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Short- and Long-Term Effects of Universal Preschool: Evidence from the Arab Population in Israel

Abstract

We estimate the short- and long-term effects of universal preschool education by analyzing the impact of the Israeli Preschool Law, which mandated the provision of public preschool for ages 3 and 4 starting in September 1999. We focus on the Arab population, who were the main beneficiaries of the first phase of the implementation of the Law, and exploit exogenous variation in universal preschool provision across localities due to the Law's gradual implementation. Our difference-in-differences research design compares cohorts of children in treatment localities before and after the Law's introduction to equivalent cohorts in comparison localities. We find that individuals benefited from the provision of universal preschool along various dimensions: their academic performance in elementary, middle school, and high school improved significantly, and their postsecondary enrollment rates increased substantially. We also find beneficial effects of universal preschool on additional outcomes, such as a reduction in juvenile delinquency among males and a decline in early marriage among females. Results are not driven by changes in maternal income or labor supply. A potential mechanism impacting long-term outcomes was the creation of a better learning environment in elementary and middle school, with a greater sense of security and better relationships with teachers and classmates.

JEL-Codes: I240, I250, J200.

Keywords: early childhood education, preschool.

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1. Introduction

Educational interventions at young ages can have large long-term impacts on adult outcomes (Heckman and Masterov, 2007; Cunha and Heckman, 2007; Currie and Almond, 2011; Heckman et al. 2013). These findings have motivated the growing interest of policymakers in public preschool programs as a means to reduce future income inequality and promote intergenerational mobility.[2] In fact, most European countries, including the U.K., France, Germany, all Nordic nations, and some US states and cities provide universal or large-scale preschool programs aimed at promoting children's social and cognitive development. However, evidence on the causal impact of such universal programs is scarce due to challenges in the identification of causal effects of universal policies. Moreover, there is very limited evidence on the impacts of universal preschool on long-term outcomes due to the lack of long-term follow-up data.

In this paper, we examine the causal effects of universal preschool using a quasi-experimental research design generated by the implementation of universal public preschool to ages 3 and 4 in Israel that started in September 1999. We offer a unique causal analysis of the life-cycle effects of universal preschool education, combining information from multiple datasets that cover individual histories for up to 20 years after treatment. We follow individuals throughout their elementary, middle, and high school years by examining their test scores, their success in the matriculation exams at the end of high school, and postsecondary enrollment. We also analyze possible mechanisms focusing on the learning environment in elementary and middle schools and mothers' employment and income. In addition, we evaluate important social outcomes such as juvenile crime and early marriage.

We focus on one of the more disadvantaged segments of Israeli society: the Arab population residing in localities with low socioeconomic status. The literature usually finds that disadvantaged groups benefit more from public preschool compared to children from higher socioeconomic backgrounds, primarily due to the lower quality of alternative childcare arrangements and home inputs in the former group (see, for example, the meta-analysis by van-Huizen and Plantega, 2017). In our case, the entire population in question is relatively disadvantaged, and given the large sample size, we are able to shed light on a

[2] See, e.g., President Obama's 2013 State of the Union Address: https://obamawhitehouse.archives.gov/the-press_office/2013/02/12/remarks-president-state-union-address, and President Biden's The American Families Plan: https://www.whitehouse.gov/briefing-room/statements-releases/2021/04/28/fact-sheet-the-american-families-plan/

more nuanced heterogeneity of the universal public preschool effect within this population by parents' education, fathers' income, maternal employment, and predicted performance across multiple outcomes. We also examine heterogeneous impacts by gender – an issue for which the evidence in the literature is often controversial (see, e.g., Anderson, 2008).

Our identification strategy exploits the gradual implementation of the Israeli Compulsory and Free Preschool Law for Ages 3 and 4 (hereafter "the Law") since September 1999, which states that free preschool education should be provided to all Israeli children aged 3 and 4. The implementation of the Law began in the most disadvantaged localities of Israel, which are mainly Arab localities (hereafter "treatment localities"), and led to a drastic change in the scope of public preschool provision in these localities within a relatively short time frame, and to a profound increase in the share of children attending preschool.

We focus on Arab localities and use a difference-in-differences (DID) research design, where we compare changes in individuals' outcomes in treatment localities among both exposed and unexposed cohorts to changes in equivalent cohorts from the remaining Arab localities that were not affected by the first phase of the Law's implementation. We perform several robustness tests to assess the validity of our identification strategy and confirm that our results are not driven by differential time trends, additional confounders, or the sample composition. We also apply an alternative research design based on a family fixed effects model where we compare changes in the outcomes of exposed and unexposed siblings residing in treatment localities to equivalent changes among children from comparison localities.

We find that the provision of universal preschool had a profound impact on the public preschool enrollment of Arab children in treatment localities who received preschool education for the first time. Public preschool enrollment rates increased from 23% to 90% at age 4, and from 16% to 80% at age 3, while enrollment rates in the comparison localities remained relatively stable. We also find that the reform substantially improved the educational attainment of treated cohorts: their high school graduation rates increased, as well as their participation and passing rates in the high school matriculation exams. There was also an improvement in the quality of their matriculation certificate as reflected by an increase in the number of credit units in math and English, and in the number of subjects in science. Concurrently, we find a significant increase in psychometric college-entrance exam (the Israeli SAT-equivalent) participation and psychometric test scores, and a significant increase in postsecondary enrollment rates, both in academic and vocational institutions. One possible driver of the aforementioned positive effects on educational attainment is an improvement in native language and math proficiency that we find at earlier stages of the schooling cycle. Another possible driver of the estimated long-term benefits is a significant improvement in the learning environment, better relationships with teachers and classmates, and a greater sense of security, as self-reported by the students. The positive effects of universal preschool are not driven by an increase in maternal employment or income, as there was no significant change in employment and earnings of women who lived in localities where universal preschool was introduced during this period.

We find significant beneficial effects of preschool education that go beyond educational attainment. Boys in cohorts exposed to universal preschool education were significantly less likely to have a juvenile criminal record, and young women tended to marry later. These findings are particularly important since the Arab population of Israel suffers from a relatively high crime rate, and is also a traditional society where women's age of marriage is much lower than in most Western countries.

The literature on the impacts of early childhood education has expanded significantly over recent years (see van Huizen and Plantenga, 2018; Cascio, 2021; Duncan et al. 2022; and Brum and Emick, 2023 for recent reviews). Most of the early studies focused on small-scale demonstration programs implemented in the 1960s and 1970s that provided comprehensive services to disadvantaged children and their families and were implemented as randomized control trials (e.g., Schweinhart et al., 2005; Anderson, 2008; Heckman et al., 2010, 2013). These studies found important benefits on individuals' cognitive and non-cognitive skills at different stages of the life cycle. The predominant consensus emphasizes short term benefits, that occasionally fade when evaluating tests scores in school years, and reemerge in the long term, impacting both on cognitive and non-cognitive outcomes. However, given the broad treatment provided, the selected targeted populations, and the early period when these interventions were implemented, these findings may not be directly applicable to universal preschool. Moreover, targeted interventions are unlikely to be scalable to the entire population because of their high costs and the difficulty in maintaining high standards and providing individualized treatment when implemented at a large scale.

The literature on the effects of universal or large-scale preschool programs is relatively limited given the empirical challenges in isolating causal effects. Most studies focus on specific time horizons, such as short-term outcomes during preschool and early school years (Berlinski et al., 2008; Berlinski et al., 2009; Cornelissen et al., 2018; Cascio, 2023) or long-term outcomes, such as high school graduation, years of schooling, college attendance, and employment (Havnes and Mogstad, 2011, 2015, Bailey et al., 2021). The evidence on the effects of these programs is mixed. Only a small number of studies examine outcomes over several time horizons (Felfe et al., 2015; Blanden et al., 2016; Gray Lobe et al., 2023). Notably, there is no consensus in these studies on the dynamic impacts. The conflicting findings may, in part, be attributed to differences in the counterfactual care and family background of children attending public preschools, the quality and pedagogical approach of the preschool programs, and whether initial gains from the preschool years could be capitalized after children start elementary school. Unfortunately, not all studies have the necessary information to fully assess these factors.

Our paper contributes to the literature on early childhood education in several ways. First, we provide a detailed causal analysis of the life-cycle effects of universal preschool on a large scale, by combining information from multiple sources and incorporating a very rich set of outcomes, based on individual histories for up to 20 years after treatment without attrition problems. These include human capital outcomes such school performance and post-secondary attainment as well as social outcomes such as juvenile crime and early marriage. The comprehensive set of outcomes allows us also to examine how different individuals were affected at different margins.

Second, our research setting provides a clear counterfactual for the children in our study as the vast majority of them would have been at home or in informal care in the absence of the preschool law. This contrasts with the introduction of universal preschool in many developed countries, where it sometimes results in the substitution of private preschools for high SES children or other early childhood programs for disadvantaged populations (e.g. Kline and Walters, 2016). In addition, the response to the reform was substantial, inducing almost all children to attend preschool compared to a very low prereform baseline. This large take up provides an opportunity to examine the impacts of bringing all children to the same starting point in first grade.

Third, our study provides comprehensive understanding of the potential mechanisms of universal preschool on long-term outcomes as we are able to examine the impacts in the short- and the mid- term on classroom environment and parents' employment and income.

The Arab population is a significant minority in Israel, constituting about 20% of the population. Evidence from minorities in other countries, limited only to short-term effects, indicates that preschool programs have the potential to enhance minority children's language and motor skills, thereby improving their school readiness (Gormley, 2008; Drange and Telle, 2015; Felfe and Huber, 2016; Cornelissen et al., 2018). Consequently, our study adds also valuable insights to the question of whether universal preschool programs serve as effective policy tools for fostering human capital development of children from minority groups.

Our paper also constitutes the first large scale study to provide evidence on the long term causal impacts of preschool education among Arab speakers - the fifth most spoken language in the world. The focus on early childhood education for Arab children is also important due to the diglossic nature of the Arabic language (Ferguson, 1959). In this linguistic context, Modern Standard Arabic (MSA), used for reading, writing, and school instruction differs from the spoken language, presenting a significant challenge for literacy acquisition (e.g., Saiegh-Hadad, 2003). Notably, a recent report from the World Bank (Gregory et al., 2021) emphasizes the critical need to enhance literacy skills among children in the Middle East and North African region (MENA), where 59 percent of children are in learning poverty, unable to read and understand an age-appropriate text by age 10. While preschool education is cited as playing a central role in laying the foundation for early literacy skills, the MENA region exhibits the lowest enrollment in preprimary education worldwide, ranking alongside Sub-Saharan Africa.

The rest of the paper is organized as follows. Section 2 provides some background on early education in the Israeli Arab population and on the implementation of the Law. Section 3 describes our identification strategy and Section 4 describes the data and presents summary statistics for our sample. Section 5 reports our main results. Section 6 provides a heterogeneity analysis along several dimensions, and discusses potential mechanisms of the long-term benefits of universal preschool education by presenting evidence on intermediate outcomes. Section 7 discusses several falsification and robustness tests and presents results from a family fixed effects specification. In Section 8 we compare our results with other early childhood educational programs implemented worldwide and with other educational interventions implemented in Israel at older ages. Section 9 concludes.

2. Institutional Background

The Arab minority comprises 21% of the Israeli population and numbered 2 million people at the end of 2021. They have lower educational attainment, lower incomes, and higher poverty rates compared to the Jewish population (Bank of Israel, 2021). Most Israeli Arabs are Muslim (about 84%), but there are also notable Christian (7%) and Druze minorities (8%).[3] They are considered a traditional society, especially in the context of gender relations and roles. The majority of the Arab population in Israel is residentially segregated from the Jewish population. Nearly 85% live in Arab towns and villages (in which they comprise almost the entire population), 10% live in mixed towns (populated by Arabs and Jews), and 5% are Bedouins who live in places that have not been officially recognized by the Ministry of Interior.[4] The Arab education system is also separated from the Jewish education system up until the end of high school. Most Arab students study in Arab public schools, where the majority of the teaching staff are Arabs. The language of instruction in Arab schools is Modern Standard Arabic (MSA), which differs from the colloquial spoken Arabic. Extensive research has revealed that the linguistic distance between MSA and spoken Arabic poses a considerable hurdle in the process of Arabic literacy acquisition (e.g. Saiegh-Haddad, 2003, Abu Ahmad et al., 2014).[5] Moreover, various studies have emphasized the crucial role of early exposure to MSA during preschool years in laying the foundational groundwork for the development of literacy skills (e.g., Abu-Rabia, 2000; Aram et al., 2013; Saiegh-Haddad and Spolsky, 2014; Saiegh-Haddad 2022).

Unlike the Jewish population, who already had a high preschool enrollment rate during the 1990s, only a small share of Arab children attended public preschools during that period. In the 1998/1999 school year, prior to the implementation of the Preschool Law, enrollment rates in public preschools for Jewish children aged 3 and 4 were 79.7% and 89.1%, respectively, while the corresponding rates for the Arab population were only 21.3% and 32.2%. Enrollment of five-year-old Arab children was significantly higher compared to that of younger children, with a rate of 81%; however the rate was still 12 percentage points lower than that of the Jewish population (CBS, 2000).

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[3] The data is from 2020. The authors' calculations are based on Table 2.3 in the 2021 Statistical Abstract of Israel, published by the Israeli Central Bureau of Statistics (CBS).

[4] The authors' calculations are based on Table 1.2 in the Inaugural Annual Statistical Report on Arab Society in Israel, published by the Israel Democracy Institute (2021). East Jerusalem is not included in the calculation.

[5] For the impact of diglossia and dialectal variations on language and literacy acquisition in different languages and countries see the collection of papers compiled by Saiegh-Haddad et al. (2022).

The higher enrollment rate at age 5 among Arab children can be mainly attributed to the fact that public preschool for this age has been endorsed by the Israeli government since the Compulsory Schooling Law of 1949. By contrast, until 2000, the provision of public preschools for ages 3 and 4 fell under the auspices of local authorities, who were not obliged by law to supply such services. The Ministry of Education provided substantial subsidies of 80%–90% to children of new immigrants or children who resided in areas defined by the government as targets for development.[6] As the criteria for subsidies were not applicable to most Arab localities, coupled with the financial distress faced by Arab local authorities, the majority of these localities did not provide preschool services (Abu-Jaber, 1992; Israeli State Comptroller, 1992). For example, in 1993, only 15 of 100 Arab local authorities surveyed by Ghanem (1993) provided preschool services.

