

Starting Together, Diverging Later? Gender Differences in Universal Pre-K's Long-Term Effects

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February 15, 2026

Abstract

This paper uses administrative data linking enrollment in universal public pre-K at age three to school progression and high school outcomes to estimate the treatment effect of pre-K attendance, rather than the exposure (ITT) effects that dominate much of the long-run literature. To address selection into pre-K, I estimate a family fixed-effects model comparing siblings within the same household. The analysis focuses on Arab students in Israel during a period of rapid expansion in public pre-K. I show that this expansion explains most of the within-household variation in enrollment, supporting the identifying assumption. I find that attending pre-K yields significant improvements in school progression, while high school achievement remains largely unchanged. I also find that girls benefit substantially more than boys across all progression outcomes. I evaluate two mechanisms that could generate this pattern: differential developmental readiness, whereby girls mature earlier and are better positioned to benefit from the pre-K environment; and a worse counterfactual home-care environment for girls in this context, where gender norms and son preference may shape children's daily experiences and parental investments. The evidence is inconsistent with a readiness-based explanation and instead supports the home-counterfactual hypothesis.

JEL Codes: I21, I26, I28, J13, J16, J18

Keywords: Children, Preschool, UPK, Gender Gap

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1 Introduction

A broad consensus across multiple disciplines holds that shocks and interventions during infancy and early childhood can have long-lasting effects on human capital formation and later-life outcomes (Knudsen et al., 2006; Almond, Currie, and Duque, 2018). This recognition has generated substantial interest in the public provision of pre-K.¹ Empirically, however, estimating the long-run impacts of pre-K is challenging, as data that jointly observe pre-K attendance and long-run outcomes are rare. Consequently, much of what we know about the long-run impacts of pre-K comes from studies that exploit changes in program availability across time and space, yielding intention-to-treat (ITT) estimates of the effect of exposure. These reduced-form estimates are informative about the overall consequences of expanding access, but translating ITT effects into treatment effects typically requires additional information about take-up that is rarely observed directly. Moreover, heterogeneity in ITT effects conflates heterogeneity in treatment effects with heterogeneity in take-up.² These limitations matter especially when the goal is to learn about mechanisms. For example, an ITT estimate showing that poor children benefit more from exposure to a pre-K expansion could reflect either higher take-up among poor children or larger treatment effects for poor children (perhaps because their counterfactual environments are worse). Thus, without enrollment data, it is hard to distinguish between these two sources.

In this study, I provide precise *treatment* estimates of the long-term effects of attending universal public pre-K (UPK) at age three. Using comprehensive administrative data from Israel, I observe both children’s enrollment in public preschool at age three and their subsequent school progression and educational achievements upon high school completion. My identification strategy employs a family-fixed-effect model, which addresses the selection of different types of families into pre-K by comparing siblings within the same household. Family fixed-effects models are commonly used in the evaluation of preschool programs—a literature I review shortly—and have also been employed in a broad range of other settings.³

¹Additional policy motivations often include increasing female labor force participation and raising fertility rates.

²This observation is, of course, not new. For example, Havnes and Mogstad (2015) write: “A limitation of the reduced form parameters is that we cannot tell whether differential effects of the child care reform operate through differences in child care take-up or potentially heterogeneous impacts of uptake (depending on the quality of the child care center and the counterfactual form of care). Distinguishing empirically between these two sources to heterogeneity would be interesting, but requires data we do not have...” (p. 106).

³These include the long-term effects of neighborhoods and housing programs (Chetty and Hendren, 2018; Pol-lakowski et al., 2022), intergenerational consequences of teenage motherhood (Aizer, Devereux, and Salvanes, 2022),

Family-fixed-effect estimates may be biased when unobserved factors systematically influence parents’ decisions to enroll some of their children in pre-K but not others. As noted by [Deming \(2009\)](#), “*Something* is driving differences in participation among siblings.” To address this identification challenge, I leverage an expansion of public pre-K in Arab-majority municipalities in Israel during the early 2000s. In 2000, Israel launched a national initiative to implement universal free public pre-K. This reform disproportionately affected Arab communities, driving a dramatic increase in pre-K enrollment of over 30 percentage points within just six years. I provide evidence that this policy expansion is the main driver of within-family differences in pre-K attendance, mitigating concerns about systematic within-family unobserved selection that could violate the underlying identification assumption.⁴ In addition, I show that within-household variation in enrollment is uncorrelated with predetermined child characteristics (such as sex, birth order, and birthweight) and time-varying household conditions (i.e., maternal employment at age three); the estimates are robust to alternative municipality samples and model specifications; and I find no evidence that one sibling’s pre-K enrollment affects another sibling’s outcomes, which relaxes concerns about potential violations of the Stable Unit Treatment Value Assumption.

I find that starting pre-K at age three rather than four significantly improves markers of school progression. Children who attended pre-K are 1.9 percentage points more likely to start first grade on time and 2.4 percentage points more likely to graduate from high school on time. However, I find limited evidence that pre-K improves educational achievements: The effects on obtaining a high school diploma (Bagrut) and performance in key subjects such as mathematics and English are small and statistically nonsignificant.

I document important interactions between selection patterns and treatment gains. For relative age (measured by birth month relative to the school-entry cutoff),⁵ I find evidence of reverse

school peer effects ([Bertoni, Brunello, and Cappellari, 2020](#); [Figlio et al., 2024](#); [Gazze, Persico, and Spirovska, 2024](#)), and various health conditions and interventions ([Karbownik and Wray, 2025](#); [Lleras-Muney, Price, and Yue, 2022](#); [Miller, Wherry, and Aldana, 2022](#)).

⁴While a difference-in-differences strategy may appear appealing in this setting, it is not feasible for two main reasons. First, my administrative data on pre-K attendance begin only in 2000, after the expansion had already started, leaving me unable to observe pre-reform enrollment patterns. Second, the expansion unfolded gradually and continued throughout all the birth cohorts for which I observe high school outcomes. Because there are no pre- and post-treatment periods in which both pre-K attendance and later outcomes can be observed, a conventional difference-in-differences design is not appropriate. Under these constraints, the family fixed-effects strategy provides a more credible approach for leveraging these unique data to identify long-term treatment effects.

⁵The eligibility cutoff date for age-three pre-K (and other school-entry rules) was set according to the Hebrew calendar, typically falling in December ([Attar and Cohen-Zada, 2018](#)).

selection on gains: younger children benefit more from the program but are less likely to enroll. In contrast, maternal education strongly predicts attendance and is also associated with larger gains. The analysis of treatment heterogeneity by sex yields a striking finding: despite no difference in participation, girls benefit substantially and statistically significantly more than boys across all progression outcomes. For example, pre-K attendance significantly increases girls' likelihood of ever completing high school, an effect not observed in the pooled sample.

In the second part of the analysis I take a deeper look into the treatment heterogeneity across sexes and test mechanisms that could generate this pattern. I test both biological and cultural mechanisms. First, because girls develop faster than boys (especially in their social-emotional skills) (Crockenberg, 2003), I test the hypothesis that boys are less developmentally ready to benefit from pre-K, with relatively younger boys being the least ready of all. However, contrary to this hypothesis, I find that relatively younger boys (those born later in a given year) benefit significantly more than relatively older boys. Next, since I study a population in which traditional gender norms are prevalent (Yassine-Hamdan and Strate, 2020), I test the hypothesis that boys and girls have different home environments and thus different care counterfactuals to pre-K. My results are consistent with this hypothesis, as the heterogeneous treatment effect across sexes is stronger in (1) families with low maternal education, in which we expect gender norms to be stronger, and (2) families that have both boys and girls (rather than siblings of the same sex), in which we expect within-household resources to be skewed toward the preferred sex.

This paper makes two key contributions to the literature. First, it provides precise treatment-effect estimates of the long-term impacts of attending a UPK program and documents how these effects vary across key subgroups. While numerous studies document the short- and medium-term effects of public pre-K (Gormley and Gayer, 2005; Berlinski, Galiani, and Manacorda, 2008; Fitzpatrick, 2008; Berlinski, Galiani, and Gertler, 2009; Kottelenberg and Lehrer, 2017; Blanden et al., 2016; Lipsey, Farran, and Durkin, 2018; Cornelissen et al., 2018; Weiland et al., 2020; Cascio, 2023; Humphries et al., 2024), examining long-term *treatment* effects has proven much harder.⁶ Existing research has largely estimated the long-run effects of *exposure* to pre-K programs, rather than the

⁶Examining long-term outcomes is essential for two reasons: First, research on early-childhood programs frequently shows a fade-out of effects in the medium term followed by a re-emergence in adulthood (Currie and Thomas, 1995; Heckman, Pinto, and Savelyev, 2013; Bruhn and Emick, 2023). Second, standardized test scores alone might not capture the full spectrum of skills that matter for adult human capital (Jackson, 2018).

effects of actual attendance (Havnes and Mogstad, 2011; Felfe, Nollenberger, and Rodríguez-Planas, 2015; Baker, Gruber, and Milligan, 2019; DeMalach and Schlosser, 2024; Berlinski et al., 2025).^{7,8} Evidence on the long-run treatment effects of UPK participation remains comparatively limited (Gray-Lobe, Pathak, and Walters, 2022; Akee and Clark, 2024).

The setting of this paper is closely related to DeMalach and Schlosser (2024) (henceforth DS), which studies the expansion of public pre-K in Arab municipalities in Israel and, using a difference-in-differences design, estimates ITT effects of exposure to this expansion. The estimates I obtain are smaller than those in DS. One potential reason is that the ITT estimates may capture indirect spillover effects of the expansion on non-participating peers (in addition to direct treatment effects), consistent with recent work showing that public pre-K can generate broader equilibrium effects (Berne, 2025; Jackson, Turner, and Bastian, 2025). In addition, DS estimates the effect of a reform that expanded access to pre-K at ages three and four, whereas I estimate the effect of attending pre-K at age three. An age gradient in impacts is consistent with evidence from studies of childcare at ages zero to three documenting adverse effects on child development (Baker, Gruber, and Milligan, 2008; Fort, Ichino, and Zanella, 2020).

This paper is also closely related to a strand of the literature that uses family fixed effects to identify the treatment effects of public preschool programs (Currie and Thomas, 1995; Berlinski, Galiani, and Manacorda, 2008; Deming, 2009; Pages et al., 2020; Miller, Shenhav, and Grosz, 2023).⁹ To my knowledge, this is the first study to implement this approach in a UPK context using administrative data. As a result, the sample is substantially larger, yielding more precise

⁷To obtain treatment effect estimates, these studies typically apply the rationale of the Wald estimator, scaling the ITT by the reform’s first stage—often treated as known and taken from an auxiliary source. Inference on such treatment estimates is problematic and generally requires strong assumptions: Consider the Wald estimator for the treatment effect, $\widehat{TE} = \widehat{ITT}/\widehat{\pi}$, where π denotes the first stage, the effect of the reform on take-up. By the Delta Method, the treatment effect asymptotic variance is

$$\text{Var}(\widehat{TE}) \approx \pi^{-2} \text{Var}(\widehat{ITT}) + ITT^2 \pi^{-4} \text{Var}(\widehat{\pi}) - 2 ITT \pi^{-3} \text{Cov}(\widehat{ITT}, \widehat{\pi}).$$

In many applied papers, the last two terms, which capture first-stage uncertainty, are ignored. An exception is Bailey, Sun, and Timpe (2021), who explicitly incorporate first-stage uncertainty and correlation between first-stage and reduced-form estimates when converting reduced-form estimates into treatment-on-the-treated effects using a bootstrap procedure that jointly re-estimates both the first stage and the reduced form.

