds on Pre-K enrollment. Subfigures (b) shows the effect of access to Pre-K that are available for 4 year-olds on Pre-K enrollment. Subfigures (c) shows the effect of access to Pre-K that are initially available for 4 year-olds then open to 3+ year-olds on Pre-K enrollment.

Figure A2: The Effect of Access to Pre-K on Having Positive Earnings

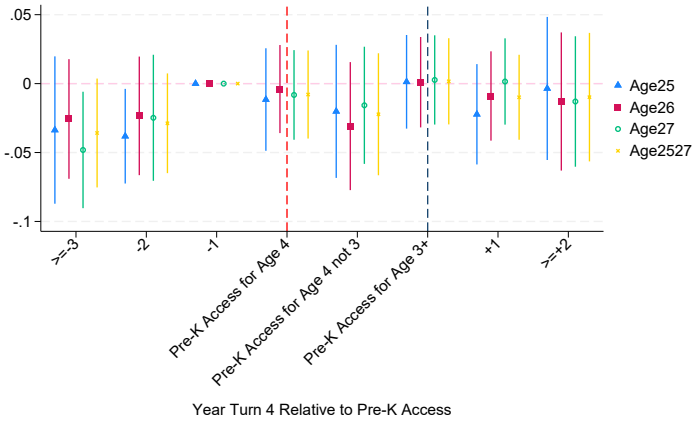
(a) Pre-K Available to 3+ Year-Olds



(b) Pre-K Available to 4 Year-Olds



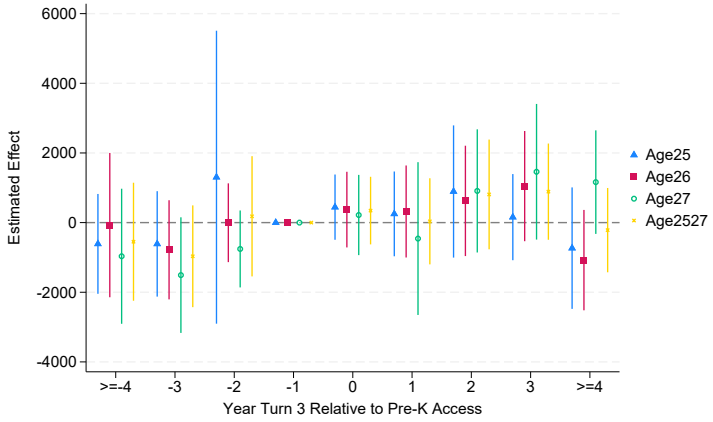
(c) Pre-K Available to 4 then 3+ Year-Olds



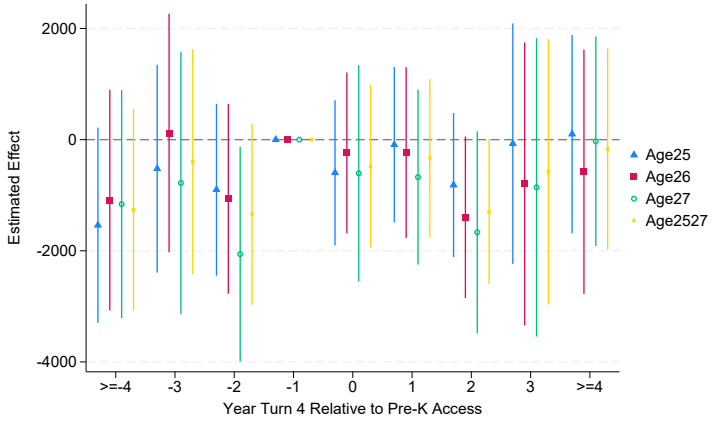
Notes: These figures show the effect of access to Pre-K on the likelihood of having positive earnings, at ages 25, 26, and 27 by treatment type, using our main estimation equation (Equation 1). Sub-figure (a) shows the effect of Pre-K availability for ages 3 and 4, sub-figure (b) shows the effect of access to Pre-K at age 4 only, and sub-figure (c) shows the effect of access to Pre-K in districts where Pre-K is available for age 4 first then age 3. 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 A3: The Effect of Access to Pre-K on Annual Earnings