Arab children below the age of 5 mainly stayed at home and did not attend any type of daycare (private or public). According to the 2009 PISA Student Questionnaire (which relates to the 1993 cohort), only 34% of Arab children reported that they attended any type of formal preprimary education program for more than one year, compared to 86% of Jewish children. The labor force participation of Arab women at that time was extremely low: 17% (for ages 25–64) in 1998 compared to 64% of Jewish women.[7]

In September 1999, the Israeli government began the gradual implementation of the Compulsory and Free Preschool Law for Ages 3 and 4. The Law states that free and compulsory preschool education should be provided to all Israeli children aged 3 and 4, and the state is responsible for providing it. The implementation of the Law started in the most disadvantaged localities, and aimed to include additional localities each year, and to cover the entire country within ten years.[8] The time frame for the addition of localities was determined according to their classification into socioeconomic clusters, which ranged from 1 (lowest) to 10 (highest).[9]

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[6] These include localities with the status of "National Priority," "Confrontation Line", and neighborhoods and localities included in the "Urban Renewal Project". Historically, preschool subsidies in localities with the special governmental status of "target for development" began as early as 1978 (*Ma'ariv*, June 4, 1978). However, until the mid-1980s, Arab localities were not granted such status. Since then, some Arab localities were included in this category. See, e.g., Government Decision 323 of April 1987, which equalized eligibility between Druze localities and nearby Jewish development localities, providing preschool subsidies also to Druze localities (12th Knesset Proceedings, Booklet 17, January 21, 1991, p. 2064) and another government decision which equalized eligibility to public benefits between Jewish and Arab localities near the borderline of Israel (11th Knesset Proceedings, Booklet 35, July 6, 1988, p. 3591).

[7] The authors' calculations are based on data from the 1998 CBS Labor Force Survey.

[8] For a review of the Law's implementation, see Kop (2002) and Blass and Adler (2004).

[9] The Israeli Central Bureau of Statistics computes a socioeconomic index for each locality, which reflects a combination of some basic characteristics such as financial resources of the residents,

Beginning in September 1999, universal free preschool education was provided in localities classified into clusters 1 and 2. The Law affected the Arab population to a great extent as 91% of the localities included in clusters 1 and 2 were Arab. As a result, the majority of Arab children covered by the Law got access to preschool education for the first time. The Law was also officially implemented in the special areas defined by the government as targets for development. However, as noted above, these localities were already receiving substantial preschool subsidies of 80%-90% prior to the Law, so universal preschool was de-facto available for them many years before, since the mid-1980s. Preschools operated 6 days a week for 6.5 hours per day in classes of up to 35 children with one preschool teacher and one or two teacher aids.

The original intention of the government was to gradually extend the Law's coverage to additional localities following their cluster classification. However, in practice, this gradual expansion was repeatedly postponed over the years due to budget constraints. Only fifteen years later, in 2015, was the Law's coverage officially expanded to include the entire country.[10] Throughout the whole period, there was no enforcement of compulsory education in any of the localities included in the Law's mandate.

Figure 1 plots the geographical distribution of Arab localities. Localities that received preschool education for the first time due to the implementation of the first phase of the Law's mandate are marked in red. All other Arab localities are marked in blue. The Central district contains only Arab localities that were not included in the first phase of the Law's mandate. The Southern district is comprised exclusively of Bedouin localities that were included in the first phase of the Law's mandate. These localities differ along many dimensions from the rest of the Arab population (see, e.g., Abu-Bader and Gottlieb, 2013). The Northern district of Israel is the only region that contains both a significant number of Arab localities that were included in the initial stage of the Law's mandate and a significant number of Arab localities that were not, and will therefore be the focus of our study.

3. Research Design and Identification Strategy

We focus our study on the Arab localities situated in the Northern district of Israel. This encompasses 15 localities that first gained access to universal free preschool in 1999

housing, education, employment, etc. Localities are then ranked according to this index which defines their socioeconomic ranking and allocated into 10 clusters that are as homogeneous as possible according to a measure of distance in their socioeconomic index. For more information, see CBS (2003).

[10] Some localities started to be covered by the Preschool law after 2003 due to a change in their socioeconomic cluster (i.e., they were reclassified in clusters 1 or 2).

(hereinafter treatment localities) and 22 localities where there was no significant change in access to public preschool education during the first phase of the Law implementation (hereinafter comparison localities). Among the latter group, 17 localities received preschool subsidies before the Law was implemented (always treated), while 5 localities did not have access to public preschool education throughout the sample period (never treated).[11]

Figure 2 presents enrollment rates for our analysis sample by age and year, stratifying localities by treatment status: treated, never treated, and always treated. To simplify the discussion, and in line with Ministry of Education data, we define the first year of the Law's implementation to be 2000 (which corresponds to the 1999/2000 academic year). Enrollment rates increased significantly for the treated group: from 18% and 31% to 91% and 93 % between 1999 and 2003 for ages 3 and 4, respectively. By contrast, enrollment rates in comparison localities (never treated or always treated) did not change much. Enrollment rates for age 5 were already close to 100% during the whole period and did not trend in any specific direction. Trends in enrollment rates of these three groups in the Northern district highly resemble the overall trends observed in all Arab localities (see Figure A1).

To examine the impact of universal preschool education on children's outcomes we apply a difference-in-differences (DID) approach. Specifically, we compare the change in outcomes between cohorts of children who lived in treatment and comparison localities and reached preschool age before and after the implementation of the Preschool Law. The *prereform* cohorts were born in 1991–1994, while the *postreform* cohorts were born in 1995–1999, since the first year of implementation was the 1999/2000 school year.

The estimating equation for our analysis is:

$$Y_{ist} = \alpha + \beta Exposed\_Preschool_{s(t+4)} + \gamma X_{ist} + \delta_s + \lambda_t + \varepsilon_{ist} \qquad (1)$$

where Y_{ist} denotes the outcome of interest, measured for individual *i* from locality *s* who was born in year t. $Exposed\_Preschool_{s(t+4)}$ is an indicator that takes a value of 1 if an

[11] We include only localities with independent local authorities that have their own socioeconomic cluster definition, as specified by the Israeli Central Bureau of Statistics (CBS). We exclude from the sample 5 localities whose cluster classification was updated and, as a consequence, were added to the Law's mandate a few years after the Law's initial implementation, 3 Druze localities in the Golan Heights that did not participate in the 1995 census and as a result did not have a CBS cluster classification, and 6 localities that could not be classified either to the treatment or the comparison group.

individual lived in a treatment locality and was at most 4 years old when the Law was implemented, and 0 otherwise. X_{ist} includes the following individual-level covariates: parental years of education, indicators for deciles of paternal annual labor earnings when the child was 2 years old (with a separate indicator for individuals with missing/zero earnings), maternal employment when the child was 2 years old, family religion (Christian, Druze, or Muslim), and gender.[12] δ_s are locality fixed effects that control for any cohort-invariant differences across localities and λ_t are cohort fixed effects that nonparametrically control for time effects at the level of the cohort. In all estimations, standard errors are clustered at the locality level. The coefficient of interest β should be interpreted as an estimate of the intention-to-treat (ITT) effect of universal preschool education. It is the parameter of interest from a policy perspective when the objective is to capture the effect of providing universal preschool education. In Section 8, we also present local average treatment effect (LATE) estimates for the effects of enrollment in universal preschool by scaling the ITT estimates by the increase in public preschool enrollment that followed the reform in order to compare our results with the existing literature.

Our empirical strategy relies on the assumption that trends in outcomes in treatment and comparison localities would have been the same in the absence of the implementation of the Law. To assess the validity of this assumption we perform a battery of tests that are summarized in Section 7 and discussed in detail in Appendix C.

In some settings, it can be problematic to use always-treated units as a comparison group, especially in a staggered DID design, where different units receive treatment in different periods of time (see, e.g., Callaway and Sant'anna, 2021; Roth et al., 2023). Though our DID design is not staggered, it can be argued that universal preschool in the always-treated localities had some dynamic effects over time that persisted during the period of study, biasing our estimates. Nevertheless, it is important to note that since the group of always treated localities received preschool education since the mid-1980s, more than ten years before the reform, we expect the effect of preschool to be stable in our period of study for this group and therefore not to bias our estimates. This assumption is also supported by Figure 2 presented above, where we show that preschool enrollment

[12] We defined an individual as employed if his/her monthly labor earnings are at least half of the minimum wage. Results are robust to an alternative definition that defines employment if earnings are above zero. As noted above, the labor force participation of Arab women in the sample period was very low. Thus, instead of controlling for maternal wage deciles we control for mothers' employment.

rates remained relatively stable in these localities. To strengthen our claim, we also perform a robustness test in Appendix C using only never-treated or always-treated localities as comparison group, showing that estimates remain similar to the main results.

4. Data and Descriptive Statistics

Data

Our dataset was created by merging administrative records from multiple sources stored in the research room of the Israeli Central Bureau of Statistics. The starting point is the Israeli population register, from where select all Israeli Arabs born in 1991–1999. The registry includes also information on their gender, locality of residence, and marital status.[13] Using personal identifiers, we merge these data with Israeli educational registers, which provide information on individuals' enrollment in primary, secondary, and postsecondary education.

We proceed by merging the data with students' records on centralized exams administered by the Israeli Ministry of Education (MOE). The first set of exams is the GEMS (Growth and Effectiveness Measures for Schools, or *Meitzav* in Hebrew) exams, conducted in the fifth and eighth grades in four subjects: native language (i.e., Arabic), English, math, and science. The GEMS exams also include a student questionnaire on the learning environment filled by students from fifth to ninth grade.

We also merge students' data from matriculation exams, which are national high school exit exams taken in various core and elective subjects between the tenth and twelfth grades.[14] We also obtain information on students' performance on the psychometric exam, a standardized test (similar to the SAT in the US) used in combination

[13] In the best-case scenario, we would have observed the individuals' locality of residence when he/she was 2 years old, prior to reaching preschool age. Unfortunately, we observe locality of residence only in specific years (1983, 1995, 1997–2001), and the data is sometimes missing. Therefore, we use an imputation method for the locality of residence in the nearest relevant year. This measurement error is probably negligible as the rate of internal migration of Israeli Arabs is very low. In 2007, only 9.5% of adult Arabs did not live in the same locality in which they were born, where the most common reason for a move was marriage, prior to having children (Hleihel, 2011).

[14] The matriculation certificate is a prerequisite for postsecondary admission. It is one of the most important educational milestones. Similar high school matriculation exams are found in many countries and some states in the US. Examples include the New York Regents Examinations and the French baccalaureate exams. The matriculation certificate is obtained by passing a series of national exams in core and elective subjects. Students choose to be tested at various levels of proficiency, with each test awarding from one to five credit units per subject, depending on difficulty. Some subjects are mandatory, and, for many, the most basic level is three credit units. Advanced level subjects are those subjects taken at four or five credit units. A minimum of 20 credit units is required to qualify for a matriculation certificate.

with the matriculation certificate as the main admission criterion in higher education institutions.

Finally, we merge our dataset with administrative police records on juvenile crimes, which contain information on whether an individual was arrested and had a criminal record in youth (until age 18) and the general category of the crime. Table A1 places the outcomes of our study on an age timeline to provide a general overview of the cohorts and time horizon covered in this study.

We further enrich the students' data by adding family background characteristics, namely, information on parental education from the education registry and information on the number of siblings from the population registry. In addition, we use administrative records provided by the Israel Tax Authority to obtain information on the employment and earnings of the parents of the individuals in the main sample. Given that at the time of dataset construction such information was only available up to the year 2018, we cannot analyze the employment and earnings of the cohorts affected by the reform, as they are still too young.

Our final sample includes around 84,000 individuals from the treatment and comparison localities in the relevant cohorts. In Table A2 we provide a full description of the outcome variables used in this study and their definitions.

Descriptive Statistics

Table 1 presents the socioeconomic characteristics of the treatment and comparison localities based on data compiled in the 1995 Israeli census, prior to the Law's implementation. In Column (3) of the table we report differences between the two groups of localities. The population in treatment localities was significantly more disadvantaged along various dimensions than the population in comparison localities. For example, the income per capita was about 16% lower, the dependency ratio was higher, and educational attainment was lower. This is unsurprising since the Law was first implemented in the two lowest socioeconomic clusters of localities. Notably, the treatment and comparison localities are similar in terms of average population size.

Table 2 presents family background characteristics of the children in the prereform cohorts (born in 1991–1994) in the treatment and comparison localities. Here again, we see that the treatment population was more disadvantaged. The parents of children in treatment localities were less educated, had a lower income, and had more children. Also, the ethnic composition is different between the two groups of localities: the Druze Arabs are in the comparison localities, while the Bedouin are mostly in treatment localities. In

Panel B of Table 2 we examine differences in outcomes of the individuals in the prereform cohorts (born in 1991–1994) between the treatment and comparison localities. Most outcomes point to the relative advantage of the population in the comparison localities during the prereform period.

5. Results

High School Outcomes

We report in Table 3 our main DID estimates from equation (1) for high school outcomes. In Column (1), we report estimates for the full sample and in Columns (2) and (3) we show estimates by gender. We report also outcomes' means (in italics) of the prereform cohorts in treatment localities. To deal with multiple hypothesis testing, summarize our high school outcomes, and increase power, we also construct an index of high school performance (reported at the top of the table) by computing a standardized average (z-score) of all standardized individual high school outcomes.

We find that the implementation of the Law significantly improved high school graduation and matriculation exam outcomes of Israeli Arabs in treatment localities. Universal preschool increased the likelihood of graduating from high school by 2.8 percentage points (a 3.5% increase relative to the prereform mean); it increased the participation rate in the matriculation exams by 3.7 percentage points (5%). The likelihood of obtaining a matriculation certificate rose by 4.3 percentage points (11%) and the probability of obtaining a matriculation certificate that meets university entrance requirements increased significantly as well by 11%.[15] The improvement in the quality of the matriculation certificate is also reflected in the increased share of individuals who obtained at least 4 credit units in English and Math subjects (11% and 8%, respectively). Furthermore, the number of science subjects attained in the matriculation certificate increased by 0.9 (a 13% increase).[16] While some estimates for the effect of universal preschool differ by gender, the general pattern points to a significant improvement in high school outcomes for both boys and girls.