⁸The literature on Head Start’s long-term consequences is more advanced in terms of econometric methods and data usage (for example, Currie and Thomas 1995; Garces, Thomas, and Currie 2002; Deming 2009; Carneiro and Ginja 2014; Johnson and Jackson 2019; Ludwig and Miller 2007; Bailey, Sun, and Timpe 2021; Anders, Barr, and Smith 2023). However, Head Start is inherently different from UPK, as it is a targeted program that provides a more comprehensive set of services.

⁹Berlinski, Galiani, and Manacorda (2008) also compare sibling outcomes in the context of a preschool expansion, using within-family variation to evaluate Uruguay’s rollout of pre-primary education.

estimates and permitting a more granular analysis of treatment-effect heterogeneity. An additional advantage of using administrative records is that they do not rely on retrospective reporting of pre-K attendance, as many surveys do, which could be prone to misclassification (Cascio, 2021).

My second contribution is to shed light on the mechanisms behind gender differences in the returns to preschool. Although many studies find larger impacts for girls (Havnes and Mogstad, 2011; Cornelissen et al., 2018; Felfe, Nollenberger, and Rodríguez-Planas, 2015)—and some find the opposite (Gray-Lobe, Pathak, and Walters, 2022)—the sources of these differences remain unclear. I evaluate two classes of explanations. First, boys and girls may differ in their human-capital production functions: a large literature on the early origins of the educational gender gap emphasizes slower development of boys’ social-emotional skills (Bertrand, Kamenica, and Pan, 2015; Crockenberg, 2003; Deming and Dynarski, 2008; Reeves, 2022), which may reduce their gains from teacher interactions and unstructured curricula common in preschool (Magnuson et al., 2016; Fidjeland et al., 2023). Second, gender differences may reflect different counterfactual care environments (Kline and Walters, 2016; Kottelenberg and Lehrer, 2017): in my setting, where home care is the dominant alternative to public pre-K, gender norms and son preference may shape parental time and resource allocations (Dahl and Moretti, 2008; Blau et al., 2020; Baker and Milligan, 2016; Barcellos, Carvalho, and Lleras-Muney, 2014; Jayachandran and Pande, 2017) and, more broadly, children’s daily experiences at home (Boxberger and Reimers, 2019; Tandon, Zhou, and Christakis, 2012).¹⁰ Building on this literature, I test the hypothesis that public pre-K is especially beneficial for girls by compensating for lower investment at home and providing opportunities that are otherwise limited.

The remainder of this article is organized as follows. Section 2 provides background information on the UPK expansion and the Israeli context. Section 3 presents the data and sample construction. Section 4 outlines the empirical framework and validates its underlying assumptions. Section 5 presents the main analysis, examining how pre-K attendance at age three (versus age four) affects various educational outcomes. Section 6 explores heterogeneity in the results. Section 7 investigates potential mechanisms behind the observed gender differences in public pre-K returns. Section 8 concludes.

¹⁰These norms are particularly salient in my setting, where girls are less likely to play outdoors and more likely to participate in household chores (UNDP, 2006; Feki et al., 2017).

2 Background

Provision and expansion of public pre-K in Israel. Prior to 1999, local municipalities in Israel had the option to choose whether to provide pre-K education. These local programs were subject to regulation by the central government, which determined tuition fees, teacher requirements, and other regulations. Subsidies were available based on household income and were jointly funded by the central government and local municipalities, with subsidy rules established at the national level. This setup created an incentive for poorer municipalities, particularly those with a high proportion of low-income households—most of which were majority Arab—to refrain from opening pre-K programs (Kimhi, 2012).¹¹

Israel launched its UPK program in 1999 by amending the Free and Compulsory Education Law to cover children as young as 3 years old, expanding beyond its original K–12 scope. The law outlined a 10-year rollout by locality, empowering the minister of education to determine which areas would implement the reform. Using this authority, the minister prioritized specific municipalities and neighborhoods based on three criteria: municipalities in the bottom two deciles of an economic deprivation index, areas designated as confrontation lines, and regions classified as national priority areas. Since most Jewish localities already offered public pre-K before 1999, the pre-K expansion primarily benefited Arab children. The reform was implemented through two ministerial orders issued in 1999 and 2001, which specified the localities where the Free and Compulsory Education Law would apply to 3- and 4-year-olds. Two orders were necessary because many eligible municipalities were not initially equipped with the infrastructure needed to provide pre-K services to all children as required by law. Although the original plan was meant to be implemented nationwide, political and economic constraints delayed nationwide UPK coverage until 2015.

The introduction of UPK led to a significant increase in pre-K attendance among Arab children. Panel A of Figure 1 illustrates that from 2000 to 2005, the proportion of Arab students attending pre-K at age three doubled, from approximately 30% to about 60%. During this period, pre-K attendance among the Jewish population remained stable and high. The analysis in this paper is

¹¹Poor Jewish-majority municipalities were more likely to provide pre-K, as they received assistance from the central government through the Priority Localities program. The Supreme Court ruled in 2006 that the criteria for receiving this funding were discriminatory.

focused on children who resided in the Arab-majority localities included in the 2001 order since my data on student-level pre-K attendance start only in 2000. Panel B of Figure 1 shows that, similarly to the entire Arab population, Arab municipalities included in the 2001 order experienced a dramatic 20 percentage point increase in public pre-K attendance. Importantly, the baseline pre-K attendance is quite high (70%), as some municipalities included in the order were quicker in rolling out pre-K classrooms after the 1999 amendment.

Quality of Israeli public pre-K. In Israel, public pre-K programs operate for six hours a day, six days a week, 10 months a year. The Ministry of Education mandates that each class have a teacher and an assistant teacher. The teacher must hold a teaching certificate and, in many cases, also a bachelor’s degree (Kimhi, 2012). Additionally, the ministry sets the maximum classroom size, which, at the time of the study, was 35 students. The average class size in my sample was 32, resulting in a relatively low adult-to-child ratio of 1:16. For comparison, Norway, Germany, and Spain have adult-to-child ratios of 1:8, 1:12.5, and 1:13, respectively (Cornelissen et al., 2018; Havnes and Mogstad, 2011). While I lack data on expenditure per student for the period under study, in 2019, the estimated expenditure per student was \$4,300 (PPP), significantly lower than that of most European countries and Head Start.¹²

The Arab population in Israel. Israel’s population includes a significant Arab minority. Arab citizens of Israel, also known as Palestinian citizens of Israel,¹³ represent about 20% of all Israeli citizens and numbered 1.3 million people at the end of 2004. Arabs in Israel belong to three major religious groups: 83% are Muslims, 9% are Christians, and 8% are Druze (CBS, 2005b). The majority of the Arab population (80%) live in majority-Arab municipalities or villages (Haddad Haj-Yahya et al., 2021). There are substantial economic disparities between the Arab and Jewish populations in Israel. In 2004, income of Arab households was 36% lower than that of Jewish households (CBS, 2004). Part of this gap is driven by the lower participation of Arab women in the labor force, which in 2004 was 24% for women aged 25–64, compared to 79% for Jewish non-Ultraorthodox women (CBS, 2005a).

The low labor force participation rate of Arab women in Israel at that time was closely linked

¹²In the same year, the estimated expenditure per student in Norway and Germany was \$11,000 and \$7,700, respectively. Source: https://www.oecd.org/els/soc/PF3_1_Public_spending_on_childcare_and_early_education.pdf (accessed 11/27/2024)

¹³This refers to Arab citizens within Israel’s pre-1967 borders and does not include Palestinians residing in the West Bank or Gaza Strip.

to childcare arrangements for young children. According to a 2004 survey by the Israeli Central Bureau of Statistics (CBS), 81% of Arab children aged 0–5 were cared for at home by a parent or unpaid relative.¹⁴ This rate was significantly higher than in the Jewish population, in which only 30% of children in the same age group received such care. The disparity extended to private early-childhood programs as well: During the 2004/5 school year, just 3.1% of Arab children aged 0–6 attended private programs, compared to 9.5% of Jewish children.¹⁵ Given these patterns, the counterfactual to public pre-K attendance was most likely home care by mothers or relatives.

The Israeli education system is largely separated, so Arab children typically attend Arabic-language public schools (from pre-K through high school) with predominantly other Arab students. Instruction in these schools is primarily in Arabic, with Hebrew taught as a second language and assessed within the national matriculation system; teachers in the Arab system are overwhelmingly Arab and largely female.

High school diploma (Bagrut). In Israel, high school students are examined in a series of centrally administered matriculation exams. Each exam is associated with a number of credits (1 to 5), and to be awarded a high school diploma, students must receive a passing grade in exams collectively worth at least 20 credits. Some subjects (such as English and math) have a minimum credit requirement, and all students have to take at least the lowest-level exam in these subjects. I therefore define an advanced subject as one in which a student earns more than the minimum required credits. A Bagrut diploma is an important prerequisite for postsecondary education and a requirement for many entry-level positions in the labor market.

3 Data and Sample Construction

The data used in this study are sourced from administrative records of the Israeli Ministry of Education. I linked two types of files. First are enrollment records, which cover all students in the public education system. These records include details on each student’s grade, school identifier, and demographic characteristics such as date of birth, gender, ethnicity, immigration status, and parental education. Additionally, these records contain unique identifiers for parents, enabling

¹⁴Source: https://www.cbs.gov.il/he/publications/doclib/2004/social_survey/pdf/ty01.pdf (accessed 11/27/2024)

¹⁵Source: <https://www.cbs.gov.il/he/publications/doclib/2005/children/pdf/t07.pdf> (accessed 11/27/2024)

sibling identification. The coverage extends from the 1995/96 school year to 2018/19 for students in 1st through 12th grades, and from 1999/2000 to 2017/18 for public pre-K and kindergarten participants. The second type of file contains data on achievements on high school matriculation tests. These outcomes include an indicator for whether a diploma was awarded, total number of credits awarded, and indicators for advanced-level study in English, math, and Hebrew, which are important compulsory subjects.

I begin by constructing a census of all children born between 1996 and 2001.^{16,17} These children were aged three during school years 1999/2000 through 2004/5, coinciding with the pre-K expansion in Arab municipalities. I then restrict my sample to Arab-majority municipalities that were included in the special order issued in 2001.¹⁸ This sample restriction leaves 33 municipalities and an average of about 9,000 students per cohort, compared to an average of 32,520 Arab students per cohort in the population. Column 1 of Table 1 presents summary statistics for the entire analysis sample, while column 2 focuses on children included in the family-fixed-effect analysis—that is, children from families with at least two children in the main sample. The sibling sample is characterized by slightly larger families, lower parental education, and slightly lower academic achievements compared to the main sample.