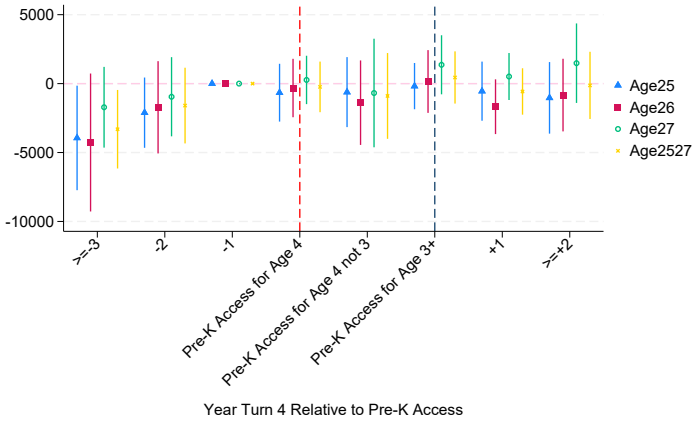
(a) Pre-K Available to 3+ Year-Olds



(b) Pre-K Available to 4 Year-Olds



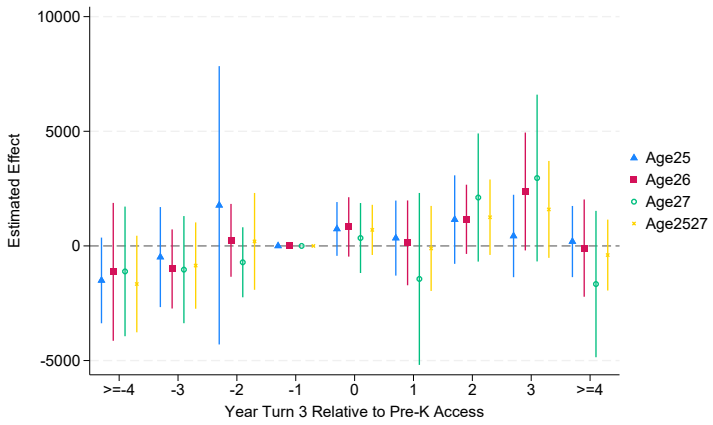
(c) Pre-K Available to 4 then 3+ Year-Olds



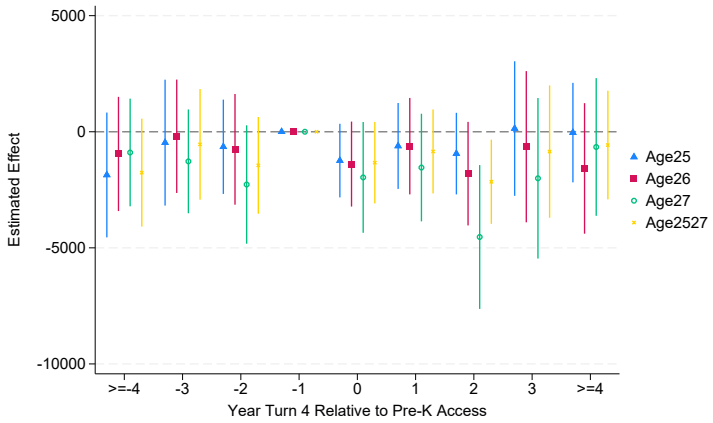
Notes: These figures show the effect of access to Pre-K on annual earnings at ages 25, 26, and 27, by treatment type, using our main estimation equation (Equation 1). Missing earnings are replaced by zero. Sub-figure (a) shows the effect of Pre-K availability for ages 3 and 4, sub-figure (b) shows the effect of access to Pre-K at age 4 only, and sub-figure (c) shows the effect of access to Pre-K in districts where Pre-K is available for age 4 first then age 3. 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 A4: The Effect of Access to Pre-K on Positive Earnings