Figure 4 presents estimates and 95% confidence intervals for the high school performance index and all high school outcomes in the form of an event-study design

[15] A matriculation certificate that meets university entrance requirements includes at least 4 credits in English and another subject at a level of 4 or 5 credits.
[16] Science subjects include physics, chemistry, biology, and computer science.

where year zero denotes the first year of the Law's implementation.[17] The figure reports also the p-values for a joint test of significance for the coefficients in the pre or in the post reform period. The estimates of the prereform period are small in magnitude and not statistically different from zero (individually or jointly) and they do not show any clear pattern of a differential trend in outcomes in treatment versus comparison localities before the implementation of the Law. This is also consistent with the placebo exercise we discuss in Table A8 in Section 7 where we find no differential changes in outcomes between treatment and comparison localities when we compare the first two and the last two years of the prereform period. By contrast, the postreform period estimates observed in Figure 4 show a substantial increase in outcomes for the treated localities relative to the comparison localities for the cohorts exposed to universal preschool education relative to the prereform period.

We assess the robustness of this result to possible violations of the common trends assumption using the method suggested by Rambachan and Roth (2023) focusing on the high school performance z-score. Results, discussed in Appendix A and reported in Figure A2, suggest that our results would remain significant even if we allow for some deviations of the parallel trends assumption.

Postsecondary Outcomes

Having found that preschool education improved educational outcomes by the end of high school, we proceed to examine whether the effect persists in the longer term.

Psychometric Test

Admission to most higher education institutions in Israel is based on a weighted average of the matriculation average score and the psychometric test score. The psychometric test is a standardized test, similar to the SAT in the US. It includes three sections: quantitative, verbal, and English and is administered in various languages including Arabic. The positive effect of universal preschool education on the matriculation rate and quality of matriculation certificate enhanced access to higher education. It is therefore likely that to

[17] The figure plots estimates for β_τ and their standard errors from the following model:

$$Y_{ist} = \alpha + \sum_{\tau=-4,\tau\neq-1}^{\tau=4} \beta_\tau Treated_s \times D_{i,2000+\tau} + \gamma X_{ist} + \delta_s + \lambda_t + \varepsilon_{ist}$$

where for a given τ, the indicator $D_{2000+\tau}$ takes a value of 1 if the individual was 4 years old in year 2000+τ, and 0 otherwise. The omitted period is $\tau = -1$, which is the year before the Law's implementation. For $\tau = -4, \dots, 4$, β_τ denotes the evolution of outcomes in treatment localities net of equivalent changes in comparison localities.

find an increase in the participation rate in the psychometric test. Indeed, as reported in the first row of Table 4, we find that the participation rate in the psychometric test increased significantly: by 2.8 percentage points (a 7% increase) when we examine whether individuals ever took the psychometric exam, and by 3.3 percentage points (a 9% increase) when we examine whether individuals took the psychometric exam by age 19.[18] We find an effect for both genders with a larger impact for boys, who have a lower baseline mean relative to girls.

We also examine performance in the psychometric test using a series of indicators for performance above different quartiles of the test score distribution to avoid selection bias due to the increase in the probability of taking the test.[19] The indicators get a value of zero for students who did not take the test.[20] Estimates for the test score indicators suggest that universal preschool education improved individuals' total score as well as the score in each section: Verbal, Quantitative, and English. We observe positive effects not only for score threshold indicators at the bottom of the test score distribution (probably induced by the increase in the number of test takers) but also for threshold indicators in the middle part of the distribution, and sometimes even for the top part of the distribution. Generally, the effect is larger for boys than for girls.

Enrollment in Postsecondary Institutions

We next examine the effects of the Law on enrollment in postsecondary institutions. We cannot fully observe the realization of this outcome for all cohorts as the youngest cohort in this study (born in 1999) was 18–19 years old in the last year of our data (2018). We therefore limit the analysis to the 1991–1998 cohorts and examine postsecondary enrollment (at any age), which, even if censored, might be informative of the Law's effects as long as enrollment timing in treatment and comparison localities is similar and is captured by cohort fixed effects. In addition, we also examine an uncensored outcome

[18] We examine the outcome taking the test by age 19 to focus on a result that does not suffer from censoring.

[19] Students can take the psychometric test multiple times and choose their best score for application to institutions of higher education. The table reports the results on the maximum score attained. Results using the first score are similar and available upon request.

[20] The quartiles are defined based on the full distribution of test scores of tests in the Arabic language in 2015, which is roughly the middle of the sample period (NITE, 2017, pp. 13 and 303). Test scores in the Arabic version of the exam are much lower than in the Hebrew one. In 2015, for example, the average total score of students who took the exam in Hebrew was 576, whereas the average total score of students who took the exam in Arabic was 477. The gap in that year accounts for 0.9 of a standard deviation.

defined as postsecondary enrollment by age 19. Figure A3 shows that this is the most common age of undertaking postsecondary studies among Israeli Arabs.

The results reported in Table 5 show that preschool education had substantial effects that go beyond the reported increase in high school achievement. Focusing on the estimates that denote enrollment at any age (Columns (1)–(3)) we see that the reform increased the probability of enrollment in any postsecondary education institution by 5.3 percentage points (a 16% increase relative to the prereform mean). This effect is pronounced at almost all levels of postsecondary education: first-tier university education, second-tier college education, and vocational education. Additionally, we see a decrease in the probability of attending teacher training institutions.[21] Note that the decline in enrollment in teacher training institutions is smaller than the increase observed in other institutions, implying that the increase in postsecondary academic institutions stems both from an increase in postsecondary enrollment and from some switching of individuals from teacher training institutions to academic institutions of higher quality. Our findings are qualitatively similar when we examine an uncensored outcome: postsecondary enrollment by age 19 (Columns (4)–(6)). There are some differences by gender for the uncensored outcomes, but once we examine the effects in percentage terms (relative to the outcome means), the impact seems to be similar for boys and girls, with a slightly larger increase for boys. For example, the probability of postsecondary enrollment by age 19 increased by 24% for boys and by 21% for girls.

Additional Outcomes

Juvenile Crime

Small-scale targeted preschool programs have been found to benefit individuals' life prospects along many dimensions by improving mental health, reducing criminal activity, increasing stability of marriages, and diminishing tobacco use (Schweinhart et al., 2005; Anderson, 2008; Heckman et al., 2013; Conti et al., 2016). For universal, or large-scale programs, the evidence on these types of outcomes is scarce. Two exceptions are Gray et al. (2021) who find improved disciplinary behavior in high school and a reduction in juvenile incarceration and Havnes and Mogstad (2011) who find some evidence for a delay

[21] Teacher training institutions are the least selective postsecondary academic institutions. In the 2017/2018 academic year, the average psychometric score of students enrolled in these institutions (488) was significantly lower than that of students enrolled in universities (628) and in colleges (521) (CBS, 2019a, 2019b).

in marriage and parenthood but no reduction in the probability of becoming a single parent. Our comprehensive data allows us to shed light on some of these effects.

Arabs are disproportionately represented in criminal activity records in Israel. In 2019, Arab youth accounted for 35% of juvenile criminal records while their share in the population was only 28% (The Knesset Research and Information Center, 2020). Furthermore, in 2019, 20% of Arabs reported that they did not feel safe from violence in their locality of residence, compared to only 8% of Jews (CBS, 2021). Focusing on the population of our study, we observe that the share of males with at least one criminal conviction in their juvenile record (until age 18) was 17% in the prereform cohorts in the treatment localities.

Preschool education might reduce juvenile crime by improving personality traits and reducing externalizing behavior (Heckman et al., 2013), by keeping children longer in school and mechanically keeping them off the streets during schooldays (Lochner and Moretti, 2004). Additionally, it might affect individual preferences for crime, instilling moral values, and increasing the psychic costs of breaking the law (Arrow, 1997) and increase individuals' patience, inducing them to avert risky behaviors (Becker and Mulligan, 1997).

Our results in Table 6 show that universal preschool reduced the likelihood of having a juvenile crime record by 3 percentage points for boys (an 18% decrease relative to the prereform mean). The reduction in crime stems from a decline in life and body offenses and in sex and property offenses.[22] Interestingly, the effect on security and order offenses is much smaller and insignificant. This is in line with the literature that finds no causal relationship between education or economic conditions and terrorism or hate crime (see, e.g., Krueger and Malečková, 2003; Abadie, 2006; Benmelech et al., 2012). Estimates for the effects of preschool education on juvenile crime among women are essentially zero. This finding is expected given the low baseline mean for women (less than 0.5% versus 17% for men).

Early Marriage

Although Israeli Arabs went through a rapid modernization process in the last half century, they remain a more traditional society than most Western societies. In 2017, the average

[22] Security and order offenses include offenses against the security of the state or against public order. Life/body offenses include offenses against a person's life and bodily harm. Sex/property offenses include sexual offenses and property offenses. Other offenses include fraud, morality offenses (usually drug-related), economic offenses, licensing offenses, and administrative offenses. Our data does not include a more detailed breakdown of the offenses for confidentiality reasons.

age of first marriage was 23 years for Israeli Arab women in contrast to an average age of 26 years for Israeli Jewish women and 30 years for women in the OECD countries.[23] Given the role of early marriage for women's educational and fertility decisions we examine the impact of preschool education on the probability of early marriage. Figure A4 presents the cumulative share of married men and women between the ages of 17 and 27 in the 1991 cohort (pre-treatment cohort), for which we can observe the longest time horizon. As the figure shows, a notable portion of the women, about one-third, married at early ages (18–21). By contrast, only 2% of men married by age 21. We examine the effect of preschool education on marriage by age 21, since we can observe this outcome across several postreform cohorts without censoring.

Preschool education could potentially delay the age of first marriage by reducing the probability of dropping out of high school and by increasing the probability of enrollment in higher education institutions, as documented above. In addition, the better employment and earnings prospects of educated women are expected to reduce gains from marriage in a framework where men and women specialize in market and non-market work, respectively, as is typical of traditional societies (Becker, 1981; Blau et al., 2000). Finally, increased education might affect the age of marriage by reducing religiosity and eroding traditional values (Cesur and Mocan, 2018; Hungerman, 2014).

The effects of universal preschool on the probability of marrying at an early age are presented in Figure 4, where we plot DID estimates and 95% confidence intervals from models in which the dependent variable is marrying by age 18, 19, 20, or 21. Panel A reports estimates for women. The estimates are a bit noisy but they all point to a decline of about 1.5–2 percentage points in the probability of early marriage. Focusing on marriage by age 21, we observe that the point estimate implies a decline of 5% relative to a baseline of 32%. Panel B reports estimates for men. The estimates are all small with confidence intervals that do not reject the hypothesis of a zero effect.[24]

6. Heterogeneity Analysis, Mechanisms, and Intermediate Outcomes

Early childhood interventions are generally found to be more beneficial among disadvantaged populations (Blau and Currie, 2006; Elango et al., 2016). One critical factor when examining heterogeneity of preschool programs is the counterfactual childcare. This is particularly important in the case of universal preschool provision as it might crowd out

[23] The statistics for Jews and Arabs were calculated by the authors from Tables 2.35 and 2.36 in CBS (2020). OECD statistics are taken from Indicator SF3.1 in OECD (2019).

[24] Estimates for marriage of males by age 18 are not included since there are almost no married males by this age in the sample.

high-quality targeted programs (e.g., Bassok et al., 2014). Alternatively, universal preschool might provide an educational environment for children who would have otherwise been at home or would have attended low-quality childcare. Evidence on at-home care versus formal childcare points to beneficial effects for children from lower SES families (Cascio and Schazenbach, 2013; Felfe et al., 2015; Drange and Havnes, 2019) and mixed or detrimental effects for children from high SES families (Herbst, 2013; Havnes and Mogstad, 2015). In our setup, the counterfactual childcare was mainly home care either by the mother or by a close relative. So, the results should be interpreted in this context.

Another important issue to consider when analyzing heterogeneity across groups, is the compliance rate for each group. Unfortunately, we lack the data on preschool enrollment at the individual level for the prereform period.[25] Nevertheless, to gain some insights on the characteristics of compliers, we examine differences in preschool attendance by family background in the postreform period between treated localities and localities from the comparison group that did not have access to universal preschool during that period (never treated). Overall, the analysis reported in Appendix Table A3 show no consistent pattern of selection into preschool enrollment by sociodemographic characteristics, implying that the universal preschool policy reached children from all socioeconomic backgrounds.[26] These results imply that our ITT estimates among different groups reported below reflect differences in the impact of preschool attendance rather than differences in compliance.

In Table 7, we report DID estimates and outcome means for the effects of universal preschool for different groups. To save space, we report estimates for the summary outcomes reported in the main analysis that refer to each of the domains analyzed above. Our results for other outcomes are highly consistent with the results discussed below. Given the extremely low incidence of juvenile crime among girls and of early marriage among boys, we report estimates for the relevant genders for these two outcomes (crime for boys and marriage for girls), while for all other outcomes we focus on the full sample.

Estimates obtained from the stratification by parental education (Columns (1)–(4)) suggest that the positive effects of universal preschool human capital outcomes are

[25] Data on preschool enrollment in the prereform period is only available at the aggregate locality level.

[26] Estimates from a linear probability model of preschool attendance at age 3 and 4 are reported in columns (1) and (2) respectively. The model includes background characteristics and their interactions with a treatment dummy. Overall, most estimates do not show any significant pattern of selection, except one estimate of positive selection into enrollment at age 3 by father's education, and one estimate of negative selection into enrollment at age 4 by number of siblings.

stronger among children whose parents, especially mothers, did not complete 12 years of schooling. We did not find such heterogeneity by parental education for social outcomes, i.e., juvenile crime and early marriage.[27]

We also examine heterogeneous effects along two additional dimensions: father's income (Columns (5) and (6)) and mother's employment (Columns (7) and (8)), both measured when children were two years old. For the analysis by father's income, we stratified the sample according to whether the father's real annual income was below or above the sample median (28,400 NIS - equivalent to 8,200 US$ in 2021).[28] The impact of universal preschool tends to be similar for children from low versus high income fathers for most outcomes, while the decline in women's early marriage comes mainly from households with low income fathers. The impact of preschool on human capital outcomes is higher for children of non-employed mothers.