4 Empirical Strategy

A simple comparison of students who attended pre-K with those who did not is potentially biased, as the decision to enroll a child is correlated with other significant determinants of human capital. Table 2 illustrates this. The table displays coefficients from regressing pre-K attendance on student and family-background characteristics. Column 1 presents results from univariate models, while columns 2 and 3 show results from a multivariate model that includes all the variables (column 2) and municipality-cohort fixed effects (column 3). The results reveal significant selection in who enrolls their children in public pre-K: Children of parents with higher education levels and children born earlier in the year are more likely to attend pre-K. As shown in Appendix Table A1, this

¹⁶I can observe nearly all children in these birth cohorts, as almost all children in Israel attend publicly funded schools and thus appear in my data when they enroll in first grade.

¹⁷The 1996 birth cohort is the first with available data on pre-K attendance at age three, while 2001 is the last cohort for which I can observe high school graduation outcomes.

¹⁸In the main analysis, I do not use municipalities included in the 1999 special order since my pre-K enrollment data only begin in 2000.

selection into treatment biases OLS estimates of public pre-K’s effect on educational outcomes: The pre-K coefficient decreases substantially when controls are added, suggesting substantial selection on both observable and potentially unobservable characteristics (Oster, 2019).

To overcome this selection bias, I compare siblings within the same family who attended and did not attend public pre-K by estimating a family-fixed-effect model. The family-fixed-effect approach controls for family unobserved characteristics that are time invariant. This method has been used in the preschool literature, mainly in the context of Head Start (Currie and Thomas, 1995; Garces, Thomas, and Currie, 2002; Deming, 2009). Formally, I estimate the following model:

$$y_i = \alpha + \beta PreK_i + X_i\gamma + \delta_{j(i)} + \tau_{m(i),c(i)} + \epsilon_i \quad (1)$$

Here, y_i is child i ’s educational outcome, $PreK_i$ is an indicator for whether child i attended public pre-K at age three, X_i is a vector of child i ’s background characteristics (which include sex, month of birth, and birth order), $\delta_{j(i)}$ is a family fixed effect, and $\tau_{m(i),c(i)}$ is a municipality-by-birth-cohort fixed effect.

The primary focus of this paper is β . For β to have a causal interpretation as the effect of attending pre-K, the identifying assumption requires that within a family, attendance in a public pre-K program is not correlated with a child’s unobserved characteristics. Indeed, column 4 of Table 2 shows that within-family variation in pre-K enrollment is not correlated with predetermined characteristics such as sex or birth order, though, as discussed later, it is still correlated with month of birth, which I control for flexibly. Estimates of β could be biased if, for instance, parents choose to enroll children with lower endowments in public pre-K as a compensatory measure; this could lead to an underestimate of the true effect. Conversely, if mothers who lose their jobs decide against sending their children to public pre-K, this could result in an overestimate of the true effect, as pre-K attendance will be positively correlated with household income at age 3. Considering that the program is free and demands minimal investment from parents, both scenarios are unlikely. In addition, in the next section I provide evidence that these violations are unlikely using supplemental data from the Israeli Census Bureau on maternal employment, birth weight, and enrollment (for a later period than the main analysis).

As noted by Deming (2009), “*Something* is driving differences in participation among siblings.”

In this setting, the most probable cause of within-family variation is the substantial expansion of public pre-K in Arab municipalities during the sample period. This expansion is likely to induce within-household variations in attendance that are uncorrelated with unobserved differences among siblings. To further investigate variation in public pre-K attendance within families, I adapt concepts from the local average treatment effect framework (Imbens and Angrist, 1994), with a slight abuse of terms. I categorize families into four types based on the pre-K enrollment status of the oldest and youngest child¹⁹: “never takers” (who sent none of their children to public pre-K), “always takers” (who sent all their children), “expansion compliers” (who sent their youngest but not eldest children), and “expansion defiers” (who sent their eldest but not youngest child). Following Miller, Shenhav, and Grosz (2023), I term the expansion compliers and defiers collectively as “switchers.” Importantly, only switchers help identify β (Miller, Shenhav, and Grosz, 2023).

The pattern that emerges in Table 3 is consistent with the idea that within-family differences in pre-K attendance are due to the expansion: Expansion compliers—families who, consistent with increased availability of pre-K, send only younger siblings—make up about 80% of switchers. The table also provides suggestive evidence that for a large portion of families that are expansion defiers—a group that is small—the reason for sending the oldest child but not younger children is that the younger children were born late in the year.²⁰

The switcher analysis suggests that there are two important variables to control for. First, since I mostly compare older siblings to younger ones, a cohort fixed effect is necessary; otherwise, the estimates might conflate time trends with the treatment effect. Second, since within expansion-defier families treatment status among siblings is correlated with month of birth, I add a month-of-birth fixed effect. Importantly, both sets of parameters are identified off variation among siblings in always-taker and never-taker families.

Finally, the switcher analysis reveals that the most common counterfactual to attending pre-K at age three is attending it at age four. This can be seen in the last two rows of Table 3, which display pre-K attendance rates at ages three and four, respectively, for the oldest sibling across the four family types. By definition, the share of oldest children attending pre-K at age three is

¹⁹The classification is based on the oldest and youngest siblings present in the sibling sample.

²⁰Lower enrollment rates among children born late in the year may stem from two factors: parents’ concerns about readiness, as these children would be the youngest in their class; or municipalities’ capacity constraints, which lead them to prioritize older children.

zero for never-taker and expansion-complier families, and one for always-taker and expansion-defier families. Among expansion-complier families, 73% of oldest siblings attended pre-K at age four. Therefore, my identification strategy primarily compares older children who did not attend pre-K at age three but did attend at age four with their younger siblings who started attending at age three. Thus, I interpret the results as the effect of beginning pre-K one year earlier—at age three rather than four.

5 Results

Table 4 presents results from estimating equation 1 for various outcomes. Panel A focuses on school-progression outcomes. The estimates suggest that pre-K attendance significantly enhances students' progression through the school system. Columns 1–3 show that children who attended pre-K are 1.9 percentage points more likely to start primary school on time, 1.3 percentage points less likely to repeat a grade, and 0.9 percentage points more likely to advance to 11th grade. While the second and third estimates are only marginally statistically significant, column 4 shows that all three factors contribute to a statistically significant improvement in on-time high school graduation by 2.4 percentage points. However, this effect primarily reflects better progression rather than improved educational attainment, as the impact on overall high school graduation is smaller and not statistically significant (column 5).

Panel B further indicates that pre-K plays a limited role in improving educational achievements. Column 1 shows a small and statistically nonsignificant effect on obtaining a Bagrut diploma. While column 2 suggests some improvement in the quality of the diploma, as measured by an increase in diploma credits, this increase does not stem from taking additional credits in key subjects such as mathematics, English, and Hebrew (columns 3–5).

In the main analysis in Table 4 I include in the sample expansion defiers for two reasons. First, as discussed above, the decision not to send the youngest children is plausibly exogenous, as the majority of these children are born late in the year (and I control for month of birth). Second, the division into four family types is somewhat simplistic since families may have more than two children in the sample. However, Table 5 demonstrates that my results are robust to excluding expansion defiers. The findings are also robust to several additional specification checks. First, Table 5 shows

that the results hold when imposing common cohort fixed effects across all municipalities, rather than municipality-specific cohort fixed effects. Second, the results are not sensitive to controlling for month of birth. Third, while my main analysis focuses on Arab-majority municipalities included in the 2001 special order, expanding the sample to include Arab-majority municipalities reveals similar patterns.

Finally, to address concerns about families with more than two children receiving higher weight (Miller, Shenhav, and Grosz, 2023), I estimate models using only the oldest and youngest child from each family in the family-fixed-effect sample. These estimates remain consistent with the main analysis. Keeping two siblings per family fixes family size and equalizes the variation in treatment across all switcher families—two factors that Miller, Shenhav, and Grosz (2023) show can lead to a biased estimate of the treatment effect for the target population. However, this approach has the drawback of reducing the sample size, resulting in less precise estimates. In Appendix Table A2, I implement the reweighting procedure introduced by Miller, Shenhav, and Grosz (2023). Intuitively, this method downweights families that are overrepresented in the switcher group and upweights those that more closely resemble pre-K participants. The resulting estimates closely mirror the main results, though most are somewhat larger in magnitude.

To contextualize my results, I compare my estimates to existing findings. I begin by examining estimates from large-scale pre-K programs that report treatment effects on similar outcomes and provide standard errors or confidence intervals. Panel A of Figure 2 shows that my estimates are smaller than other studies’, though they generally fall within these studies’ confidence intervals.²¹ The figure also demonstrates that my estimates are more precise than most existing estimates. The weaker effects of the Israeli expansion may stem from the program’s lower quality as reflected in its lower per-student expenditure. Notably, despite the smaller effects, the results demonstrate that even lower-quality programs can improve school-progression outcomes, leading to more on-time high school graduation and potentially earlier labor force entry.

Next, I compare my findings to estimates from DS, which also studies pre-K expansion in Arab municipalities in Israel.²² Panel B of Figure 2 shows that my estimates are smaller than those in DS

²¹A notable exception is Berlinski, Galiani, and Manacorda (2008), who find substantial improvements in school progression by age 15, the oldest age they observe, whereas I find a small and statistically insignificant effect of pre-K on the probability of ever attending 11th grade.

²²To enable appropriate comparison, I derive treatment estimates by dividing the ITT estimates by the first stage reported in DS. I construct confidence intervals by similarly scaling the implied confidence intervals from their study

for overlapping outcomes. Two differences between the estimands can account for this gap. First, if the expansion affects non-enrollees, for example through changes in peer interactions or broader impacts on the local economy (Jackson, Turner, and Bastian, 2025; Berne, 2025), then the ITT reflects both direct effects on participants and spillovers onto non-participants. In that case, the Wald ratio (ITT divided by take-up) need not identify the effect of attendance and may overstate it. Second, I estimate the effect of starting pre-K at age three rather than four, while DS studies an expansion that increased enrollment at both ages three and four (Figure 2 in DS). This gradient in effect sizes, in which impacts increase with child age, aligns with evidence from studies of public childcare for children aged zero to three that document negative impacts on child development (Baker, Gruber, and Milligan, 2008; Fort, Ichino, and Zanella, 2020). While a few studies examine pre-K at age three (Blanden et al., 2016; Cornelissen et al., 2018; Felfe, Nollenberger, and Rodríguez-Planas, 2015), the majority of the literature focuses on age four (or ages three and four combined), and little is known about how returns to pre-K at age three compare to returns at age four in the same context. Taken together, this study and DS suggest that expanding pre-K at age four yields higher returns.

5.1 Threats to Identification

The causal interpretation of β in equation 1 relies on two key assumptions: (1) within-household variation in pre-K attendance is uncorrelated with unobserved individual-level factors, and (2) there are no spillover effects on siblings from attending pre-K. While I cannot directly verify these assumptions, in this subsection I present diagnostic tests that provide supporting evidence for them.