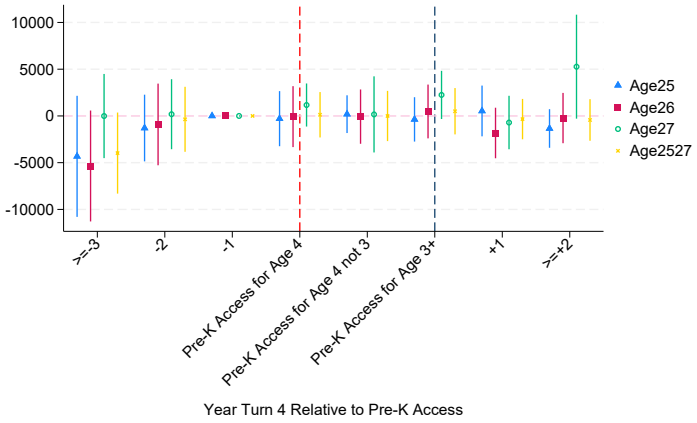
(a) Pre-K Available to 3+ Year-Olds



(b) Pre-K Available to 4 Year-Olds



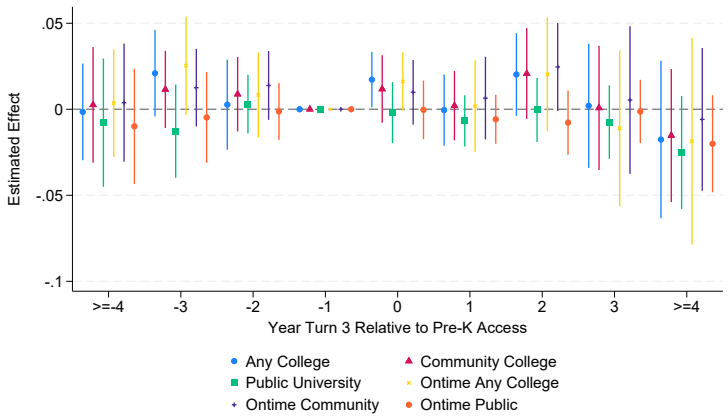
(c) Pre-K Available to 4 then 3+ Year-Olds



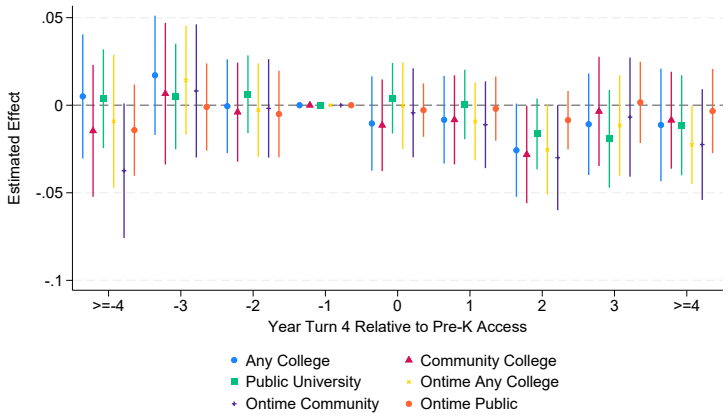
Notes: These figures show the effect of access to Pre-K on earnings, conditional on having non-zero earnings, at ages 25, 26, and 27, by treatment type, using our main estimation equation (Equation 1). Sub-figure (a) shows the effect of Pre-K availability for ages 3 and 4, sub-figure (b) shows the effect of access to Pre-K at age 4 only, and sub-figure (c) shows the effect of access to Pre-K in districts where Pre-K is available for age 4 first then age 3. 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 A5: The Effect of Access to Pre-K on College Enrollment Outcomes