We also examine heterogeneity in treatment effects with respect to children's predicted outcomes. We predict outcomes for each individual using a prediction model that uses student-level covariates for the prereform cohorts, separately for boys and girls. For each outcome of interest, we divide the entire population into tertiles based on the value of the predicted outcome and estimate equation (1) separately for each of the tertiles. This allows us to study how the effect of universal preschool education varies across individuals whose expected performance would have been low, medium, or high absent the reform.

The results of the heterogeneity analysis with respect to predicted outcomes are shown in Table 8. The effects on high school performance is larger for those with low and medium predicted outcomes than for those located in the highest tertile. Notably, for the latter group we see a substantial increase in postsecondary enrollment. Our results are similar when we stratify the sample by using a single predicted outcome, namely, the likelihood of obtaining a matriculation certificate, and estimate our DID model for all outcomes based on this stratification (see Table A4). Overall, universal preschool education benefited different children at different margins. It had a large impact in high school performance among the most disadvantaged children. At the same time, it also benefited more advantaged children by improving their achievement in post-secondary

[27] We also estimated heterogeneous effects stratifying the sample by number of siblings and by parity (i.e. first born versus later born children). Estimates (not reported here to save space) did not point to a consistent pattern of heterogeneity along these dimensions.

[28] We assign a value of zero to fathers with no earnings during the year. Therefore, the annual median income is quite low.

enrollment. Our results stress the importance of studying multiple outcomes across different population groups to properly assess the effects of universal preschool education.

Intermediate Outcomes in Elementary and Middle School

Test Scores

In order to investigate potential mechanisms for the effects we found on individuals' long-term outcomes, we also investigate intermediate outcomes measured in elementary and middle school. For this analysis, we focus on a subsample of individuals for which we have data on achievement in the GEMS exams in elementary and middle school. The GEMS exams are standardized tests administered by the National Authority for Measurement and Assessment of Education (RAMA) in Israel to students in the fifth and eighth grades in four subjects: native language (i.e., Arabic), English, math, and science.

The administration of the GEMS exams is designed so that only a national representative sample of schools is tested each year.[29] Such design imposes some challenges for our estimation methodology. First, it implies that we have a smaller sample for the estimation of the effect of universal preschool on test scores in a given subject. Second, the cohort fixed-effect (λ_t) of our main DID specification in equation (1) is affected by the sample composition of the localities in which GEMS exams are administered for each cohort.[30] To circumvent this problem, we replace the cohort fixed-effect with a cohort-by-test-year fixed-effect, effectively comparing localities that took the GEMS exams in exactly the same years.

Estimates of this DID specification with 95% confidence intervals are presented in Figure 5. We find that the most pronounced effect of universal preschool was on individuals' native language skills (Arabic). Test scores in Arabic increased significantly by 0.12 standard deviations in fifth grade. Notably, the effect persisted also in eighth grade, where the test scores in Arabic improved by 0.17 standard deviations. We also find an effect on math test scores of 0.20 standard deviations in fifth grade but we find no such

[29] All localities are grouped into four groups, where each group constitutes a representative sample of all Israeli schools. Each cluster is tested in every other year in only two subjects: math and native language, or science and English (as a foreign language). Thus, students in a given school are tested in the same subject only once in four years. However, the localities in our study did not fully comply with this official test-taking calendar.

[30] Since the sampling design is supposed to provide a representative sample of the entire population of schools, the potential bias should vanish for a large sample of localities that fully comply with the official test-taking calendar. However, our analysis sample encompasses a limited number of localities (37).

effect in eighth grade. Thus, it seems that either the beneficial effects on math achievements diminish over time (as in Deming, 2009, and other studies that examine the short- versus long-term effects of preschool education) or that the math skills that are tested in the fifth grade are not highly correlated with the math skills tested in the eighth grade. Our results are consistent with Felfe et al. (2015) who examine the effects of a universal preschool reform in Spain during the 1990s on tenth-grade achievement scores, and find a 0.15 increase in reading scores, and no effect on math achievements. The large improvement in Arabic test scores may explain the sharp increase in enrollment in higher education documented in Section 5. This aligns with the findings of Aucejo and James (2021), who assert that verbal skills play a pivotal role in explaining the variation in university enrollment among individuals, with their marginal effect being more than twice as large as that of math skills.

We find no significant effect of universal preschool education on children's performance in English and science in the fifth and eighth grades. At first blush, this seems to contradict some of our previous findings, which show a significant increase in the number of English units and science subjects included in the high school matriculation exams. However, one should bear in mind that science and English skills are not directly taught in preschools. Rather, based on the evidence of Heckman et al. (2013), it is likely that participation in preschool boosted children's non-cognitive skills such as academic motivation, persistence, and initiative in learning, which are needed to succeed in the matriculation exams. This explanation further supported by the distinction between matriculation exams, which are high-stakes tests influencing access to higher education and certain jobs, and the GEMS tests, which are low-stakes assessments designed to evaluate general trends in the Israeli public education system.[31]

[31] An alternative explanation for the different results across subjects in elementary school and middle school could be a ceiling effect or insufficient variation in test scores. However, this unlikely because the standard deviation in test scores is similar across subjects and grades. Another possible explanation could be that achievement in some of these tests is not related to long term outcomes. While we cannot estimate the causal effect of test scores on long term outcomes we note that test scores in each of the four subjects in 5th and in 8th grade are associated with the long term outcomes examined in our study, even after controlling for students' background characteristics (based on the comparison group sample or on the pretreatment cohorts). Nevertheless, when including test scores in the four subjects together, we observe a larger coefficient for Arabic and math relative to science and English in models that predict the probability of obtaining a matriculation certificate or postsecondary enrollment, which is consistent with our larger treatment effects on test scores for these two subjects in 5th grade.

Learning Environment

We use data from the GEMS student questionnaire for the years 2002–2013 to examine how universal preschool education affected the learning environment in elementary and middle school. Students were asked to indicate the extent to which they agree with a number of statements on a 6- or 5-point Likert scale ranging from 1 (strongly agree) to 5 or 6 (strongly disagree). In order to have consistent outcomes for ease of interpretation, we construct binary indicators that take a value of one if respondents partially or strongly agree with each statement, and 0 otherwise.[32] Our specification is similar to equation (1), where we control for the type of school (Druze, Bedouin, or other Arab) and fixed effects for cohort, locality, grade, and year of test. We do not include students' covariates as the questionnaires are completely anonymized.

The results in Table 9 show that students who received universal preschool education experienced a better learning environment in elementary and middle school, as they were significantly more likely to report that they enjoyed school (5.3 percentage points, or a 7% increase) and that students tended to help each other in class (3.6 percentage points, or a 5% increase). In addition, they were significantly less likely to report frequent noise in the classroom (3.6 percentage points, or a 5% decrease).

Students in the treated cohorts also reported a greater sense of safety and security. They were 7.8 percentage points (27%) less likely to report that they are sometimes afraid to go to school, and they were also 3.3 percentage points (4%) more likely to report that teachers help prevent violence and maintain discipline. In addition, the teacher–student relationship improved, as the share of students who reported having a good relationship with teachers increased by 3.8 percentage points (5%) and they were also 6 percentage points (13%) less likely to report being insulted by teachers.

To rule out the possibility that these findings stemmed from unobserved differential trends or confounding factors, we also examined the effects on additional students' questionnaire items that are not expected to be affected by universal preschool education,

[32] In 2007, which is roughly the middle of the sample period, the format of the student questionnaire was revised, some questions were modified, and the Likert scale was extended from 1 to 5 to 1 to 6. Therefore, we focus on a specific subset of questions that remained very similar or identical throughout the sample period. Note that the foregoing changes to the student questionnaire are not expected to bias our estimates for the following reasons: (1) we include year fixed effects, and (2) the year of the format change does not overlap with the year of the reform implementation as it occurred during the prereform period for some cohorts and during the postreform period for other cohorts.

such as computer use at home and at school in different subjects. Reassuringly, the estimated effects for all of these outcomes are insignificant. The lack of an effect on computer use at school also suggests that the positive effects we found on the educational outcomes are unlikely to be confounded by an increase in school inputs in treated localities for the cohorts that received universal preschool education.

In summary, we find that one possible mechanism explaining the impact of universal preschool on long-term outcomes is the creation of a more favorable, safer, and more conducive learning environment in elementary and middle school. These findings suggest that the provision of universal preschool affected not only the *complier* population of children who enrolled in preschool as a result of the Law, but also the entire cohort of students and the teachers in treatment localities. All benefited from the enhanced learning environment.

Maternal Employment

One possible channel that could explain the improved outcomes of children who had access to universal preschool is an increase in maternal employment and household income. In traditional societies such as the Arab community, women were the primary caregivers for children. Access to universal preschool due to the Preschool Law could have encouraged women to go to work and increased household income. We examine this channel in Appendix B and find no evidence for a significant increase in mothers' employment or income during the period analyzed in this study ruling out these channels of impact.

Robustness and Falsification Tests

We conduct several robustness tests to assess the feasibility of our identification assumption and make sure that our findings are not driven by unobserved differential trends in the treatment and comparison localities. We describe and report these tests in detail in Appendix C and summarize them here.

We begin by assessing the sensitivity of our results to the inclusion of the set of background characteristics used in our main specification. We also estimate models that include a linear time trend interacted with a locality's socioeconomic cluster or socioeconomic ranking (together with the baseline linear trend) (Table A7). In addition, we conduct a placebo analysis where we estimate our main DID model using only the prereform cohorts, assuming the Law was implemented two years before it actually came into effect (Table A8). These tests show no evidence for significant differential pretrends between treatment and comparison localities, supporting our main identification assumption.

We also examine the robustness of our results to different dynamics in outcomes in different type of localities or ethnic groups (Table A9). In particular, we show that our estimates are similar when we include in the comparison group only the always treated or only the never treated localities or when we exclude different subgroups of the treatment or comparison sample (e.g. Bedouin or Druze). We also show that our results are not driven by any specific locality by reestimating our model dropping one locality each time (Figure A5).

To address concerns that the estimated effects are driven by changes in inputs at later stages of schooling, we examine whether there were differential changes in class size in elementary and secondary school between treated and comparison localities, finding no evidence of such differences (Table A10).

Family Fixed Effects

Our comprehensive data allow us to identify siblings and estimate a model with family fixed effects. In this case, we compare the outcomes of children who were young enough to have access to universal preschool in contrast to their older siblings who were already over the age of 4 when the reform was implemented in treatment localities and the outcomes of children and siblings born in the same years in comparison localities.

A comparison of the estimates of the family fixed effects model and the estimates from the baseline DID model provides also interesting insights regarding the extent of intra-household resource allocation. For example, a larger impact within rather than across families might suggest that parents reinforce differences in human capital investments between their children. By contrast, a smaller impact within rather than across families might suggest that families compensate human capital investments. Alternatively, differences between estimates from the two models might point to unobserved trends or shocks at the locality level that could have biased our baseline DID estimates.

In Table 10 we report the estimates of the family fixed effects model. To ease comparison, we report the estimates of the baseline DID model in Column (1). In Column (2) we report the estimates of the DID model after we restrict the sample to families who have at least two children, since the family fixed effects model is based on this sample. The estimates of DID model based on the restricted sample are almost identical to our main estimates but they are slightly less precise due to the reduction in sample size. In Column (3) we report the estimates of the family fixed effects model. These are remarkably similar to those of the DID model but they are slightly noisier due to the addition of family fixed effects. The similarity in the estimates of our main DID model and of the family fixed effects model provides further evidence for the validity of our main identifying assumption.

7. Comparison with Other Preschool Programs and with Alternative School Interventions Implemented in Israel

To put our results in perspective, we compare them to the results obtained in the existing literature for other universal or large-scale preschool education programs as well as for small-scale targeted programs. So far, we have reported intention-to-treat (ITT) estimates for the effects of universal preschool education. They are interesting for policy purposes as they shed light on the effect of providing access to universal preschool education. They also provide information on the overall effect of universal preschool education on all children, including those who did not attend public preschool but lived in treatment localities and could have been indirectly affected. To compare our results with those of other studies, we report here local average treatment effects (LATE) by scaling up our intention-to-treat (ITT) estimates by the increase in public preschool enrollment generated by the reform (about 60 percentage points).[33]

Table 11 reports a comparison between our estimates and those of other studies on the effects of preschool education. We focus on the most comparable outcomes across studies, which are high school graduation and college enrollment. The ITT effect on high school graduation obtained in our study is 0.028, which implies a LATE estimate of about 5 percentage points (a 6% increase relative to the baseline outcome mean). This effect is within the range of other studies that examine the effects of large-scale preschool education programs, although it is located at the lower end of the distribution of these estimates. Note, however, that the baseline mean for our study population is higher than in other studies and might explain the lower impact on this outcome. In fact, there seems to be a negative relationship between the effect of preschool education on high school graduation rates and the baseline outcome mean when we compare across studies. At the other end, we observe a much larger effect on college enrollment in our study relative to other studies: 6.7 percentage points, or a 26% increase. This, again, might derive from the

[33] Appendix Table A11 reports DID estimates for the effects of the Law on public preschool enrollment based on aggregate data at the locality level weighted by population size.

fact that baseline college enrollment was relatively low in our sample population relative to those of other studies.

Panel B of the table summarizes results from the literature that focuses on targeted programs. Our estimates are in this case smaller for both outcomes compared to those obtained in targeted programs. Nevertheless, most of these studies seem to find beneficial effects mostly on girls while we find that universal preschool education increased human capital for both genders.