5.1.1 Within Household Selection

If parents systematically choose to send one child but not another to pre-K based on child-specific traits or household conditions at the time the child becomes eligible for enrollment, this could introduce bias into the model estimates. In the context of early childhood, one concern is that the allocation of family resources involved in this decision may depend on children’s endowments. While such selection is plausible when pre-K attendance involves financial costs, it is less clear why parents would differentially enroll siblings when pre-K is free and requires minimal investment from

using the ITTs’ standard errors.

parents. Empirically, column 4 of Table 2 shows no association between within-household variation in pre-K enrollment and either child sex or birth order. The only within-household predictor is month of birth, which, as discussed earlier, I control for throughout.

I further provide evidence on the association between birth weight and pre-K enrollment. Birth weight is often used in the literature as a proxy for child’s endowment and is a predictor and determinant of later-life outcome (Black, Devereux, and Salvanes, 2007). Thus, such analysis can indicate whether parents selectively send one sibling and not the other to pre-K. To perform this analysis, I use supplemental de-identified individual-level records from the Israeli Central Bureau of Statistics that include child birth weight and enrollment information. These data cannot be matched to the main education administrative dataset and are available only from 2006 onward, so they do not cover the period of the main analysis sample. In Panel A of Appendix Table A4, column 2 shows that, in the cross-section, birth weight is positively correlated with pre-K enrollment. However, once I include family fixed effects in column 3, within-family differences in birth weight no longer predict differences in pre-K enrollment across siblings.

Another concern is that variation in household circumstances at the time of pre-K eligibility could generate within-family differences in enrollment. For instance, mothers who do not send their child to pre-K might reduce their labor supply, leading to differences in household resources at age three between siblings and thereby confounding the causal interpretation of the estimates. To explore this possibility, I also use the supplemental data. In Panel B of Appendix Table A4, column 2 shows that, in the cross-section, pre-K enrollment is positively associated with maternal employment. However, once I include family fixed effects in column 3, this relationship disappears. Taken together, these tests suggest that within-family differences in pre-K attendance are not systematically driven by either child endowments or maternal employment at age three.

5.1.2 Sibling Spillovers

To examine whether one sibling’s pre-K attendance affects another’s educational outcomes, I conduct two analyses. Both are based on the idea that spillover effects should increase with the degree of siblings’ exposure to one another (Black et al., 2021).

In the first analysis, I exploit the fact that siblings who are closer in age are more likely to spend time together and, consequently, may exhibit stronger spillover effects from a sibling’s pre-K

attendance. Specifically, I restrict the sample to the eldest and youngest child in each household (a sample for which I have already shown in Table 5 that the main results remain unchanged) and augment equation 1 with an interaction term between pre-K attendance and the age spacing between the two siblings.

If positive within-household spillover effects exist and diminish with age spacing, the interaction term should be positive, as the estimated effect of β would be more diluted when siblings are close in age. However, as shown in Panel A of Appendix Table A5, the estimated interaction is not statistically different from zero, whether I specify a linear interaction (column 1) or a non-linear spacing dummy (column 2). These results are, of course, only suggestive, since sibling spacing is correlated with other important family characteristics.

My second analysis reproduces a difference-in-differences model introduced in Black et al. (2021) to capture sibling spillover effects. In a nutshell, under the assumption that time spent together increases spillovers, a second-born child should be more affected than a first-born by shocks to a third-born sibling. In my setting, the relevant “shock” is whether the third-born attended pre-K.

Accordingly, I restrict my sample to families with three children, keeping only the first- and second-born, and estimate the following difference-in-differences specification:

$$y_{ij} = Family_j + Child2_i + \delta(Child2_i \times Treated_j) + \gamma X_{ij} + \varepsilon_i \quad (2)$$

where y_{ij} is an educational outcome for child i in family j , $Family_j$ denotes a family fixed effect, and $Child2_i$ is an indicator for the second-born child. $Treated_j$ is a family-level indicator for whether the third-born child attended pre-K. The coefficient of interest, δ , captures the spillover effect under the assumptions described above.

Column 1 of Panel B in Table A5 reports the estimate of δ for the analysis sample. The coefficient is small and statistically insignificant, although imprecisely estimated. To improve precision, I re-estimate equation 2 using the full sample of children from Arab-majority municipalities. The estimate remains small but becomes more precisely estimated. I conclude that sibling spillovers are unlikely to bias the estimates from the family fixed effect model.²³

²³This finding is consistent with DeMalach and Schlosser (2024), showing that an earlier pre-K expansion in northern Arab municipalities in Israel did not affect maternal labor supply, a key channel through which sibling spillovers from pre-K might operate.

6 Heterogeneous Effects

I next analyze how effects vary across student subgroups. While prior research often reveals heterogeneous impacts of public pre-K, most long-term studies can only estimate ITT effects because data on program enrollment are incomplete. By contrast, I am able to estimate treatment effects directly, providing new insights into how the impact of public pre-K varies across populations.

Table 6 reports estimates of the fixed-effects model for the school-progression outcomes for different subgroups.²⁴ Since this exercise involves cutting the sample multiple times into smaller subsamples, I report for each sample cut the p -value from a test that all differences are zero for all outcomes.

Columns 1 and 2 present results for boys and girls, respectively. The effects are consistently larger for girls: Girls who attend pre-K are more likely to start 1st grade on time than boys, are less likely to repeat a grade, and show a more pronounced increase in the probability of reaching 11th grade. These differences translate to a larger effect on girls' on-time high school graduation rates compared to boys. Notably, I find a significant effect on high school completion for girls—a finding not observed in the pooled sample. I can reject the null hypothesis of no differences across all outcomes with high statistical significance. In the next section, I explore what may explain this striking difference in treatment effects between boys and girls.

Columns 3 and 4 examine heterogeneity by relative age, comparing students born earlier versus later in the birth year.²⁵ This analysis is particularly relevant because month of birth is a strong predictor of enrollment: children born later in the year are less likely to attend public pre-K (Table 2). The point estimates suggest larger benefits for younger students, especially for starting first grade on time and graduating high school on time, and a joint test rejects equality of effects across outcomes. These patterns suggest reverse selection on gains, whereby the group that appears to benefit more (younger children) is less likely to receive treatment.

I also examine heterogeneity by maternal education and family size.²⁶ While maternal education

²⁴Table A3 shows heterogeneity analysis for educational achievements, for which, as in the main analysis, I do not find an effect.

²⁵I define older students as those born from January through August, and younger students as those born from September to December. This choice aligns with the school-entry-age cutoff date and the Ministry of Education policy that considers the latter group less ready to start first grade on time, thus creating more lenient conditions for redshirting younger children.

²⁶Maternal education is missing for 18.5% of students. The main results are robust to restricting the sample to students with non-missing maternal education.

strongly predicts pre-K attendance (Table 2), columns 5 and 6 of Table 6 show it also predicts gains from pre-K: children of more-educated mothers benefit more on average. Columns 7 and 8 reveal no statistically significant differences by family size, though the point estimates indicate potentially larger benefits for children from larger households.

7 Mechanisms Behind Heterogeneity by Sex

What mechanisms might explain the striking gender differences in returns to UPK? A hypothesis can be formed based on the voluminous literature on the early origins of gender gaps in educational achievements (Reardon et al., 2019; Autor et al., 2016; Lavy and Sand, 2018). This literature finds that girls have early advantages in social and behavioral skills, which may enhance their ability to benefit from pre-K environments. Specifically, boys are more prone to experiencing temperament and self-regulation challenges (Bertrand and Pan, 2013; DiPrete and Jennings, 2012), whereas girls exhibit a greater propensity for engaging in constructive interactions with both peers and adults (Magnuson et al., 2016). Furthermore, girls tend to derive more benefit from unstructured curricula (Fidjeland et al., 2023). These developmental disparities are reflected in school-readiness assessments, where boys are more frequently perceived as less prepared for first grade and consequently experience higher rates of redshirting (Cook and Kang, 2020; Deming and Dynarski, 2008). This apparent lag in boys’ noncognitive skill development has led some experts to propose extending their kindergarten experience by an additional year (Reeves, 2022).

Thus, it is possible that the pre-K classroom environment is more conducive to girls’ learning because of their better interactions with teachers, greater benefits from unstructured play, and overall higher level of readiness. While I cannot provide direct evidence for the first two factors, I can test whether my results are consistent with the readiness hypothesis. If differential school readiness were driving the heterogeneity in returns to UPK, we would expect young boys, presumed to be the least ready, to benefit the least from attending UPK. In Panel A of Figure 3, I test whether sex differences in readiness explain the gender gap in UPK returns by estimating heterogeneous effects of UPK attendance on graduating from high school on time—which, as shown in the previous section, serves as a good summary index for overall school progression.²⁷ The pattern that emerges

²⁷In Appendix Figures A1 to A4, I show that these patterns are largely consistent across other school-progression outcomes.

in the figure contradicts the readiness hypothesis: The point estimate for young boys is positive and statistically larger than the estimate for older boys.

What else can explain heterogeneity in the returns to UPK? The literature on early childhood education highlights the importance of the quality of the counterfactual care environment in generating treatment heterogeneity (Kline and Walters, 2016). Since most children in this setting would remain in home care in the absence of UPK, sex differences in the home environment emerge as a potential explanation for treatment heterogeneity. In this setting—Arab municipalities with a Muslim majority—gender norms and parental preferences for boys may create different home environments for young children: Girls typically stay home with a parent or other adult relatives and participate in household chores, while boys are more likely to spend time outdoors and play with peers (UNDP, 2006; Feki et al., 2017). Consequently, the introduction of UPK may have represented a more dramatic change for girls by providing substantial exposure to peer play and nonrelative-adult interactions. For boys, these experiences were already more accessible even in the absence of UPK. Moreover, parental investments could play a crucial role. Given that favoritism toward boys is well documented in Arab and Muslim communities (Yassine-Hamdan and Strate, 2020), the introduction of UPK might have provided girls with access to quality time with adults, while for boys, the change in adult interaction was less substantial.

I provide two tests for the home-care-quality hypothesis. First, I examine whether the heterogeneity by sex is stronger in more traditional households, using maternal education as a proxy. Since gender norms and son preference are expected to be more pronounced in more traditional households, under the home-care-quality hypothesis the heterogeneous effect across sexes should be stronger among this group. Panel B of Figure 3 confirms this prediction: Girls from more traditional households benefit significantly more than boys from pre-K attendance, while in less traditional households there is no statistically significant difference between the sexes.

Second, I examine whether heterogeneity by sex is stronger among families with both sons and daughters compared to families with children of only one sex. Since there should be less scope for son preference in investments in families with only daughters, the home-care-quality hypothesis predicts that the heterogeneous effect should be driven by girls who have brothers. Panel C of Figure 3 corroborates this prediction: Girls from mixed-sex families gain significantly more from attending pre-K than boys, while in single-sex families there is no statistical difference between

girls and boys.

The analysis in this section suggests that the sex differences in returns to pre-K are more consistent with differences in home-care environments between girls and boys than with differences in developmental readiness to benefit from pre-K. However, as in most heterogeneity-based mechanism analyses, these findings should be viewed as suggestive, since month of birth, maternal education, and sibling sex composition are potentially correlated with other dimensions along which heterogeneous effects may exist. That said, any competing explanation must account not only for heterogeneity along these dimensions, but also for its interaction with sex.