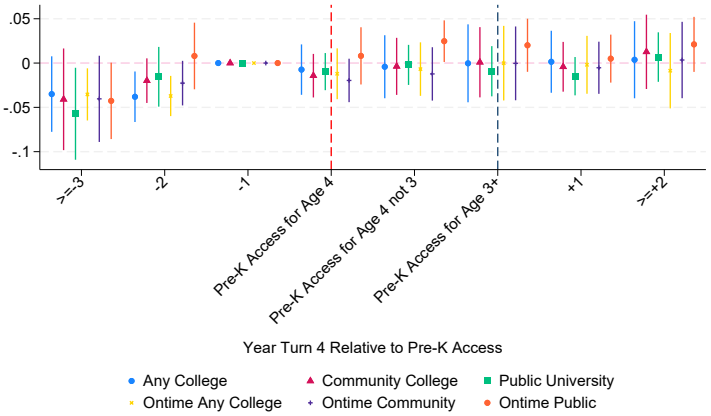
(a) Pre-K Available to 3+ Year-Olds



(b) Pre-K Available to 4 Year-Olds



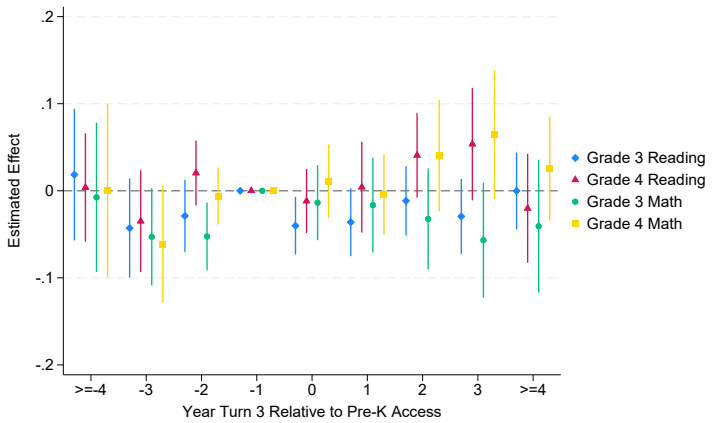
(c) Pre-K Available to 4 then 3+ Year-Olds



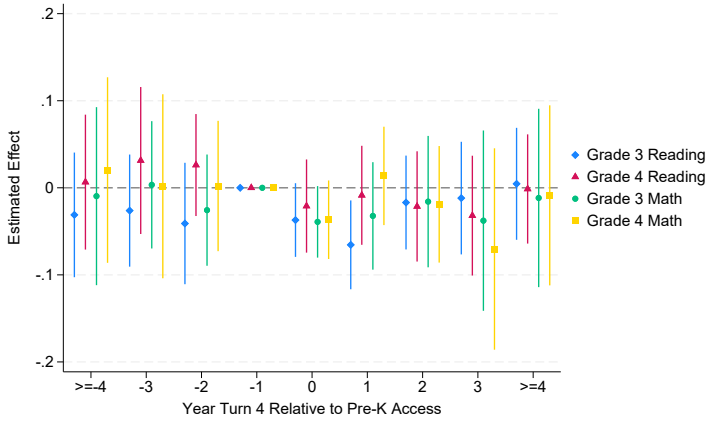
Notes: This figure shows estimated effects of accessing Pre-K on college enrollment outcomes. The sample includes potential Pre-K cohorts from 1994 to 2000. Enrollment in Any College indicates ever enrolling in a college from 2006 to 2021. On-time college enrollment indicates enrolling in any college 13 years after Kindergarten. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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