In Table 12, we also compare our results with estimates from studies that examine the impact of educational interventions implemented in Israel during the same period that were targeted at older ages. We focus on two high school interventions that report causal estimates for a subset of comparable outcomes. We compare the costs of each intervention and the estimated gains.[34] Lavy and Schlosser (2005) examine the effects of remedial education provided to underperforming high school students who were at the margin of obtaining a matriculation certificate. The per-student cost of this intervention was $1,100, while the estimated cost of universal preschool provision is $1,400. Remedial education generated an increase of 13 percentage points in the probability of obtaining a matriculation certificate among treated students. The effect in absolute terms is larger than that of universal preschool education (13 percentage points versus 7 percentage points) and the improvement relative to the outcome means are 24% for remedial education and 17% for universal preschool education. Nevertheless, the effect of universal preschool education is substantially larger in the long term: Lavy et al. (2022) find an 8 percentage point increase (15% relative to the outcome mean) in enrollment to low-tier higher education institutions (colleges), with no effect on enrollment in high-tier such institutions (universities). In our study, we find that universal preschool education increased enrollment in higher education institutions by 9 percentage points (a 27% increase), with positive effects in almost all tiers of higher education, including universities.

The second intervention, examined by Angrist and Lavy (2009), provided monetary awards to high school students from low-achieving high schools on the basis of their success in the matriculation exams. The cost of the intervention was relatively low, only

[34] The two interventions were implemented during the same period on different cohorts, and so there is no concern about overlap between the populations. In addition, only a small proportion of Arab students participated in the two interventions. Unfortunately, since the subsample of Arab students is relatively small in the two studies, these studies do not report separate estimates for the Arab population.

$385 per student, as it provided the monetary award only to students who achieved the target. The authors find a significant increase of 14 percentage points in the probability of obtaining a matriculation certificate for girls, with no significant effect for boys. Although this is a larger effect on matriculation rates compared to what we find in our study, they find no effect in the longer term on university enrollment, and find only a localized effect on postsecondary enrollment in second tier institutions for girls located in the top quartile of the achievement distribution.

In summary, our results, contrasted with the outcomes of these two high school interventions implemented in Israel, indicate that universal preschool education entails higher costs compared to interventions targeting high school students, but the longer-term benefits in terms of postsecondary attainment appear to be significantly larger. A more comprehensive comparison should include the rate of return in terms of dollars spent and embed also the monetary benefits of additional outcomes such as criminal activity, early marriage, and fertility. We plan to assess this in future work, when the cohorts exposed to universal preschool education enter the labor market.

8. Summary and Conclusions

This study provides a comprehensive set of findings regarding the impact of universal preschool education within a disadvantaged population, specifically the Arab population in Israel. Our results indicate that access to universal preschool at ages 3 and 4 benefited individuals across various stages. It enhanced children's language skills during elementary and middle school and raised their performance in fifth-grade math exams. In high school, universal preschool education decreased the likelihood of dropping out of school, raised participation in the matriculation exams, increased the eligibility for a matriculation certificate, and improved the quality of the certificate achieved, as reflected in the number of math and English units, and the number of science subjects. The probability of enrollment in postsecondary education also increased significantly, for both academic and vocational institutions. We also find beneficial effects of universal preschool education on additional long-term outcomes: a decline in the probability of engaging in juvenile crime among boys and in the probability of marrying at an early age among girls.

Possible mediating factors for the long-term benefits of universal preschool education include significant improvements in the learning environment during elementary and middle school. Students reported greater enjoyment of school, a higher sense of safety, fewer in-class disturbances, and better enforcement of discipline in the classroom, as well as better relationships with their teachers and classmates.

We find that universal preschool education affected different children at different margins. It had a larger impact on high-school performance for children from low or medium socioeconomic backgrounds, whereas it increased the probability of postsecondary enrollment for children from higher socioeconomic backgrounds. The long-term impact of universal preschool education on postsecondary enrollment is larger relative to other educational interventions implemented in Israel among high school students during the same period, emphasizing the importance of human capital investments at younger ages.

One possible lesson from our study is that disadvantaged communities can benefit from universal preschool education, even in the absence of well-targeted educational programs. Free universal preschool education can provide stimuli and social experience for disadvantaged children, which they cannot always get in their family environment. While there is a growing interest in the effects of universal preschool education on individuals' outcomes and achievements, there are almost no studies that examine its implementation in a traditional non-Western society. We believe that the Arab-Israeli experience can be a useful example, showing positive short- and long-term benefits of providing universal preschool education to disadvantaged communities.

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Figure 1: Geographical Distribution of the Localities of the Study

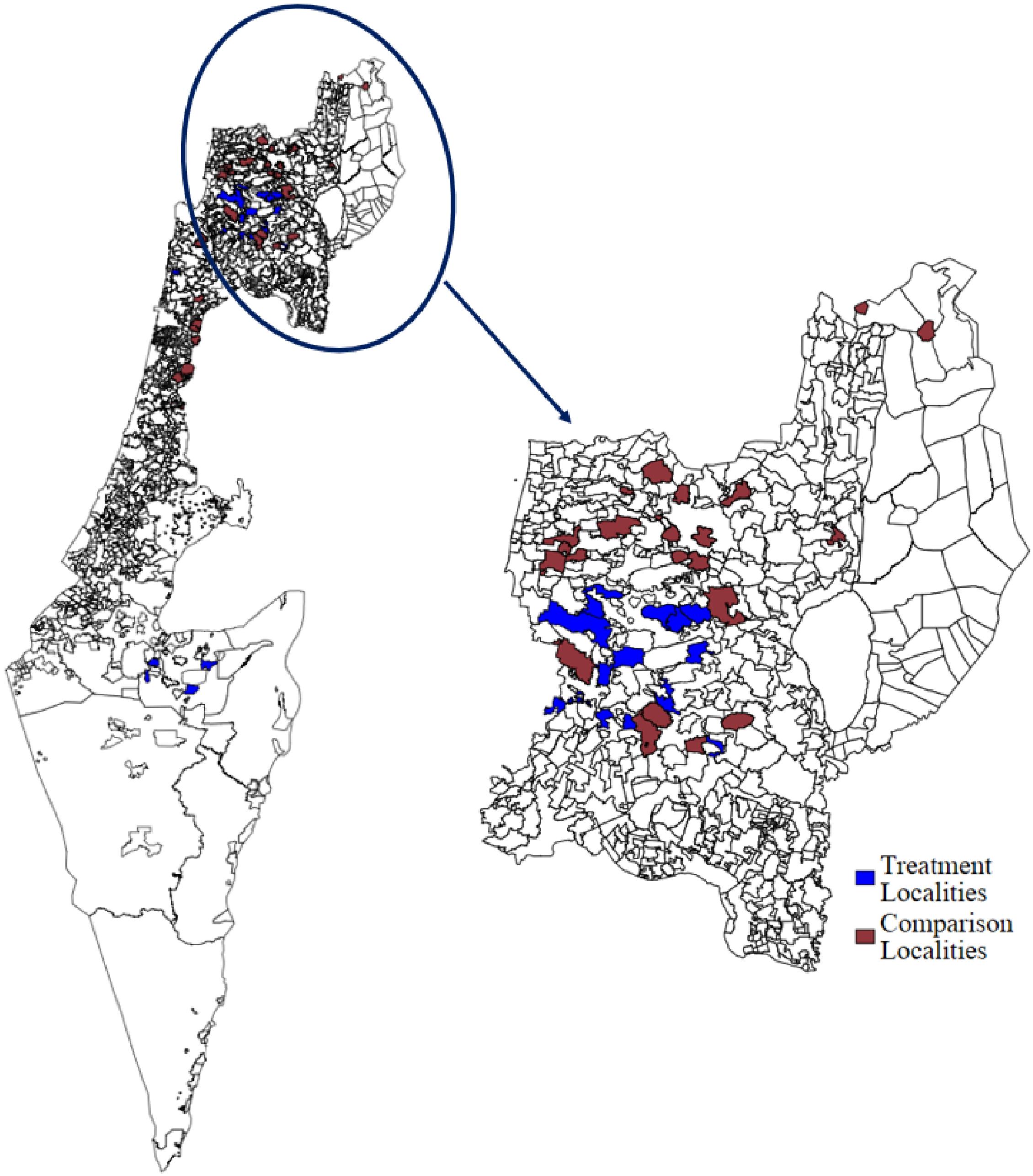
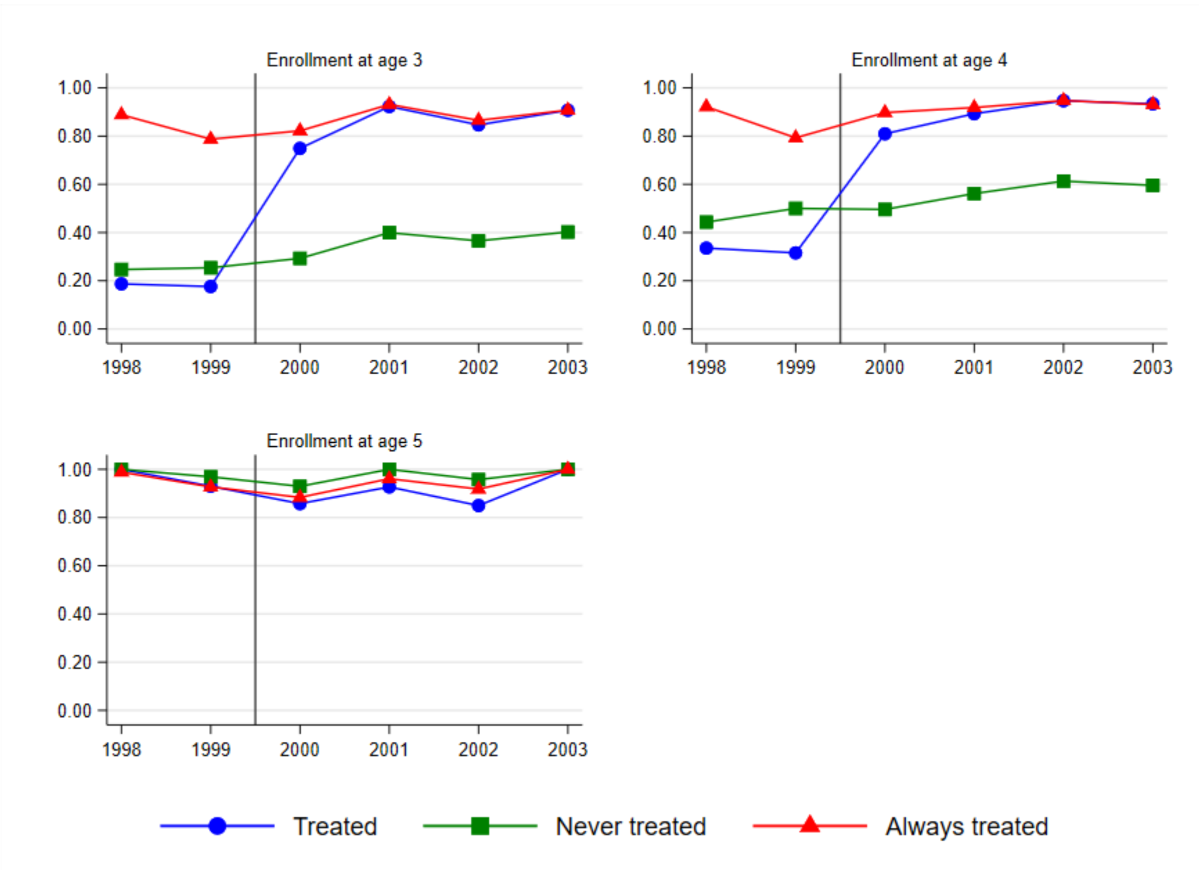


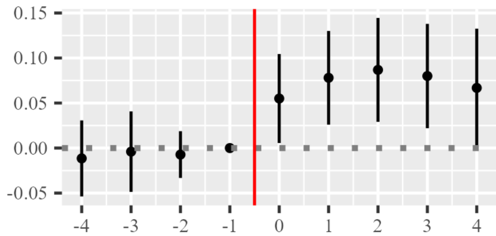
Figure 2: Preschool Enrollment in the Localities of the Study (North district) – 1998-2003



Notes: This figure shows preschool enrollment rates of Arab children by year in different groups of localities, according to their treatment status. The sample includes only localities from the North district. The analysis is based on aggregated enrollment data and population counts data by locality and year provided by the Israeli Central Bureau of Statistics. Treated localities received universal preschool education starting from the year 2000. Non-treated localities are those that were not included in the first phase of the Law implementation. Always-treated localities include localities that received preschool subsidies before the Law implementation.