8 Conclusions

This paper provides treatment estimates of the long-term effects of starting pre-K at age 3 rather than 4. By leveraging comprehensive administrative data from Israel and within-family variation in pre-K attendance driven largely by an exogenous expansion of public pre-K, I obtained precise treatment effect estimates of attending pre-K at age 3 on school progression and long-term educational-achievement outcomes. I find that attending pre-K at age 3 significantly improves school progression but has limited effects on academic achievements. These findings suggest that while earlier pre-K entry can help children progress through school more smoothly, it might not substantially enhance their academic performance. While, in theory, smoother school progression can lead to welfare gains and increased public revenues—as children enter the labor force earlier and begin earning and paying income taxes sooner—in practice, under a wide set of assumptions, these benefits are small and insufficient to justify pre-K at age 3 on their own. Applying the marginal value of public funds (MVPF) framework proposed by [Hendren and Sprung-Keyser \(2020\)](#), I calculate an MVPF that is substantially below the benchmark value of 1 for a non-distortionary cash transfer.²⁸ This calculation likely understates the true return to pre-K for two reasons. First, it does not incorporate potential effects on adult earnings, which may be positive given the im-

²⁸For example, assuming that children enter the labor force right after graduating from high school and earn NIS 7,000 in the first year with a 4% annual increase, children who graduate on time start earning at age 19 (16 years after pre-K) and children who graduate one year later start earning at age 20 (17 years after pre-K). The difference in discounted lifetime income between these two cases is NIS 14,700. Where $t=0$ is the year of pre-K for age 3 and the discount rate is 3%. Assuming a 20% income tax and an effect of 0.024 on graduating on time, I calculate that the government saves about NIS 70 for every child attending pre-K. This is significantly lower than the expenditure per child of about NIS 16,000 in 2019. The MVPF is thus: $\frac{0.024 \times 0.8 \times 14,700}{16000 - 0.024 \times 0.2 \times 14,700} \approx 0.018$. Incorporating fiscal savings from reduced grade repetition does not meaningfully affect this calculation.

provements in school progression even if high school achievements are unaffected. Second, it omits indirect effects of the expansion on non-participants and the local economy (Berne, 2025; Jackson, Turner, and Bastian, 2025). Therefore, ITT estimates may provide more appropriate inputs for cost-benefit analysis.

The analysis reveals striking heterogeneity in treatment effects across sexes, with girls benefiting significantly more than boys across all progression outcomes. I explore two potential mechanisms for this pattern: differential school readiness and differences in the home environment. The results align with the latter hypothesis, as the gender-treatment heterogeneity is stronger for children from families that are more likely to adhere to gender norms. This finding highlights the potential for UPK to serve as an equalizing force by providing opportunities that some children, particularly girls in traditional settings, might not otherwise receive at home. While this is a desired outcome, it will contribute to widening the gender educational gap, as girls outperform boys across all educational outcomes in this setting.

The findings of this paper raise several questions for future research. First, are the smaller effects I find (compared with other studies' findings) the result of estimating the impact of attending pre-K at age three rather than four? Or are they the result of a lower-quality pre-K program, as reflected by the lower expenditure per child, larger classrooms, and lower teacher qualifications? Alternatively, the gap may reflect the difference between treatment effects and ITT estimates: since most existing studies estimate ITT effects, their larger magnitudes may partly capture indirect effects of pre-K expansions on non-participants—suggesting that such spillovers are a quantitatively important component of what universal pre-K does. In addition, as I stop the analysis at the end of high school, it remains unknown whether later life outcomes will follow the pattern of school-progression outcomes (which show significant improvements) or educational-achievement outcomes (which show limited effects). Finally, a crucial question is whether it is possible to modify the preschool program in ways that maintain the benefits for girls while also generating positive effects for boys.

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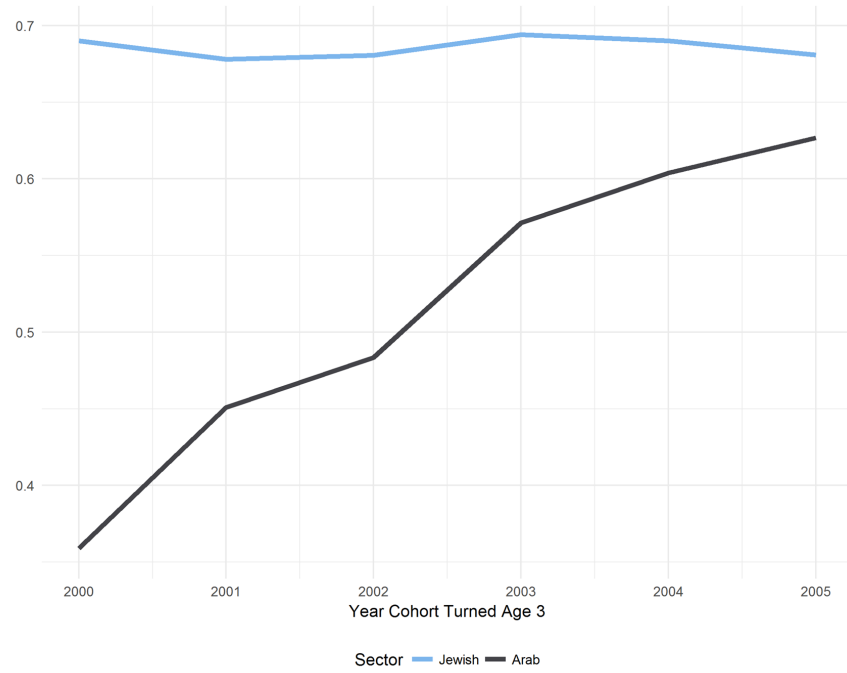
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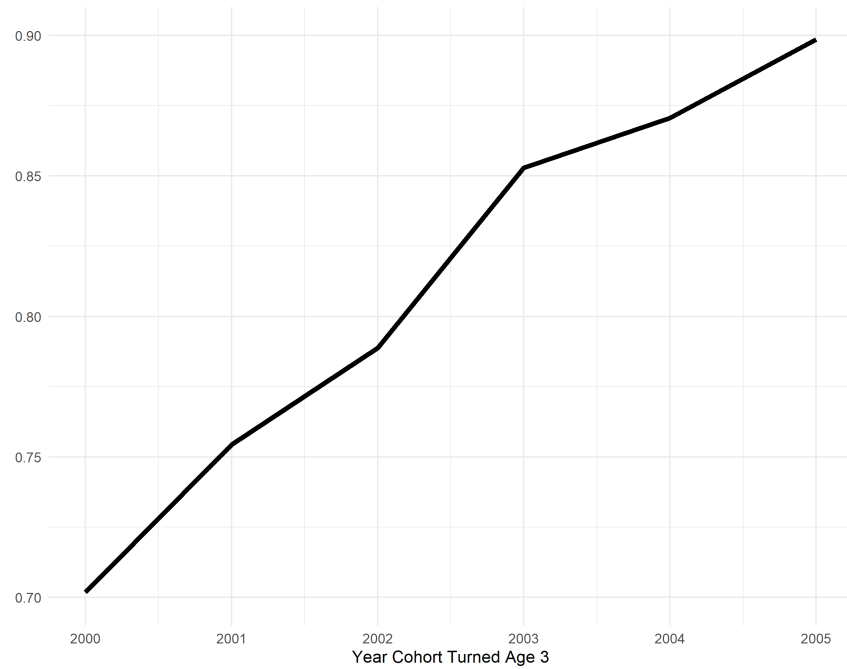
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Figures



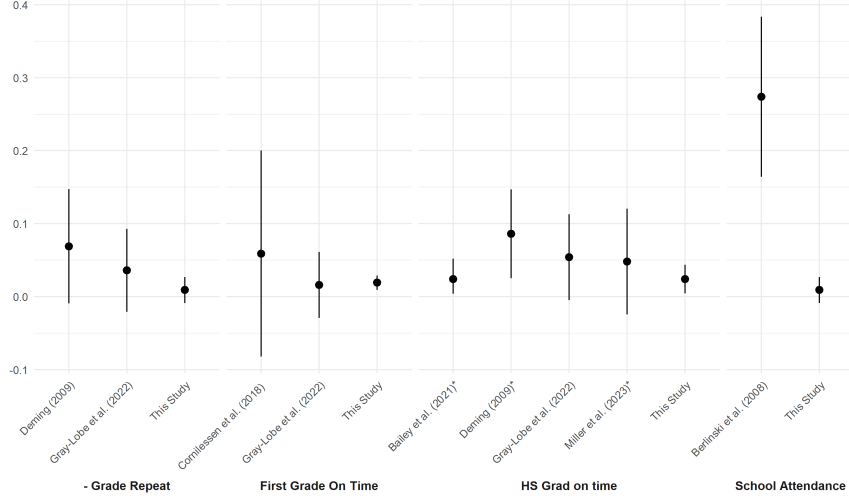
(a) Enrollment Rate in Pre-K at Age 3 by Year and Sector



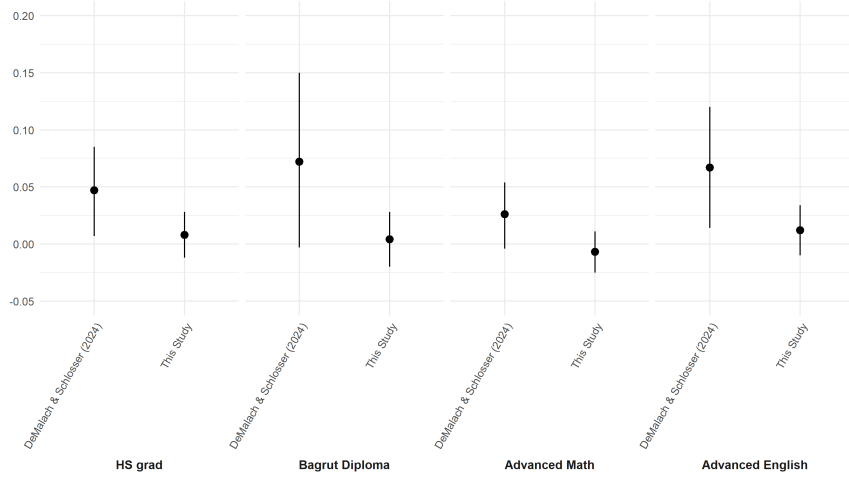
(b) Enrollment Rate in Pre-K at Age 3 in Sample Municipalities

Figure 1: Enrollment Rate in Pre-K at Age 3

Notes. The figure depicts enrollment rates in public pre-K for age three over time. Panel A shows rates for the universe of Arab (black line) and Jewish students. Panel B shows enrollment rate for the Arab localities included in the 2001 special order.