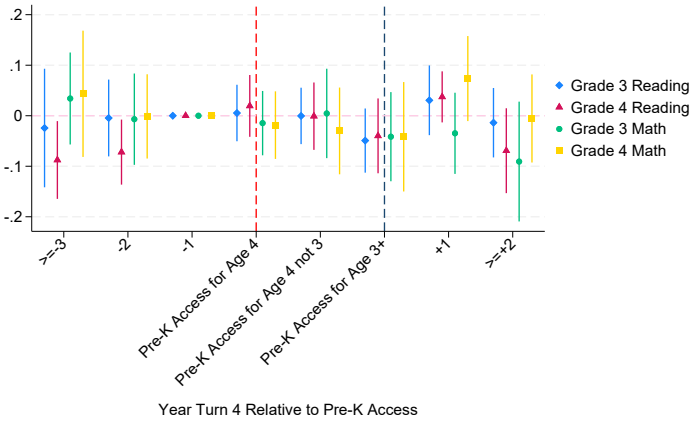
(a) Pre-K Available to 3+ Year-Olds



(b) Pre-K Available to 4 Year-Olds



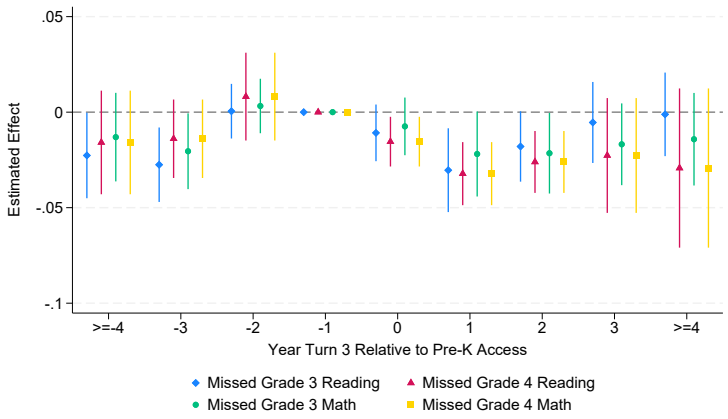
(c) Pre-K Available to 4 then 3+ Year-Olds



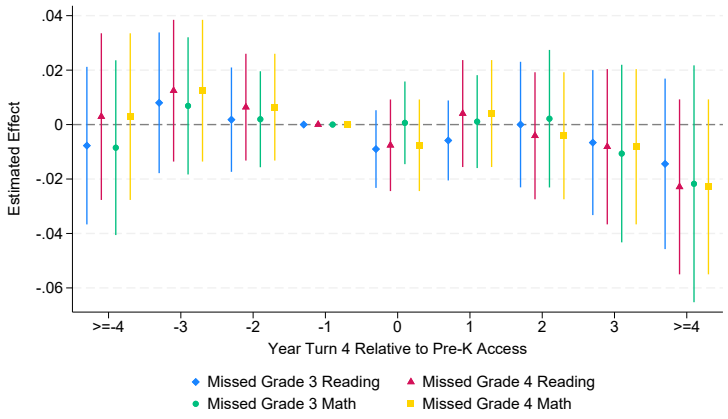
Notes: This figure shows estimated effects of accessing Pre-K on the reading and math scores outcomes by grade and Pre-K types. Third-grade students who began the 2002-2003 school year have three chances to pass the reading test under the grade retention policy. In this paper, we only use the first attempt test scores. Missing scores indicate that the other two attempts were taken or that the test was not taken at all. The reading test scores are standardized by cohort with a mean of zero and a standard deviation of one, as are math scores. The sample includes potential Pre-K cohorts from 1994 to 2000. Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A7: Effect of Access to Pre-K on Missing Test