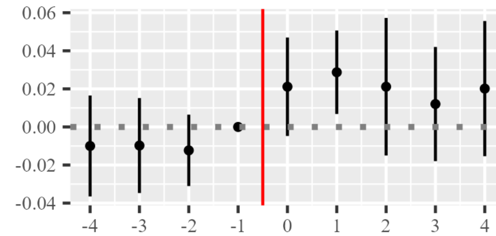
Figure 3: Event-Study Estimates of the Effects of Universal Preschool

(a) High school performance z-score



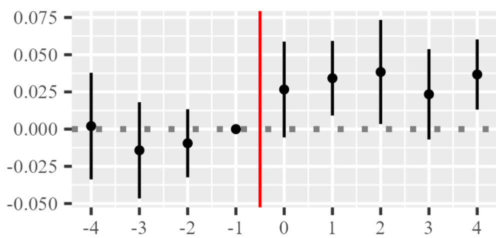
p-value pre-period: 0.616
p-value post-period: 0.000

(b) Graduated from high school



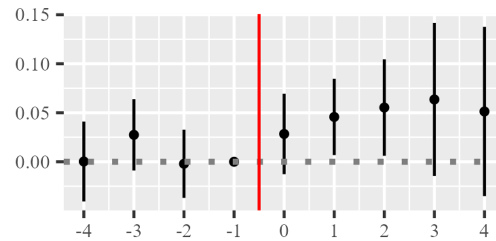
p-value pre-period: 0.252
p-value post-period: 0.091

(c) Participated in the mat. exams



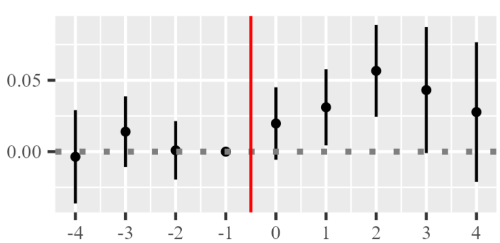
p-value pre-period: 0.536
p-value post-period: 0.007

(d) Matriculation certificate



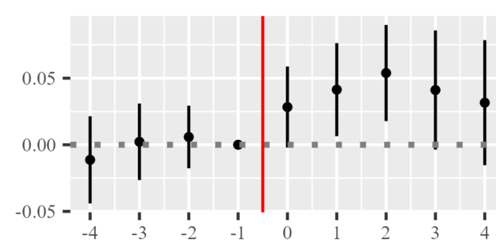
p-value pre-period: 0.575
p-value post-period: 0.054

(e) University-eligible certificate



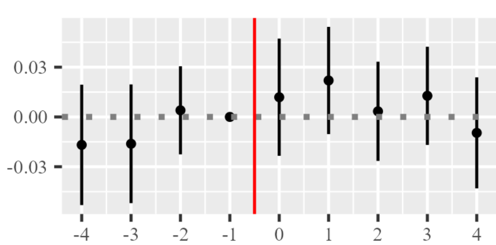
p-value pre-period: 0.675
p-value post-period: 0.005

(f) 4+ English units



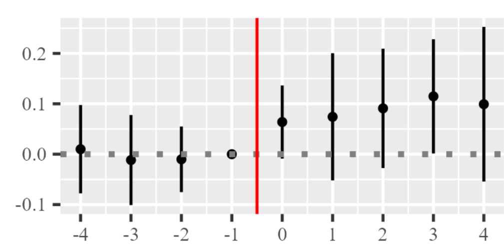
p-value pre-period: 0.919
p-value post-period: 0.011

(g) 4+ Math units



p-value pre-period: 0.516
p-value post-period: 0.537

(h) Number of science subjects

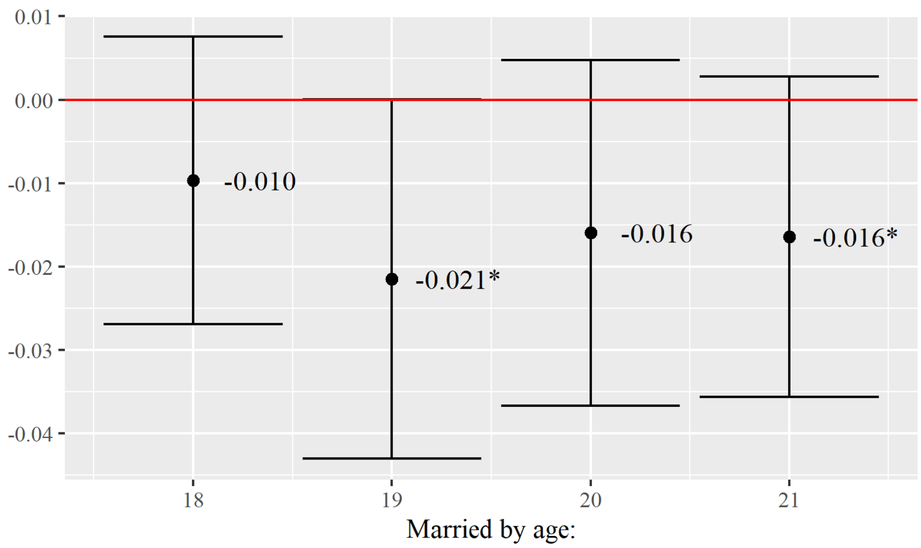


p-value pre-period: 0.900
p-value post-period: 0.065

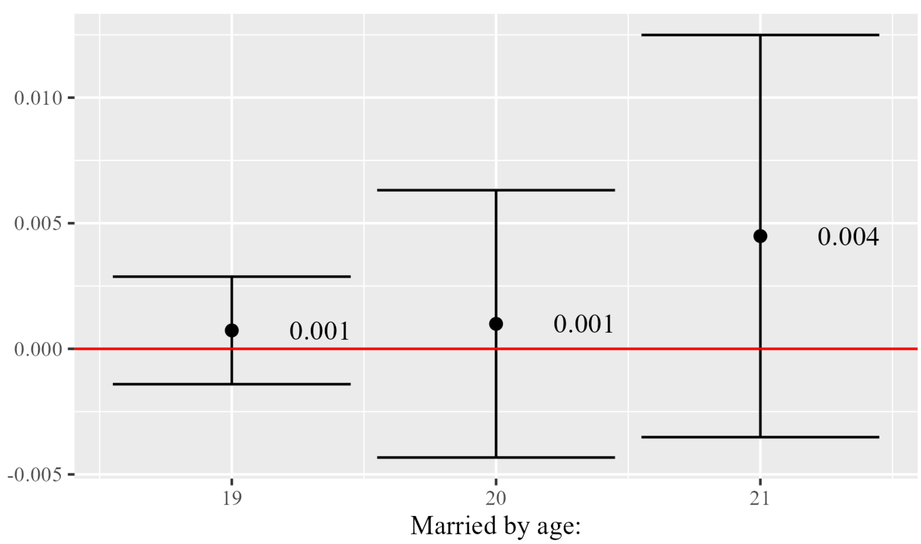
Notes: The figures plot the pretreatment and postreatment effects along 95 percent confidence intervals on high school outcomes, based on an event-study specification. The x-axis represents the years before and after the Law implementation. Year zero represents the first year of the Law implementation. The specification includes locality and cohort fixed effects and controls for parental education, mother's employment and father's earnings (in deciles) when the child was 2 years old, number of siblings, and religion. The sample includes Israeli Arabs from localities in the north, born between 1991-1999. Standard errors are clustered at the locality level. p-values for a joint test of significance for the coefficients in the pre or the post-reform period are reported at the bottom of each subfigure.

Figure 4: Impact of Universal Preschool on Individuals' Probability of Marrying at Young Age

(a) Women



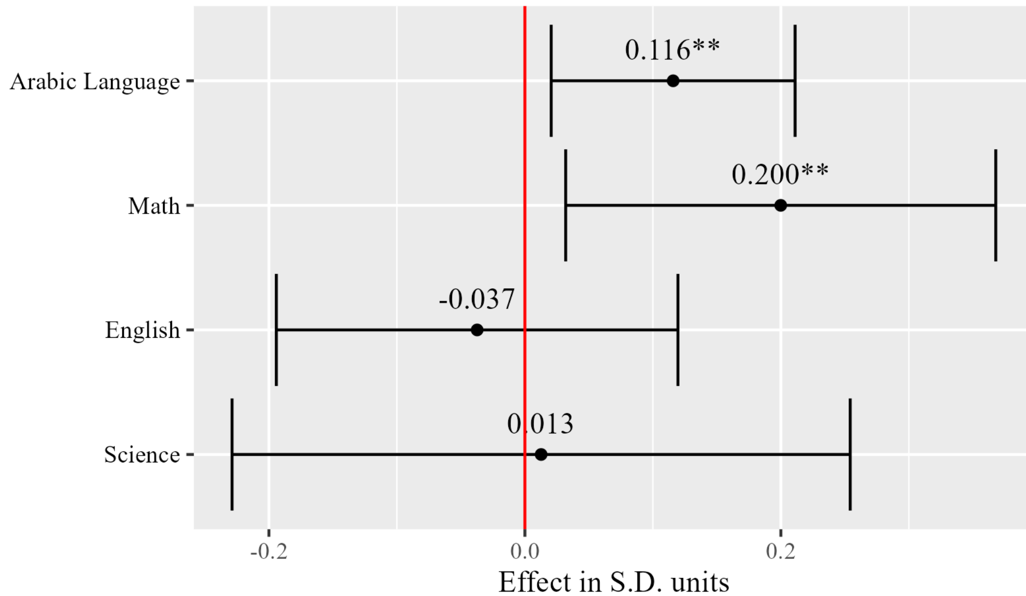
(b) Men



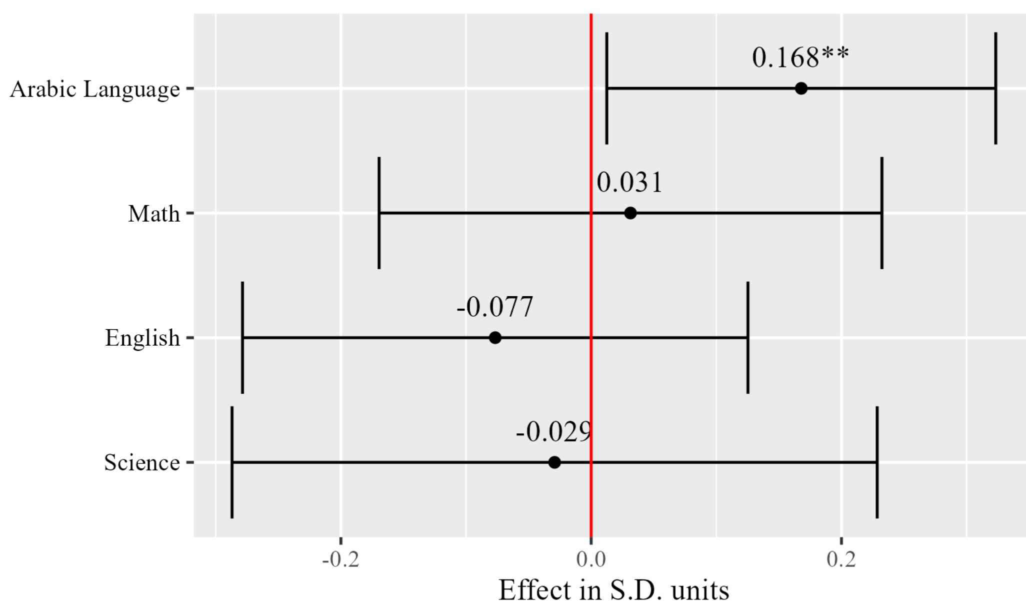
Notes: The figure reports DID estimates and 95 percent confidence intervals of the effects of universal preschool on the probability of marrying by age 18, 19, 20, and 21, based on the specification in equation (1). The specification includes locality and cohort fixed effects and controls for parental education, mother's employment and father's earnings (in deciles) when the child was 2 years old, number of siblings, and religion. The sample includes Israeli Arabs from localities in the north, born between 1991-1999. Standard errors are clustered at the locality level. *p<.0.10, **p<0.05, *** p<0.01.

Figure 5: Impact of Universal Preschool on 5th and 8th Grade Test Scores

(a) 5th Grade



(b) 8th Grade



Notes: The figure DID estimates and 95 percent confidence intervals of the effects of universal preschool on test scores in 5th and 8th grade. The specification includes locality and cohort-by-test-year fixed-effect and controls for parental education, mother's employment and father's earnings (in deciles) when the child was 2 years old, number of siblings, and religion. The sample includes Israeli Arabs from localities in the north, born in 1991-1999. Standard errors are clustered at the locality level. The original scale of the test-scores is between 0 and 100. We removed a negligible number of observations with scores below 0 or above 100. p*<0.10, **p<0.05, *** p<0.01

Table 1: Descriptive Statistics - Treatement and Comparison Localities

	Treatment	Comparison	Difference
	(1)	(2)	(3)
Population size	8,865	9,564	-700
	(6,090)	(12,550)	(3,109)
Median age	18.33	21.90	-3.57***
	(1.50)	(2.59)	(0.70)
Dependency ratio	121.69	102.79	18.90***
	(14.71)	(12.74)	(4.74)
Families with 4 or more children (%)	0.40	0.30	0.10***
	(0.08)	(0.09)	(0.03)
Income per capita	1,237	1,465	-228**
	(125)	(374)	(90)
Rate of motorization	0.14	0.18	-0.04***
	(0.02)	(0.04)	(0.01)
New motor vehicles (%)	0.16	0.18	-0.02
	(0.04)	(0.04)	(0.01)
Students among aged 20-29 (%)	0.04	0.08	-0.05***
	(0.02)	(0.04)	(0.01)
Entitled to matriculation certificate among aged	0.28	0.42	-0.14***
17-18 (%)	(0.09)	(0.16)	(0.04)
Earners below minimum wage (%)	0.55	0.51	0.03*
	(0.04)	(0.06)	(0.02)
Earners above twice average wage (%)	0.01	0.03	-0.01***
	(0.00)	(0.01)	(0.00)
Recipients of income support (%)	0.03	0.02	0.01***
	(0.01)	(0.01)	(0.00)
Recipients of income supplements to old age	0.46	0.27	0.19***
pension (%)	(0.09)	(0.07)	(0.03)
Number of Localities	15	22	

Notes: This table presents balance tests between the treatment and comparison localities based on characteristics from the 1995 census. Columns (1) and (2) display the means (and standard deviations in parentheses) in each category. The differences in means between the treatment and comparison localities are reported in Column (3) , with robust standard errors in parentheses. *p<.0.10, **p<0.05, *** p<0.01.

Table 2: Descriptive Statistics Prereform Cohorts

	Treatement	Comparison	Difference		Treatement	Comparison	Difference
	(1)	(2)	(3)		(1)	(2)	(3)
Panel A: pre-treatment covariates				**Panel B: outcomes**			
Father's years of	9.92	10.65	-0.73***	Completed high	0.80	0.83	-0.03
education	(3.19)	(3.20)	(0.24)	school	(0.40)	(0.37)	(0.03)
Mother's years of	9.42	10.13	-0.71*	Participated in the	0.76	0.79	-0.03
Education	(3.09)	(3.04)	(0.38)	matriculation exams	(0.43)	(0.40)	(0.03)
Father employed in 1998	0.67	0.66	0.01	Matriculation	0.40	0.46	-0.06
	(0.47)	(0.47)	(0.02)	certificate	(0.49)	(0.50)	(0.04)
Mother employed in 1998	0.13	0.18	-0.05***	University-eligible	0.30	0.37	-0.07***
	(0.33)	(0.38)	(0.02)	matriculation	(0.46)	(0.48)	(0.02)
Father's monthly wages in	4,942	5,941	-999***	4+ English units	0.36	0.45	-0.08***
1998	(3,926)	(4,780)	(177)		(0.48)	(0.50)	(0.03)
Mother's monthly wages	2,743	2,973	-230	4+ Math units	0.20	0.23	-0.03
in 1998	(1,979)	(2,368)	(164)		(0.40)	(0.42)	(0.02)
Number of siblings	3.65	3.06	0.59***	Number of science	0.51	0.52	-0.01
	(2.11)	(1.80)	(0.14)	subjects	(0.74)	(0.70)	(0.07)
Share of females	0.49	0.48	0.00	Any juvenile criminal	0.17	0.13	0.03*
	(0.50)	(0.50)	(0.00)	record (men)	(0.37)	(0.34)	(0.02)
Share of Druze	0.00	0.25	-0.25***	Participated in the	0.39	0.41	-0.02
	(0.01)	(0.43)	(0.09)	psychometric exam	(0.49)	(0.49)	(0.03)
Share of bedouin	0.21	0.03	0.18*	Average	471.67	483.67	-11.99
	(0.40)	(0.17)	(0.10)	psychometric score	(111.65)	(113.02)	(8.29)
				Any postsecondary	0.33	0.39	-0.06**
				enrollment	(0.47)	(0.49)	(0.03)
Number of localities	15	22		Married by age 21	0.32	0.23	0.09**
Number of observations	14,454	21,253		(women)	(0.47)	(0.42)	(0.04)

Notes: This table presents balance tests between treatment and comparison groups for various characteristics of the prereform cohorts. Columns (1) and (2) display the means (and standard deviation in parentheses) in each category. The differences in means between the treatment and comparison localities are reported in Column (3), with standard errors clustered at the locality level. *p<.0.10, **p<0.05, *** p<0.01.