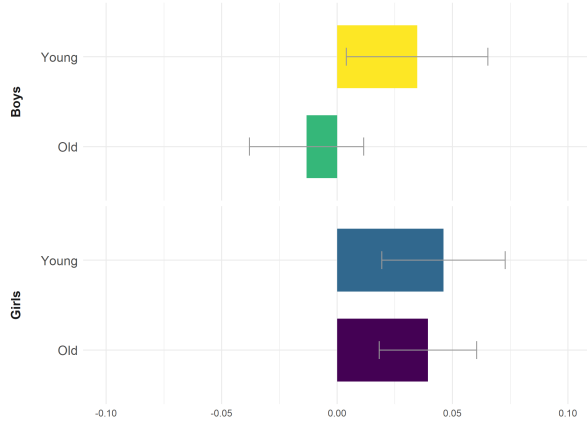
Panel A: Comparison to Other Pre-K Treatment Estimates



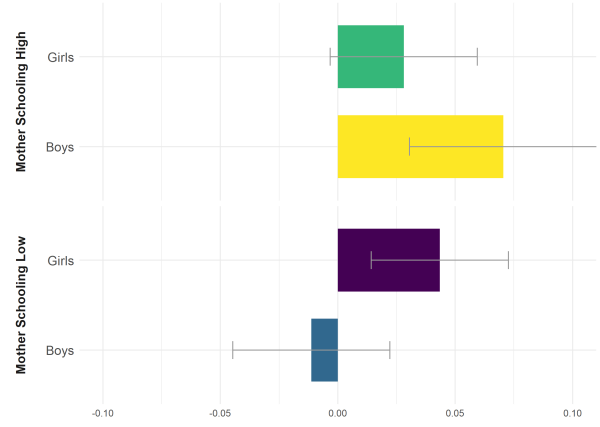
Panel B: Comparison to Estimates in [DeMalach and Schlosser \(2024\)](#)

Figure 2: Effects of Large-Scale Preschool Programs Across Studies

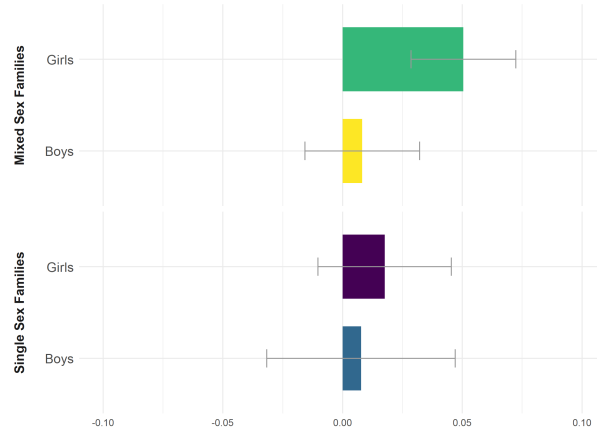
Notes. Panel A plots point estimates and 95% confidence intervals for treatment effects on four outcomes from different studies. Effects on grade repetition are multiplied by negative one for ease of presentation. For on-time high school graduation, an asterisk indicates that an on-time estimate was not available and an ever-graduated-high-school outcome is used instead. For school attendance, I use this study's estimate of ever attending 11th grade, and for [Berlinski, Galiani, and Manacorda \(2008\)](#), the estimate of attending school at age 15. For [Miller, Shenhav, and Grosz \(2023\)](#) I use the estimate with Head Start participants as the target group. Panel B shows treatment estimates and 95% confidence intervals from this study and [DeMalach and Schlosser \(2024\)](#). To calculate treatment point estimates, I scale the ITT estimate by the first-stage estimate reported in [DeMalach and Schlosser \(2024\)](#). To construct confidence intervals, I first calculate the implied 95% confidence interval for the ITT from the reported ITT standard error and then scale it by the first stage.



Panel A: Sex and Relative-Age Treatment Heterogeneity



Panel B: Sex and Maternal-Schooling Treatment Heterogeneity



Panel C: Sex and Sibling-Sex-Composition Treatment Heterogeneity

Figure 3: Estimates of Heterogeneous Effect on Graduating from High School on Time

Notes. The figure plots estimates of β in equation 1 for different subgroups. In each panel, estimates are obtained by interacting pre-K with a full set of dummy variables for each subgroup.

Tables

Table 1: Summary Statistics

	Entire Sample (1)	Family FE Sample (2)
Attended Pre-K	0.815 (0.388)	0.806 (0.395)
Girl	0.489 (0.500)	0.499 (0.500)
Father Schooling	10.296 (2.778)	10.241 (2.723)
Mother Schooling	10.092 (2.611)	10.008 (2.555)
Family Size	3.784 (1.954)	3.915 (1.973)
Month of Birth	6.621 (3.418)	6.626 (3.423)
Birth Order	3.144 (2.001)	3.161 (1.995)
First Grade on Time	0.980 (0.141)	0.979 (0.143)
Graduated High School on Time	0.826 (0.379)	0.820 (0.385)
Graduated High School	0.858 (0.349)	0.852 (0.355)
Attended 11th Grade	0.884 (0.321)	0.879 (0.326)
Ever Repeated a Grade	0.106 (0.308)	0.108 (0.310)
Diploma (Bagrut)	0.540 (0.498)	0.527 (0.499)
Diploma Credits	19.745 (12.840)	19.403 (12.872)
Advanced Math	0.223 (0.417)	0.213 (0.409)
Advanced English	0.483 (0.500)	0.466 (0.499)
Advanced Hebrew	0.316 (0.465)	0.312 (0.463)
Number of Students	54,264	40,644
Number of Families	30,466	16,747

Notes. The table shows summary statistics for key background and outcome variables. Column 1 shows statistics for students in birth cohorts 1996 to 2001 who resided in sample localities. Column 2 shows results for children who have a sibling in the sample.

Table 2: Predictors of Public Pre-K Enrollment

	<i>Dependent variable: Pre-K Enrollment</i>			
	Univariate	Multivariate Models		
	(1)	(2)	(3)	(4)
Girl	−0.001 (0.003)	0.001 (0.003)	−0.001 (0.003)	0.004 (0.004)
Father Schooling > 10	0.014*** (0.003)	0.009*** (0.003)	0.019*** (0.004)	-
Mother Schooling > 10	0.118*** (0.003)	0.068*** (0.004)	0.029*** (0.008)	-
Family Size	−0.033*** (0.001)	−0.039*** (0.003)	−0.003 (0.003)	-
Month of Birth	−0.007*** (0.0005)	−0.007*** (0.0005)	−0.007*** (0.001)	−0.007*** (0.001)
Birth Order	−0.025*** (0.001)	0.024*** (0.002)	−0.004 (0.003)	0.003 (0.003)
Municipality–Cohort Fixed Effects	✗	✗	✓	✓
Family Fixed Effects	✗	✗	✗	✓
Observations	54,264	54,264	54,264	40,644

Notes. The table presents OLS estimates from regressions of public pre-K enrollment on various characteristics. Column 1 reports coefficients from separate univariate regressions in which each characteristic is the sole independent variable. Columns 2–4 report multivariate specifications: without municipality-cohort fixed effects (column 2), with municipality-cohort fixed effects (column 3), and with municipality-cohort and family fixed effects (column 4). The sample includes all children residing in sample municipalities from birth cohorts 1996–2001. Robust standard errors are reported in parentheses below each coefficient; in column 4, standard errors are clustered at the family level. *p<0.1; **p<0.05; ***p<0.01.

Table 3: Characteristics of Switchers and Non-switchers

	Non-switchers		Switchers	
	Never Takers	Always Takers	Expansion Compliers	Expansion Defiers
	(1)	(2)	(3)	(4)
Number of Families	1,430	11,982	2,608	727
Children in Sample per Family	2.58	2.37	2.63	2.47
Father Years of Schooling	9.87	10.44	9.92	10.08
Mother Years of Schooling	9.09	10.37	9.53	9.61
Young Child Born Sep-Dec (%)	44.8%	38.8%	35.3%	50.6%
Birth Order	3.99	2.83	3.69	3.50
Family Size	4.82	3.49	4.45	4.19
Oldest Child Attended				
Pre-K at Age 3	0	1	0	1
Pre-K at Age 4	0.40	0.97	0.73	0.85

Notes. The table reports means of selected family characteristics by four mutually exclusive groups of families based on pre-K enrollment status of siblings that are included in the sample: neither the oldest nor youngest sampled sibling attended (column 1), both the oldest and youngest sampled siblings attended (column 2), the youngest sampled sibling attended but the oldest did not (column 3), and the oldest sampled sibling attended but the youngest did not (column 4).

Table 4: Effects of Public Pre-K On School Progression and High School Achievements

Panel A: School-Progression Outcomes					
	First Grade on time (1)	Grade Repeat (2)	11th Grade (3)	Grad HS on Time (4)	Grad HS (5)
Estimate	0.019*** (0.004)	−0.013* (0.007)	0.009 (0.007)	0.024*** (0.008)	0.008 (0.007)
Mean	0.979	0.108	0.879	0.820	0.852
Observations	40,644	40,644	40,644	40,644	40,644
Number of Families	16,747	16,747	16,747	16,747	16,747
Adjusted R^2	0.077	0.237	0.293	0.305	0.309
Panel B: HS Diploma Outcomes					
	HS Diploma (Bagrut) (1)	Diploma Credits (2)	Advanced Math (3)	Advanced English (4)	Advanced Hebrew (5)
Estimate	0.003 (0.009)	0.442** (0.215)	−0.007 (0.007)	0.012 (0.009)	0.010 (0.008)
Mean	0.527	19.403	0.213	0.466	0.312
Observations	40,644	40,644	40,644	40,644	40,644
Number of Families	16,747	16,747	16,747	16,747	16,747
Adjusted R^2	0.396	0.549	0.436	0.476	0.439

Notes. The table shows estimates of β in equation 1. Panel A shows estimates of the effects on school-progression outcomes. Panel B shows estimates of the effects on educational outcomes measured at the end of high school. All models control for sex, month of birth, birth order, and municipality-specific cohort fixed effects. Standard errors, clustered at the family level, are displayed in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 5: Robustness Tests

Robustness Test	HS Grad On Time	HS Diploma (Bagrut)
No Expansion Defiers	0.020** (0.009)	−0.005 (0.011)
Number of Observations		38,846
Common Cohort Fixed Effect	0.025*** (0.007)	−0.005 (0.009)
Number of Observations		40,644
No Control for Month of Birth	0.028*** (0.008)	0.006 (0.009)
Number of Observations		40,644
All Arab-majority Municipalities	0.015*** (0.004)	0.001 (0.005)
Number of Observations		133,751
Only Youngest and Oldest Child	0.023** (0.009)	0.005 (0.011)
Number of Observations		34,182

Notes. The table shows estimates of β for high school graduation on time (column 1) and obtaining a high school diploma (Bagrut) (column 2) across five robustness tests: excluding expansion defiers from the sample; replacing municipality-specific cohort fixed effects with common cohort fixed effects; removing controls for month of birth; including all Arab-majority municipalities; and keeping only the oldest and youngest children in the sample. Standard errors, clustered at the family level, are reported in parentheses.