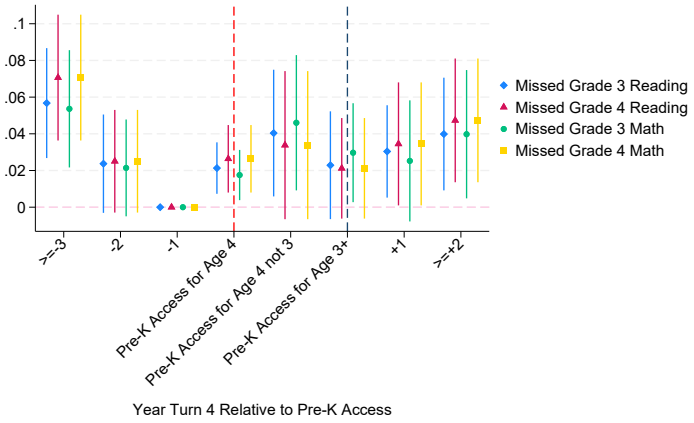
(a) Pre-K Available to 3+ Year-Olds



(b) Pre-K Available to 4 Year-Olds



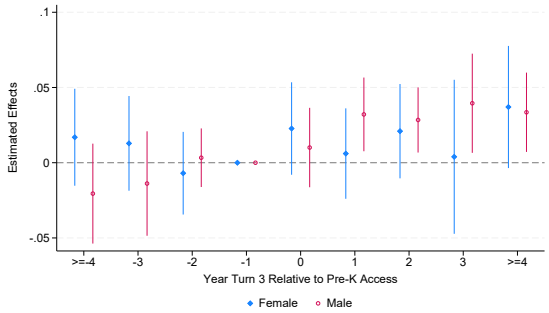
(c) Pre-K Available to 4 then 3+ Year-Olds



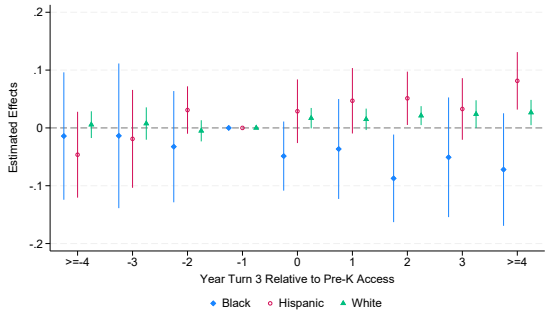
Notes: This figure shows estimated effects of accessing Pre-K on the missing reading and math tests by grade and Pre-K types. Third-grade students who began the 2002-2003 school year have three chances to pass the reading test under the grade retention policy. That said, the cohorts that attend Pre-K in 1999 and 2000 would be affected by the grade retention policy and have three chances to pass the test. For these two cohorts, we only use the first attempt test scores. Missing scores indicate that the other two attempts were taken or that the test was not taken at all. The sample includes potential Pre-K cohorts from 1994 to 2000. Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A8: Effect of Access to Pre-K on High School Graduation by Subgroups