Table 3: Impact of Universal Preschool on High School Achievement

Dependent Variable	Full Sample (1)	Boys (2)	Girls (3)
High school performance z-score	0.079***	0.073***	0.086***
	(0.020)	(0.026)	(0.024)
	-0.058	*-0.298*	*0.197*
Graduated from high school	0.028**	0.030	0.026**
	(0.012)	(0.019)	(0.011)
	0.802	*0.690*	*0.920*
Participated in the matriculation exams	0.037***	0.050***	0.023**
	(0.011)	(0.016)	(0.011)
	0.763	*0.635*	*0.898*
Matriculation certificate	0.043*	0.022	0.066**
	(0.023)	(0.022)	(0.030)
	0.396	*0.278*	*0.522*
University-eligible certificate	0.033**	0.020	0.048**
	(0.013)	(0.013)	(0.018)
	0.300	*0.198*	*0.407*
4+ English units	0.040**	0.029*	0.053**
	(0.016)	(0.017)	(0.020)
	0.364	*0.252*	*0.482*
4+ Math units	0.015*	0.018**	0.012
	(0.009)	(0.007)	(0.014)
	0.197	*0.140*	*0.258*
Number of science subjects	0.092**	0.098**	0.089*
	(0.041)	(0.038)	(0.046)
	0.688	*0.484*	*0.904*
Number of observations	84,457	43,362	41,095

Notes: This table shows DID estimates of the impact of universal preschool on various educational outcomes. The specification includes locality and cohort fixed effects, and controls for parental education, mother's employment and father's earnings (in deciles) when the child was 2 years old, number of siblings, and religion. Mean outcomes of the prereform cohorts (born between 1991-1994) in the treatment localities are reported in italics. Standard errors in parentheses are clustered at the locality level. The high school performance z-score reported in the first row of the table, is constructed by computing a standarized average of all standarized individual outcomes. * p<.0.10, **p<0.05, *** p<0.01

Table 4: Impact of Universal Preschool on Psychometric Test Performance

Dependent Variable	Full Sample (1)	Boys (2)	Girls (3)	Dependent Variable	Full Sample (1)	Boys (2)	Girls (3)
Took the Psychometric Exam	0.028***	0.037***	0.020*	Took the Psychometric Exam by age 19	0.033***	0.045***	0.023**
	(0.008)	(0.009)	(0.010)		(0.008)	(0.009)	(0.010)
	0.389	*0.252*	*0.534*		*0.350*	*0.213*	*0.494*
Total Score				Quantitative Score			
above first quartile (≥400)	0.022***	0.033***	0.010	above first quartile (≥85	0.025***	0.034***	0.017**
	(0.006)	(0.007)	(0.009)		(0.005)	(0.006)	(0.008)
	0.269	*0.181*	*0.362*		0.284	0.197	0.377
above second quartile (≥470)	0.017***	0.021***	0.013	above second quartile (≥99)	0.020***	0.024***	0.016**
	(0.006)	(0.006)	(0.009)		(0.005)	(0.006)	(0.007)
	0.177	*0.126*	*0.230*		0.188	0.142	0.238
above third quartile (≥580)	0.009	0.015***	0.002	above third quartile (≥119)	0.011**	0.021***	0.001
	(0.005)	(0.005)	(0.008)		(0.005)	(0.005)	(0.007)
	0.069	*0.051*	*0.088*		0.088	0.071	0.106
Verbal Score				English Score			
above first quartile (≥80)	0.016**	0.030***	0.002	above first quartile (≥78)	0.025***	0.033***	0.017
	(0.006)	(0.007)	(0.009)		(0.008)	(0.008)	(0.011)
	0.269	*0.171*	*0.373*		*0.249*	*0.166*	*0.336*
above second quartile (≥93)	0.017**	0.025***	0.009	above second quartile (≥88)	0.021***	0.026***	0.016
	(0.006)	(0.007)	(0.009)		(0.007)	(0.007)	(0.011)
	0.188	*0.122*	0.258		0.149	0.103	0.197
above third quartile (≥109)	0.011**	0.014**	0.009	above third quartile (≥107)	0.005	0.008	0.001
	(0.005)	(0.006)	(0.007)		(0.007)	(0.005)	(0.011)
	0.094	*0.064*	0.125		0.070	0.050	0.092
Number of Observations	84,457	43,362	41,095	Number of Localities	37	37	37

Notes: This table shows DID estimates of the impact of universal preschool on participation and achievement in the Israeli psychometric exam. The specification includes locality and cohort fixed effects, and controls for parental education, mother's employment and father's earnings (in deciles) when the child was 2 years old, number of siblings, and religion. Mean outcomes of the prereform cohorts (1991-1994) in the treatment localities are reported in italics. Standard errors in parentheses are clustered at the locality level. * p<.0.10, **p<0.05, *** p<0.01.

Table 5: Impact of Universal Preschool on Postsecondary Education

	Ever Enrolled			Enrolled by Age 19		
	Full Sample	Boys	Girls	Full Sample	Boys	Girls
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)
Postsecondary enrollment	0.053***	0.066***	0.041***	0.034***	0.025***	0.044***
	(0.010)	(0.014)	(0.014)	(0.006)	(0.006)	(0.011)
	0.332	*0.245*	*0.423*	*0.157*	*0.103*	*0.214*
Enrolled at Academic Institution	0.040***	0.044***	0.036**	0.028***	0.015***	0.041***
	(0.008)	(0.009)	(0.013)	(0.006)	(0.005)	(0.011)
	0.262	*0.147*	*0.384*	*0.121*	*0.057*	*0.189*
University (first-tier)	0.040***	0.033***	0.048***	0.029***	0.017***	0.041***
	(0.006)	(0.007)	(0.009)	(0.004)	(0.004)	(0.007)
	0.148	*0.088*	*0.212*	*0.068*	*0.036*	*0.102*
Second tier academic institution	0.023***	0.022***	0.024***	0.005	-0.001	0.011
	(0.005)	(0.004)	(0.008)	(0.004)	(0.003)	(0.007)
	0.071	*0.057*	*0.086*	*0.024*	*0.017*	*0.031*
Teacher training institution	-0.014**	-0.005**	-0.025**	-0.006*	-0.001	-0.011*
	(0.006)	(0.002)	(0.011)	(0.003)	(0.001)	(0.006)
	0.067	*0.015*	*0.122*	*0.030*	*0.004*	*0.057*
Enrolled at Vocational postsecondary institution	0.020***	0.030***	0.010**	0.007**	0.009**	0.004
	(0.007)	(0.010)	(0.005)	(0.003)	(0.004)	(0.003)
	0.080	*0.108*	*0.051*	*0.036*	*0.046*	*0.026*
Number of Localities	37	37	37	37	37	37
Number of Observations	74,452	38,198	36,254	74,452	38,198	36,254

Notes: This table shows DID estimates of the impact of universal preschool on postsecondary enrollment. The specification includes locality and cohort fixed effects, and controls for parental education, mother's employment and father's earnings (in deciles) when the child was 2 years old, number of siblings, and religion. Mean outcomes of the prereform cohorts (1991-1994) in the treated localities are presented in italics. Standard errors in parentheses are clustered at the locality level. * p<.0.10, **p<0.05, *** p<0.01.

Table 6: Impact of Universal Preschool on Juvenile Crime

Dependent Variable	Full Sample (1)	Boys (2)	Girls (3)
Any juvenile criminal offense	-0.015**	-0.030***	-0.000
	(0.006)	(0.011)	(0.001)
	0.087	*0.166*	*0.004*
Security/order criminal offense	-0.004	-0.008	-0.000
	(0.004)	(0.007)	(0.001)
	0.046	*0.088*	*0.002*
Life/body criminal offense	-0.011***	-0.022***	0.001
	(0.003)	(0.006)	(0.001)
	0.047	*0.089*	*0.002*
Sex/property criminal offense	-0.008*	-0.017**	-0.000
	(0.004)	(0.008)	(0.001)
	0.040	*0.077*	*0.001*
Other criminal offense	-0.002	-0.004	-0.000
	(0.003)	(0.006)	(0.000)
	0.016	*0.030*	*0.001*
Number of localities	37	37	37
Number of observations	84,457	43,362	41,095

Notes: This table shows DID estimates of the impact of universal preschool on the probability of having a juvenile criminal record. The specification includes locality and cohort fixed effects, and controls for parental education, mother's employment and father's earnings (in deciles) when the child was 2 years old, number of siblings, and religion. Mean outcomes of the prereform cohorts (1991-1994) in the treatement localities are presented in italics. Standard errors in parentheses are clustered at the locality level. * p<.0.10, **p<0.05, *** p<0.01.

Table 7: Heterogeneous Effects of Universal Preschool

	Mother's education		Father's education		Father's annual income		Mother's employment	
	<12	≥12	<12	≥12	< median	≥ median	Not Emp.	Employed
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High school performance z-score	0.089***	0.048**	0.085***	0.066***	0.070***	0.083***	0.088***	0.041*
	(0.024)	(0.020)	(0.023)	(0.024)	(0.022)	(0.021)	(0.022)	(0.022)
	-0.222	*0.389*	*-0.211*	*0.311*	*-0.142*	*0.067*	*-0.111*	*0.238*
Took the psychometric exam	0.032***	0.016	0.024***	0.033**	0.019***	0.035***	0.031***	0.017
	(0.008)	(0.015)	(0.007)	(0.013)	(0.007)	(0.013)	(0.007)	(0.017)
	0.306	*0.615*	*0.310*	*0.578*	*0.353*	*0.442*	*0.361*	*0.544*
Postsecondary enrollment by age 19	0.024***	0.039***	0.021***	0.056***	0.023***	0.045***	0.033***	0.039***
	(0.006)	(0.011)	(0.006)	(0.013)	(0.007)	(0.007)	(0.007)	(0.012)
	0.108	*0.291*	*0.115*	*0.258*	*0.138*	*0.186*	*0.142*	*0.240*
Any juvenile criminal offense (men)	-0.030**	-0.025**	-0.027**	-0.033***	-0.029**	-0.031***	-0.027**	-0.047***
	(0.013)	(0.009)	(0.012)	(0.010)	(0.013)	(0.010)	(0.012)	(0.015)
	0.184	*0.115*	*0.186*	*0.117*	*0.181*	*0.143*	*0.167*	*0.157*
Married by age 21 (women)	-0.010	-0.017	-0.008	-0.026	-0.033***	-0.003	-0.015	-0.021
	(0.010)	(0.012)	(0.009)	(0.020)	(0.010)	(0.012)	(0.010)	(0.023)
	0.368	*0.179*	*0.353*	*0.235*	*0.342*	*0.283*	*0.334*	*0.229*
Number of localities	37	37	37	37	37	37	37	37
Number of observations	50,659	33,649	51,462	32,555	42,228	42,229	65,697	18,760

Notes: This table shows DID estimates of the impact of universal preschool on various subsamples. The specification includes locality and cohort fixed effects, and the relevant list of the following controls: parental education, mother's employment and father's earnings (in deciles) when the child was 2 years old, number of siblings, and religion. Mean outcomes of the prereform cohorts (1991-1994) in the treatement localities are presented in italics. Standard errors in parentheses are clustered at the locality level. * p<.0.10, **p<0.05, *** p<0.01.

Table 8: Heterogenous Effects of Universal Preschool by Predicted Outcomes

	Level of Predicted Outcome		
	Low	Medium	High
Dependent Variable	(1)	(2)	(3)
High school performance z-score	0.084***	0.098***	0.033*
	(0.030)	(0.028)	(0.019)
	-0.461	*0.022*	*0.576*
Took the psychometric exam	0.025***	0.030**	0.015
	(0.009)	(0.013)	(0.014)
	0.173	*0.420*	*0.726*
Postsecondary enrollment by age 19	0.018***	0.026***	0.049***
	(0.006)	(0.009)	(0.014)
	0.063	*0.151*	*0.343*
Any juvenile criminal offense (men)	-0.020**	-0.020	-0.011
	(0.009)	(0.013)	(0.014)
	0.082	*0.151*	*0.203*
Married by age 21 (women)	-0.017	-0.005	-0.005
	(0.023)	(0.016)	(0.012)
	0.126	*0.288*	*0.396*

Notes: This table shows the estimated effects of universal preschool, by tertiles of predicted outcomes defined by the pre-treatment relationship between outcomes and background characteristics. The specification includes locality and cohort fixed effects, and controls for parental education, mother's employment and father's earnings (in deciles) when the child was 2 years old, number of siblings, and religion. Mean outcomes of the prereform cohorts (1991-1994) in the treatement localities are presented in italics. Standard errors in parentheses are clustered at the locality level. * p<.0.10, **p<0.05, *** p<0.01.