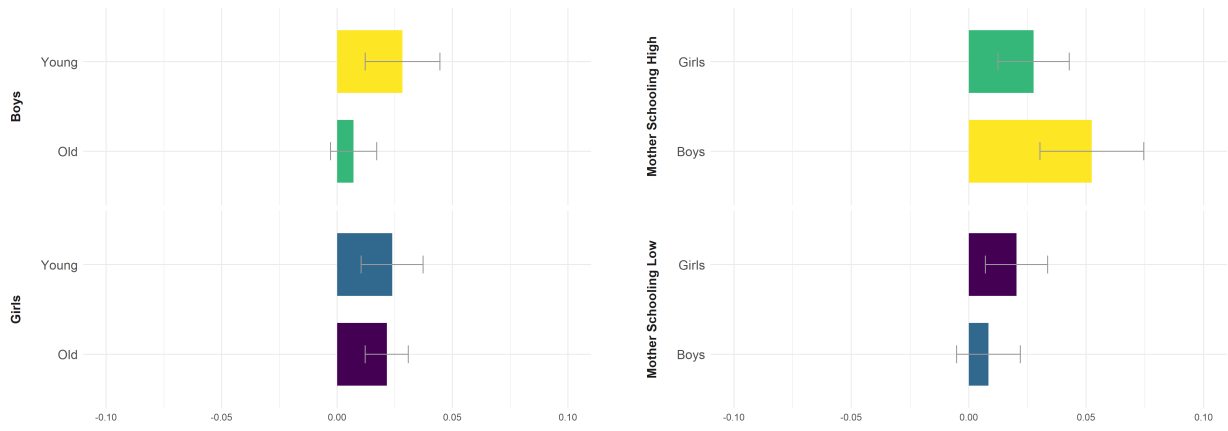
*p<0.1; **p<0.05; ***p<0.01.

Table 6: Effects of Public Pre-K on School Progression for Subgroups

	By Sex		By Relative Age		By Mother Education		By Family Size	
	Boys (1)	Girls (2)	Young (3)	Old (4)	High (5)	Low (6)	Small (7)	Large (8)
First Grade on Time	0.016*** (0.005)	0.022*** (0.004)	0.026*** (0.006)	0.014*** (0.004)	0.039*** (0.007)	0.014** (0.005)	0.016*** (0.005)	0.021*** (0.005)
<i>p</i> -value for equal effects	0.240		0.043		0.003		0.401	
Grade Repeat	-0.007 (0.009)	-0.018** (0.008)	-0.018* (0.009)	-0.009 (0.009)	-0.021* (0.012)	-0.017 (0.011)	-0.008 (0.009)	-0.016* (0.009)
<i>p</i> -value for equal effects	0.240		0.369		0.810		0.472	
11 th Grade	-0.017* (0.009)	0.036*** (0.008)	0.016* (0.009)	0.005 (0.008)	0.016 (0.011)	0.015 (0.011)	-0.011 (0.009)	0.023*** (0.009)
<i>p</i> -value for equal effects	0.000		0.263		0.978		0.003	
Grad HS on Time	0.006 (0.011)	0.042*** (0.009)	0.040*** (0.011)	0.013 (0.011)	0.050*** (0.014)	0.016 (0.012)	0.020* (0.011)	0.027*** (0.009)
<i>p</i> -value for equal effects	0.002		0.026		0.057		0.595	
Grad HS	-0.014 (0.010)	0.030*** (0.009)	0.011 (0.010)	0.005 (0.009)	0.024** (0.012)	0.001 (0.011)	0.003 (0.010)	0.011 (0.009)
<i>p</i> -value for equal effects	0.000		0.628		0.131		0.503	
Joint <i>p</i> -value across outcomes	6.03e-10		0.0448		0.0121		0.0516	

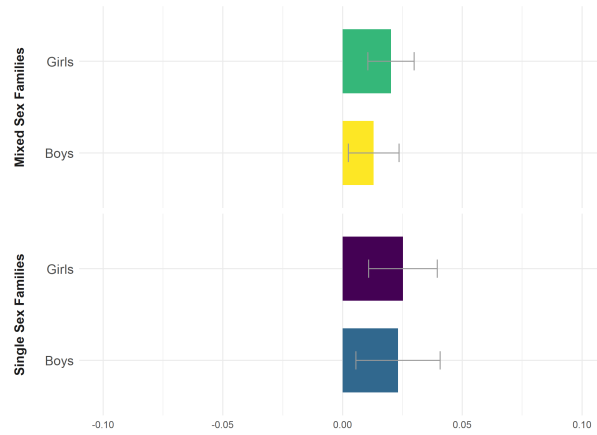
Notes. This table reports estimates of β in equation 1 on school-progression outcomes for subgroups. Columns 1 and 2 compare estimates for boys and girls; columns 3 and 4 display estimates by relative age; columns 5 and 6 show estimates by maternal education (more or less than 10 years); and columns 7 and 8 show estimates by family size (larger than three children versus smaller than or equal to three). *p*-values for equal effects come from tests of the null hypothesis that effects are equal across subgroups. All models control for sex, month of birth, birth order, and municipality-specific cohort fixed effects. The last row shows joint *p*-values from tests of the null hypothesis that subgroups have equal effects across all outcomes in the table. **p*<0.1; ***p*<0.05; ****p*<0.01.

Appendix



Panel A: Sex and Relative Age Treatment Heterogeneity

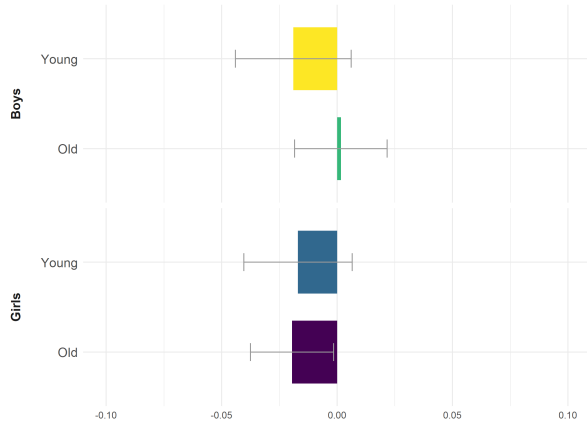
Panel B: Sex and Maternal Schooling Treatment Heterogeneity



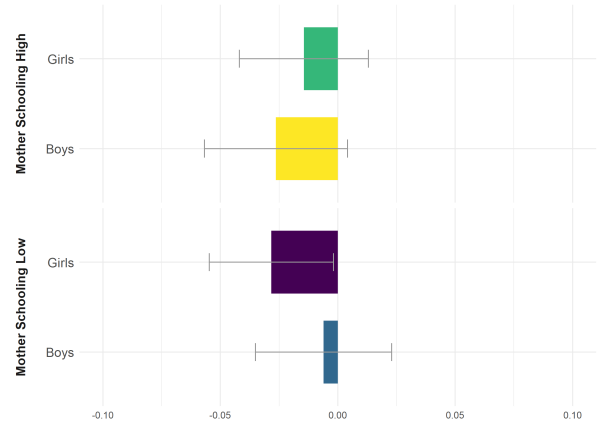
Panel C: Sex and Sibling Sex Composition Treatment Heterogeneity

Figure A1: Estimates of Heterogeneous Effect on First Grade on Time

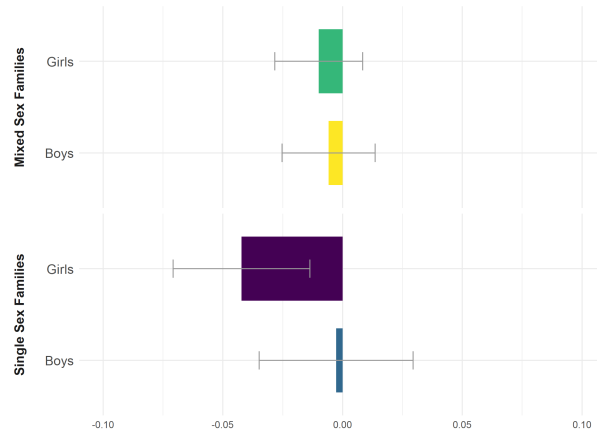
Notes. The figure plots estimates of β in equation 1 for different subgroups. In each panel estimates are obtained by interacting pre-K with a full set of dummy variables for each subgroup.



Panel A: Sex and Relative Age Treatment Heterogeneity



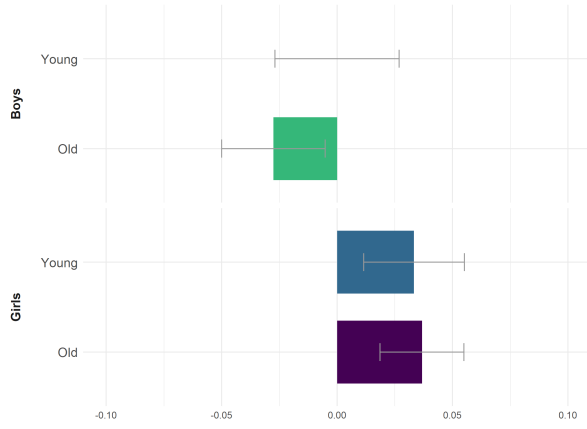
Panel B: Sex and Maternal Schooling Treatment Heterogeneity



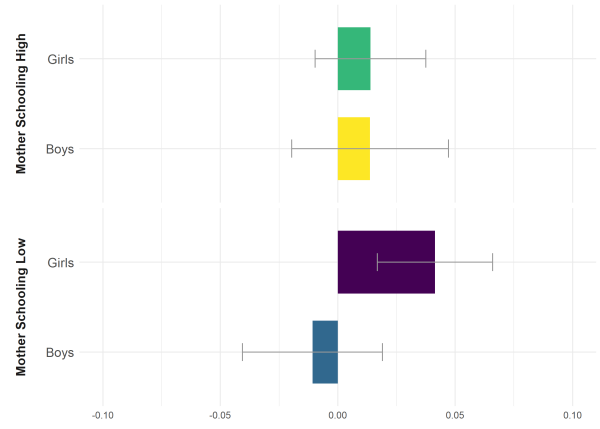
Panel C: Sex and Sibling Sex Composition Treatment Heterogeneity

Figure A2: Estimates of Heterogeneous Effect on Ever Repeating a Grade

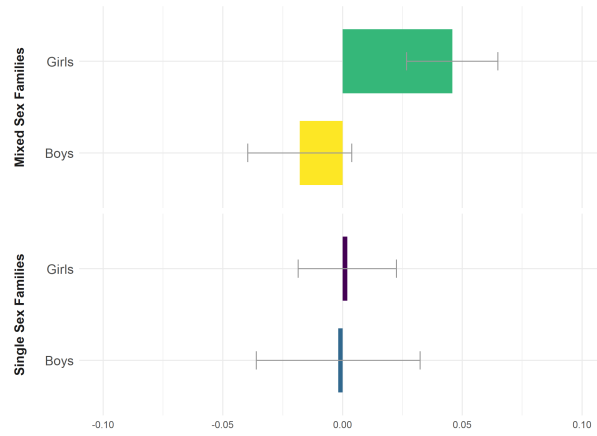
Notes. The figure plots estimates of β in equation 1 for different subgroups. In each panel estimates are obtained by interacting pre-K with a full set of dummy variables for each subgroup.