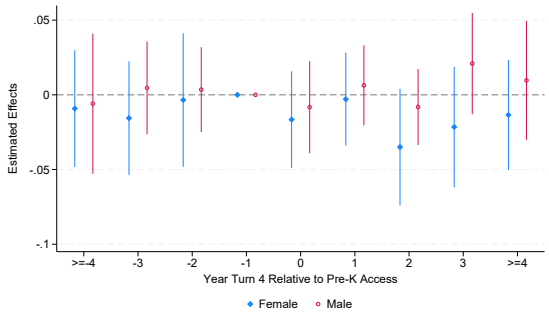
(a) Pre-K 3+: By Gender



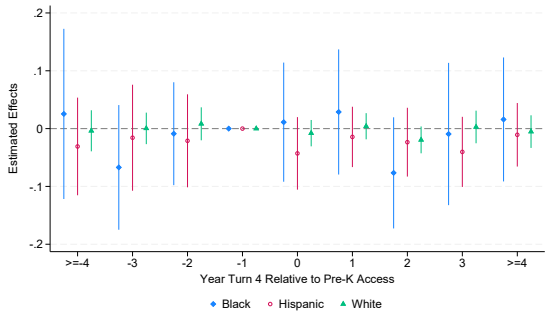
(b) Pre-K 3+: By Race



(c) Pre-K 4: By Gender



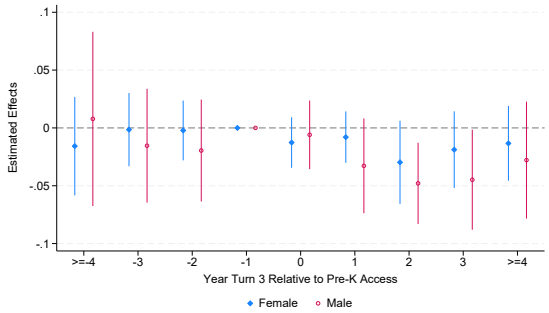
(d) Pre-K 4: By Race



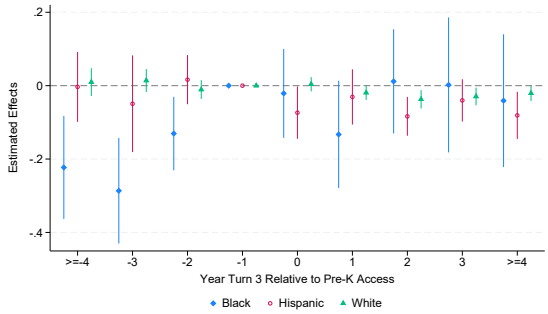
Notes: This figure shows estimated effects of accessing Pre-K on ever graduating from high school by gender and race. The sample includes potential Pre-K cohorts from 1994 to 2000. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A9: Effect of Access to Pre-K on Crime by Subgroups

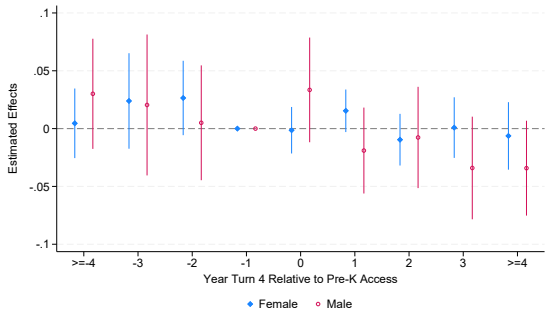
(a) Pre-K 3+: By Gender



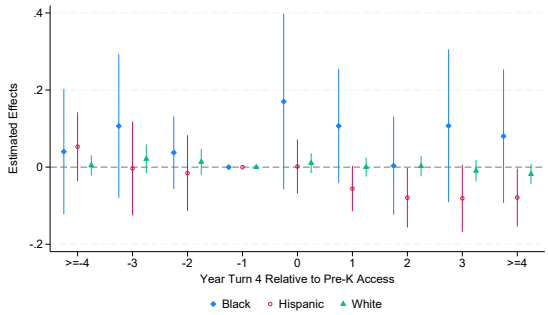
(b) Pre-K 3+: By Race



(c) Pre-K 4: By Gender



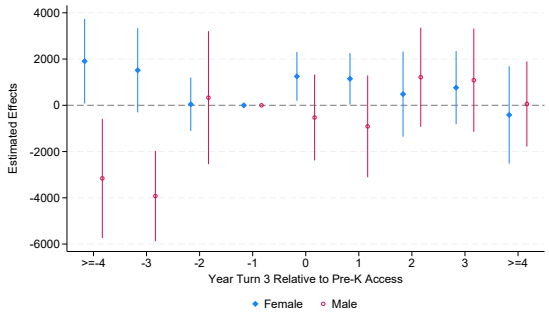
(d) Pre-K 4: By Race



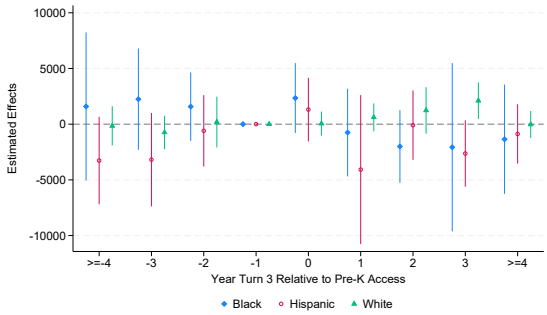
Notes: This figure shows estimated effects of accessing Pre-K on juvenile crime by gender and race. The sample includes potential Pre-K cohorts from 1994 to 2000. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A10: Effect of Access to Pre-K on Average Earnings between Ages 25 and 27 by Subgroups