Panel A: Sex and Relative Age Treatment Heterogeneity



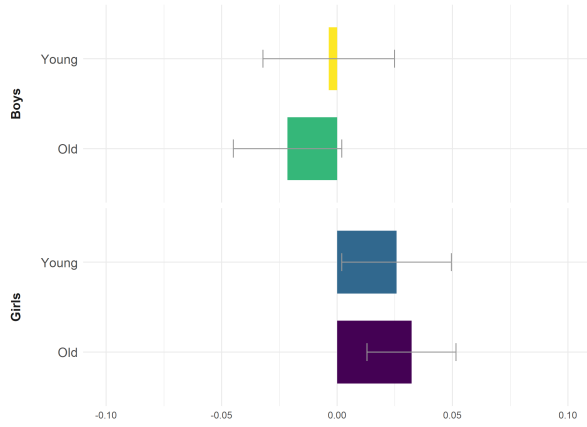
Panel B: Sex and Maternal Schooling Treatment Heterogeneity



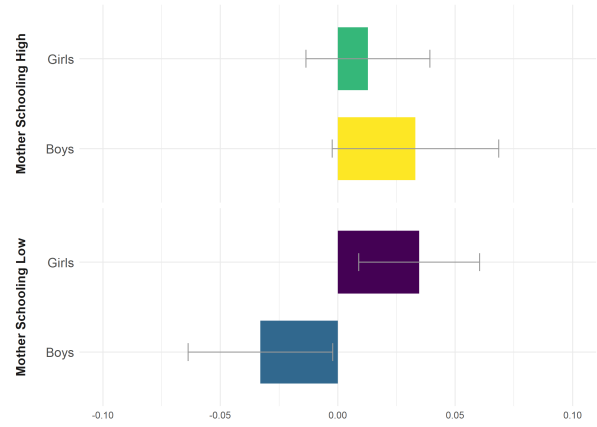
Panel C: Sex and Sibling Sex Composition Treatment Heterogeneity

Figure A3: Estimates of Heterogeneous Effect on Attending 11th Grade

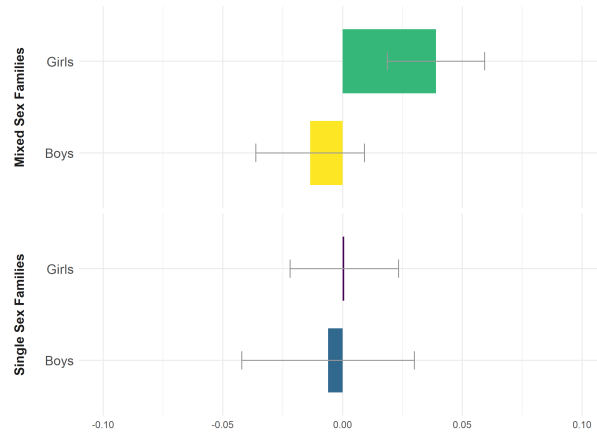
Notes. The figure plots estimates of β in equation 1 for different subgroups. In each panel estimates are obtained by interacting pre-K with a full set of dummy variables for each subgroup.



Panel A: Sex and Relative Age Treatment Heterogeneity



Panel B: Sex and Maternal Schooling Treatment Heterogeneity



Panel C: Sex and Sibling Sex Composition Treatment Heterogeneity

Figure A4: Estimates of Heterogeneous Effect on Ever Graduating from High School

Notes. The figure plots estimates of β in equation 1 for different subgroups. In each panel estimates are obtained by interacting pre-K with a full set of dummy variables for each subgroup.

Table A1: Estimates of the Impact of Public pre-K attendance on Obtaining a High School Diploma (*Bagrut*)

	<i>Dependent Variable: High School Diploma (<i>Bagrut</i>)</i>				
	(1)	(2)	(3)	(4)	(5)
PreK-3	0.124*** (0.006)	0.089*** (0.007)	0.056*** (0.006)	0.050*** (0.007)	0.003 (0.009)
Background Controls	✗	✓	✓	✓	✓
Municipality-Cohort FE	✗	✗	✓	✓	✓
Siblings Sample	✗	✗	✗	✓	✓
Family FE	✗	✗	✗	✗	✓
Observations	54,264	54,264	54,264	40,644	40,644
Adjusted R ²	0.009	0.058	0.195	0.194	0.396

Notes. This table reports coefficients from regressing high school diploma on public pre-K attendance at age three. Column 1 presents the estimated coefficient from a univariate model. Column 2 adds controls for sex, month of birth, birth order, and parental education. Columns 3 and 4 also incorporate municipality-cohort fixed effects. Column 5 further adds family fixed effects. The sample in columns 1-3 includes all children residing in sample municipalities from birth cohorts 1996-2001. Columns 4-5 restrict the sample to children with at least one sibling in the sample. Robust standard errors are reported in parentheses below each coefficient. *p<0.1; **p<0.05; ***p<0.01.

Table A2: Robustness of Pre-K Effect: Reweighted ATE Estimates

Panel A: School-Progression Outcomes					
	First Grade on time (1)	Grade Repeat (2)	11th Grade (3)	Grad HS on Time (4)	Grad HS (5)
Estimate	0.025*** (0.007)	−0.018* (0.010)	0.013 (0.010)	0.033*** (0.012)	0.017 (0.010)
Panel B: HS Diploma Outcomes					
	HS Diploma (Bagrut) (1)	Diploma Credits (2)	Advanced Math (3)	Advanced English (4)	Advanced Hebrew (5)
Estimate	−0.001 (0.014)	0.724*** (0.334)	−0.010 (0.012)	0.015 (0.014)	0.019 (0.013)

Notes. The table reports estimates of β from Equation 1, using observation weights designed to recover the average treatment effect following the reweighting method proposed by [Miller, Shenhav, and Grosz \(2023\)](#). The method was implemented in two stages. First, a multinomial logit model was estimated to predict the probability that each family belongs to one of four groups defined by the interaction of switching status and pre-K attendance. In the second stage, weights were computed as the ratio of the predicted probability of being in the attendance group to the probability of being in the switcher group. This ratio was multiplied by the inverse of the family’s conditional variance of pre-K attendance. These weights were then used to estimate Equation 1. For additional details, see the notes to Table 4. *p<0.1; **p<0.05; ***p<0.01.

Table A3: Effects of Public Pre-K on High School Achievements for Subgroups

	By Sex		By Relative Age		By Mother Education		By Family Size	
	Boys (1)	Girls (2)	Young (3)	Old (4)	High (5)	Low (6)	Small (7)	Large (8)
HS Diploma (<i>Bagrut</i>)	0.003 (0.011)	0.002 (0.011)	0.009 (0.012)	-0.001 (0.010)	-0.013 (0.018)	-0.009 (0.014)	0.002 (0.013)	0.003 (0.011)
<i>p</i> -value for equal effects	0.916		0.472		0.871		0.940	
Total Units	0.277 (0.272)	0.608** (0.261)	0.499* (0.291)	0.405 (0.247)	0.764* (0.411)	0.307 (0.316)	0.432 (0.301)	0.446* (0.262)
<i>p</i> -value for equal effects	0.296		0.766		0.353		0.969	
Advanced Math	-0.013* (0.008)	-0.000 (0.009)	-0.005 (0.009)	-0.008 (0.008)	-0.013 (0.016)	-0.005 (0.010)	-0.003 (0.010)	-0.010 (0.008)
<i>p</i> -value for equal effects	0.200		0.772		0.637		0.583	
Advanced English	0.010 (0.010)	0.014 (0.011)	0.016 (0.012)	0.010 (0.010)	0.020 (0.017)	0.013 (0.013)	0.014 (0.013)	0.011 (0.010)
<i>p</i> -value for equal effects	0.741		0.627		0.752		0.879	
Advanced Hebrew	0.019* (0.010)	0.001 (0.011)	0.023** (0.011)	0.001 (0.010)	-0.020 (0.018)	0.029** (0.012)	0.010 (0.012)	0.010 (0.010)
<i>p</i> -value for equal effects	0.140		0.079		0.019		0.973	
Joint <i>p</i> -value across outcomes	0.412		0.547		0.241		0.997	

Notes. This table reports estimates of β in equation 1 on high school educational achievements for subgroups. Columns (1) and (2) compare estimates for boys and girls; columns (3) and (4) display estimates by relative age; columns (5) and (6) show estimates by maternal education (more or less than 10 years); and columns (7) and (8) show estimates by family size (larger than three vs. smaller or equal to three). All models control for sex, month of birth, birth order, and municipality-specific cohort fixed effects. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A4: Association Between Pre-K Enrollment and Child and Maternal Characteristics

Panel A: Child Birth Weight			
	Mean [SD] (1)	No Family FE (2)	Family FE (3)
Birth weight (kg)	3.23 [0.52]	0.012*** (0.002)	0.004 (0.003)
Family Fixed Effects		X	✓
Number of children		144,348	
Number of families		61,913	
Panel B: Maternal Labor Supply			
	Mean [SD] (1)	No Family FE (2)	Family FE (3)
Mother worked	0.27 [0.44]	0.060*** (0.003)	0.003 (0.004)
Months worked	2.39 [4.41]	0.0054*** (0.0003)	0.0004 (0.0004)
Mother's earnings (in NIS 1,000)	10.11 [21.45]	0.0008*** (0.0001)	−0.00007 (0.0001)
Family Fixed Effects		X	✓
Number of children		151,773	
Number of families		66,441	

Notes. The table examines the relationship between pre-K enrollment at age three and child birth weight (Panel A) and maternal labor market outcomes when the child was three years old (Panel B). The sample includes Arab children born 2002–2007 with at least one sibling born in the same period. Panel A restricts to children with non-missing birth weight. Column (1) reports means with standard deviations in brackets. Columns (2) and (3) report coefficients from regressions of each outcome on a pre-K attendance indicator. All specifications include municipality-by-cohort fixed effects, birth-order fixed effects, and a female indicator. Column (3) adds family fixed effects. Standard errors are clustered at the family level. Data are from the Israeli Central Bureau of Statistics and cover a different period than the main analysis (birth cohorts 1996–2001). *p<0.1; **p<0.05; ***p<0.01.

Table A5: Siblings Spillovers from Pre-K Attendance – Effect on On-Time High School Graduation

Panel A: Heterogeneity by Sibling Spacing		
	(1)	(2)
PreK	0.023 (0.008)	0.024 (0.015)
PreK \times Age Gap	0.007 (0.009)	–
PreK $\times 1\{\text{Age Gap} > \text{Median}\}$	–	-0.005 (0.019)
Number of students	34,182	34,182
Number of families	16,747	16,747
Panel B: Difference-in-Differences		
	Main Sample (1)	All Arab-majority Towns (2)
Child 2 \times Treated	0.001 (0.020)	-0.007 (0.008)
Number of students	8,966	29,916
Number of families	4,483	14,966

Notes. This table examines whether one sibling's pre-K attendance affects another's likelihood of graduating high school on time. Panel A reports the estimate of β and the coefficient on the interaction between pre-K attendance and the age gap between the youngest and oldest sibling in each family (column 1), and the interaction with a dummy indicating whether the age gap exceeds the sample median (2.11 years) (column 2). To facilitate interpretation of the coefficient in column (1), the age gap variable is standardized using the sample mean and standard deviation of the age gap. The sample for these regressions is restricted to the oldest and youngest child from each family. Panel B reports estimates of δ from equation 2. The sample includes first- and second-born children in families with three children in the sample. *Treated* is an indicator for whether the third-born child attended pre-K. Column (1) uses the main analysis sample, while column (2) uses the full population of children residing in Arab-majority municipalities. In both panels, the dependent variable is an indicator for on-time high school graduation, and standard errors in parentheses are clustered at the family level.