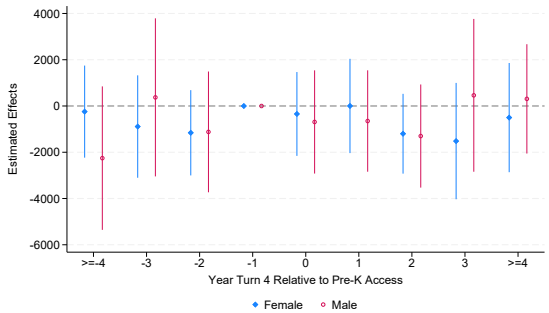
(a) Pre-K 3+: By Gender



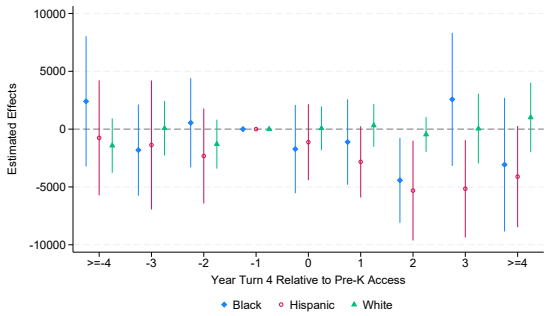
(b) Pre-K 3+: By Race



(c) Pre-K 4: By Gender



(d) Pre-K 4: By Race



Notes: This figure shows estimated effects of accessing Pre-K on average earnings between ages 25 and 27 (missing earning records are coded as zero) by gender and race. The sample includes potential Pre-K cohorts from 1994 to 2000. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A1: List of criminal incidents categories

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 and Attrition

	Ever Drop Out	Ever Exit Texas Public School System
Panel A: Pre-K for 3+		
Pre-k 3+	-0.000	0.015
	(0.003)	(0.018)
Observations	73523	73523
Panel B: Pre-K for 4		
Pre-k 4+	0.004	0.003
	(0.004)	(0.016)
Observations	67919	67919
Panel C: Pre-K for 4 then 3+		
Panel C1: Access to Pre-K Available to 4		
Pre-k 4+	0.004	-0.004
	(0.006)	(0.015)
Observations	49240	49240
Panel C2: Access to Pre-K Available to 3+		
Pre-k 3+	-0.000	-0.007
	(0.005)	(0.019)
Observations	49240	49240
Control mean	0.03	0.18

Notes: This table shows the effect of access to public Pre-K on the likelihood of ever dropping out and the likelihood of exiting the Texas Public School System. According Texas law, individuals are allowed to drop out of school at age 17 or 18, so ever dropping out is defined as equal to 1 if an individual is observed to drop out of school between 2003 and 2015. Ever exiting Texas Public School System is defined as equal to one if an individual exits between 1995 and 2015. Reasons for exiting the public system include switching to a private school, home schooling, or an out-of-state school. Standard errors are clustered at the school district level and are reported in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A3: Comparison of 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)	3,992	2,211	2,226	8,890	3,721
	Our paper	Our paper	Durkin et al. (2022)	Gray-Lobe et al. (2023)	Fitzpatrick (2008)
Math test scores	-17%	-14%	-0.119%**	-11%	+1%
Reading test scores	-13%	-14%	-0.062%	+6%	+2%
High school graduation	+3%***	-0.8%	-	+9%**	-
College enrollment	+0.6%	-2%	-	+18%***	-
Earnings (age 23)	+5%***	-0.9%	-	-	-

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. The reported per child spending in Texas is based on our data and corresponds to the 1995-1996 academic year since Pre-K funding information is unavailable for other years. 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.