Table 9: Impact of Universal Preschool on Classroom Environment

Dependent Variable:		Dependent Variable:	
Satisfaction with school and classroom		**"Placebo" outcomes: computer use**	
I enjoy school	0.053***	Computer at home	0.009
	(0.017)		(0.014)
	0.737		*0.753*
Students in my classroom help each other	0.036***	Use of computer in Arabic lessons	-0.001
	(0.012)		(0.036)
	0.750		*0.336*
There are frequent disturbances in the classroom	-0.036**	Use of computer in English lessons	0.002
	(0.018)		(0.026)
	0.763		*0.328*
Safety and security			
Teachers prevent violence/keep discipline	0.033*	Use of Computer in math lessons	0.017
	(0.018)		(0.038)
	0.806		*0.367*
Sometimes I'm afraid to go to school	-0.078***	Use of computer in science lessons	-0.002
	(0.018)		(0.049)
	0.291		*0.459*
I have someone in school to consult with	0.015		
	(0.016)		
	0.736		
Relationship with teachers			
There are good relationships between teachers and students	0.038***		
	(0.013)		
	0.762		
Sometimes teachers insult children	*-0.058****		
	(0.019)	No. of localities	37
	0.460	No. of observations	63,663

Notes: This table shows DID estimates of the impact of universal preschool on various learning environment outcomes, as reflected in students' answers to the GEMS questionnaires in grades 5-9. The outcome is a binary variable that takes the value of one if respondents partially agree to strongly agree, and 0 if respondents partially to strongly disagree. The specification includes locality, cohort, year, and grade fixed effects and controls for the type of school (Arab/Druze/Bedouin). Mean outcomes of the prereform cohorts (1991-1994) in the treatement localities are presented in italics. Standard errors in parentheses are clustered at the locality level. * p<.0.10, **p<0.05, *** p<0.01.

Table 10: Impact of Universal Preschool - Family Fixed Effects Model

Dependent Variable	Locality FE Main Sample (1)	Locality FE Siblings Sample (2)	Family FE Siblings Sample (3)
High school performance z-score	0.079***	0.078***	0.075***
	(0.020)	(0.019)	(0.028)
	-0.058	*-0.046*	*-0.046*
Took the psychometric exam	0.028***	0.031***	0.040***
	(0.008)	(0.007)	(0.013)
	0.389	*0.395*	*0.395*
Postsecondary enrollment by age 19	0.034***	0.035***	0.027***
	(0.006)	(0.007)	(0.010)
	0.157	*0.157*	*0.157*
Any juvenile criminal offense (men)	-0.030***	-0.038***	-0.035**
	(0.011)	(0.012)	(0.015)
	0.166	*0.173*	*0.173*
Married by age 21 (women)	-0.016*	-0.021	-0.017
	(0.009)	(0.014)	(0.025)
	0.318	*0.342*	*0.342*
Number of localities	37	37	37
Number of observations	84,457	69,591	69,591

Notes: This table shows estimates of the impact of universal preschool. The specification includes locality fixed effects in Columns (1) and (2), and family fixed effects in Column (3). All specifications include also cohort fixed effects and control for parental education, mother's employment and father's earnings (in deciles) when the child was 2 years old, number of siblings, and religion. Mean outcomes of the prereform cohorts (1991-1994) in the treatement localities are presented in italics. Standard errors in parentheses are clustered at the locality level. * p<.0.10, **p<0.05, *** p<0.01.

Table 11: Comparison to Similar Studies - Local Average Treatment Effects

Study	Country and type of preschool	Age at intervention	High School Graduation Effect	High School Graduation Baseline mean	College enrollment Effect	College enrollment Baseline mean
A. Large Scale Programs						
Gray-Lobe et al. (2023)[1]	Universal, US (Boston)	4	0.060	0.64	0.054	0.65
Havnes and Mogstad (2011)[2]	Universal, Norway	3-6	0.058	0.74	0.069	0.37
Deming (2009)[3]	Head Start, US	3-5	0.086	Unknown	0.057	Unknown
Bailey et al. (2021)[4]	Head Start, US	3-5	0.024	0.92	0.054	0.64
This study[5]	**Universal, Israeli Arabs**	**3-4**	**0.047**	**0.80**	**0.067**	**0.26**
B. Targeted Programs						
Belfield et al. (2006)[6]	Perry Preschool, US	3-5	0.168	0.60 (at age 40)		
Campbell et al. (2012)[7]	Abecedarian, US	0-6	0.068	0.82	0.17	0.06
Heckman et al. (2010)[8]	Perry Preschool, US	3-5	0.61 (girls) -0.03 (boys)	0.23 (girls) 0.51 (boys)		
Anderson (2008) - high school[9] Elango et al. (2016) - college[10]	Abecederian, US	0-6	0.23 (girls) -0.10 (boys)	0.61 (girls) 0.74 (boys)	0.193	Unknown

Notes: [1] Estimates based on Column (2) of Table IV and Column (8) of Table III. [2] Estimates based on Column (1) and (4) of Table IV. [3] Estimates based on Column (1) of Table V. [4] Estimates based on columns (1) and (6) of Table I. [5]Estimates for high school graduation are based on Column (1) of Table 3. Estimates for college enrollment are based on Column (1) of Table 5 using enrolled at Academic Institution. Both estimates are inflated by the increase in preschool enrollment (0.6). [6] Estimates based on Table 1 and authors' calculations. [7] Estimates based on Table 3 and discussion on page 10 of the paper. The paper studies obtaining Bachelor's degree rather than college enrollment. [8] Estimates based on Columns (2) and (3) of Table III and Columns (2) and (3) of Table V. [9] Estimates based on Columns (3)-(4) and (8)-(9) of Table 6. [10] Estimates based on Figure 4.6.

Table 12: Comparison to Other Educational Interventions Implemented in Israel at Older Ages

					Matriculation certificate		Postsecondary enrollment	
Study	Intervention	Target population	Age	Cost per student (2000)	Effect	Baseline mean	Effect	Baseline mean
Lavy and Schlosser (2005) Lavy et al. (2022)	Remedial education	Underperforming students at the margin of obtaining matriculation certificate in low achieving schools	15-18	$1,100	0.13	0.55	0.08 (comes from college with no effect on university enrollment)	0.55
Angrist and Lavy (2009)	Monetary awards to students	Students in 39 low achieving high schools (10 Arab schools)	15-18	$385	0.14 girls no effect for boys	0.24 all 0.29 girls 0.2 boys	No effect overall. No effect on university enrollment. Increase in postsecondary enrollment at second tier institutions for girls in the top quartile: 0.164	0.43 (girls in top quartile of achievement distribution)
This study	**Universal preschool**	**Israeli Arabs in low SES localities**	**3-4**	**$1,400**	**0.07**	**0.4**	**0.09 (effects also on university enrollment)**	**0.33**

Notes: This table compares the long-term impacts of universal preschool to the impacts of other interventions implemented in Israeli high schools during the same period. The outcomes selected for comparison are the likelihood of obtaining a matriculation certificate and the likelihood of enrolling in a postsecondary institution. Estimates for the impact of universal preschool are scaled-up by the increase in preschool enrollment generated by the implementation of the law (60%). Estimate for the effect of remedial education on the likelihood of obtaining a matriculation certificate is taken from columns 1-3 of Table 2 of Lavy et al. (2021), which is identical to the estimate reported in columns 1-3 pf Table 8 in Lavy and Schlosser (2005). The baseline mean for this outcome is computed by subtracting the treatment effect (0.13) from the outcome mean of the treated group (0.681) reported in column 2 of Table 2 in Lavy et al. (2001). The estimate of the impact of remedial education on postsecondary enrollment is taken from columns 1-3 of Table 3 of Lavy et al. (2021). The baseline mean for this outcome is computed by subtracting the treatment effect (0.08) from the outcome mean of the treated group (0.631) reported in column 2 of Table 3 in Lavy et al. (2001). Estimates for the effects of monetary awards on the probability of obtaining a matriculation certificate are based on Table 2 columns 3 and 5 from Angrist and Lavy (2009) and are scaled-up by treatment take-up (75%). Estimates of the monetary awards on postsecondary enrollment are based on Panel C of Table 8 from Angrist and Lavy (2009) and are scaled-up by treatment take up (75%).

ONLINE APPENDIX

Appendix A - Assessing the Parallel trend assumption

We perform a sensitivity analysis suggested by Rambachan and Roth (2023). We focus on the treatment effect on the index of high school performance to summarize our results and gain statistical power. We plot the results in figure A1, where the blue line in each subfigure plots the confidence interval of the treatment effect for period 1 obtained on our DID model. Panel (a) plots the confidence intervals of the treatment effect allowing for violations of the linear pre-trend up to a parameter M (i.e. sensitivity analysis using smoothness restrictions). The figure shows that the treatment effect would still be positive and significant if we allow for the difference in trends between the treated and control groups to be linear (M=0). The breakdown value for a significant effect is at M=0.006, which is roughly 30% of the s.e. of the treatment effect of the high school index. We also apply the second approach proposed by Rambachan and Roth (2023) and plot the results in Panel (b) (i.e., sensitivity analysis using relative magnitudes restrictions). In this figure, we plot the confidence intervals for the treatment effect allowing for a post-treatment violation of parallel trends to be no larger than $\overline{\mathrm{M}}$ times the maximum pre-treatment violation of the parallel trend. The breakdown point is $\overline{\mathrm{M}} \approx 1.1$, meaning that we can rule out a null effect unless we allow for violations of parallel trends that are 1.1 times larger than the maximum violation observed in the pre-period. To sum up, both approaches suggest that our results would remain significant even if we allow for some deviations of the parallel trends assumption.

Appendix B – Impact on Maternal Employment and Earnings

We examine the impact of universal preschool on maternal employment and earnings using two approaches. We first estimate the same DID model (equation (1)) based on our main children's sample using as outcomes different measures of mothers' labor market outcomes: indicators for mothers' employment at ages 3 through 6, number of months worked, and log wages. The model uses the same list of controls as before and we also control for mother's age and age squared.

We report the results in Table A5. In Column (1) we report estimates for the full sample and in Columns (2) and (3) we report estimates for subsamples stratified by mothers' education. Mothers' employment in the prereform period was extremely low. Only 17% of the mothers of children aged 3-5 worked. The employment rate of mothers with less than high school (who account for 60% of our sample) is even lower, only 11%. Overall, there was no change in employment rates, months worked, or wages among mothers of children who received universal preschool. Estimates for all outcomes are positive but small and are not statistically significant.

As an alternative strategy, we use the mothers as a unit of analysis and estimate DID models comparing labor market outcomes of mothers of children aged 3-5 five years before and after the implementation of universal preschool in treated and comparison localities (1995-2004).[1] Such a strategy allows us to compare the effects of preschool exposure among mothers of preschool-aged children with a "placebo" effect among other mothers of children who are not preschool-aged in the same set of localities. In this way, we can rule out the possibility that the results are spuriously driven by time-varying labor market conditions that differentially affected treatment and comparison localities, such as the 2001-2002 recession in Israel.[2] As in table A5, we estimate the models using the full sample and subsamples stratified by mothers' education. Estimates reported in Table A6 show no significant effects of universal preschool provision on the labor supply or wages of mothers of children aged 3-5, nor for mothers who have children of other ages. We therefore conclude that universal preschool had no significant effect on mothers' employment or income during the period analyzed in this study. As a result, we can rule out increases in mothers' employment and income as possible channels that could explain the positive impacts we find on children's outcomes.

Appendix C – Additional Robustness Checks

We detail here robustness checks that assess the robustness of our results.

We begin by assessing the sensitivity of our results to the inclusion of the set of background characteristics used in our main specification. Our results are reported in Table A7. To ease comparison, we report in Column (1) our main results. In Column (2) we report estimates from a simple DID model that includes only time and locality fixed effects. Estimates from this simple specification remain very similar to our baseline specification, reinforcing the assumption that the results are not driven by differential changes in observable characteristics (or unobserved characteristics correlated with observed covariates) between treatment and comparison localities.

Given that the reform was implemented in localities classified with the lowest socioeconomic ranking, it could be argued that our results are driven by a convergence

[1] We also select mothers' of children aged 5 in this sample as most children turn 5 when they attend preschool since the preschool cutoff date is on September 1st.
[2] Israel experienced an economic recession in these years due to two main factors: (1) outburst of the violent period of the second Intifada (2) bust of the Dot-com bubble (see Bank of Israel, 2002, 2003).

over time between lower and higher SES localities that could have occurred even without the opening of preschools. To assess this, we present in Columns (3) and (4) of the same table estimates from a model that includes a linear time trend interacted with a locality's socioeconomic cluster (1 to 4) or socioeconomic ranking (1 to 203) (together with the baseline linear trend).[3, 4] The estimates remain largely similar to our main results. Some of the estimates are smaller, but most remain significant. Note that the interaction between a time trend and socioeconomic ranking or, alternatively, socioeconomic cluster is highly correlated with the "*Exposed_preschool"* indicator, our main variable of interest, and therefore it is not surprising that some of the estimated effects are smaller.

We also conduct a placebo analysis where we estimate baseline DID equation on all main outcomes, including only the prereform cohorts, and assume that the Law was implemented in the middle of the prereform period, two years before it actually came into effect. Most estimates shown in Table A8 are small and insignificant, and have inconsistent signs across outcomes. Thus, we find no evidence for significant differential pretrends between treatment and comparison localities, supporting our main identification assumption of no differential trends in the postreform period.

A last check we perform relates to the experimental setup. Note that our comparison group is composed of two different groups of localities: those that did not receive universal preschool education during the period we cover in this study (never treated) and those that already had preschool education before the implementation of the Law due to their special status (always treated).

In some settings, such as a staggered DID design, it is problematic to use early-treated units as a comparison group to late-treated units (e.g., Callaway and Sant'Anna, 2021; Roth et al., 2023). We explain in Section 2 in the main text why this is less of a concern in our setup. Nevertheless, we report in Table A9 the results of the estimation where we use only one specific group of localities as a comparison group: never treated (Column (2)) or always treated (Column (3)). To ease comparison, we also report in Column (1) our main estimates. Overall, most of our results hold when we use only one type of localities as a comparison group.

In Columns (4) to (6) of the same table, we assess the robustness of our results to additional issues related to the sample composition. Given that we have a relatively small

[3] The national ranking of the localities in our sample lay within the range of 8 to 138. A lower ranking implies lower socioeconomic status.

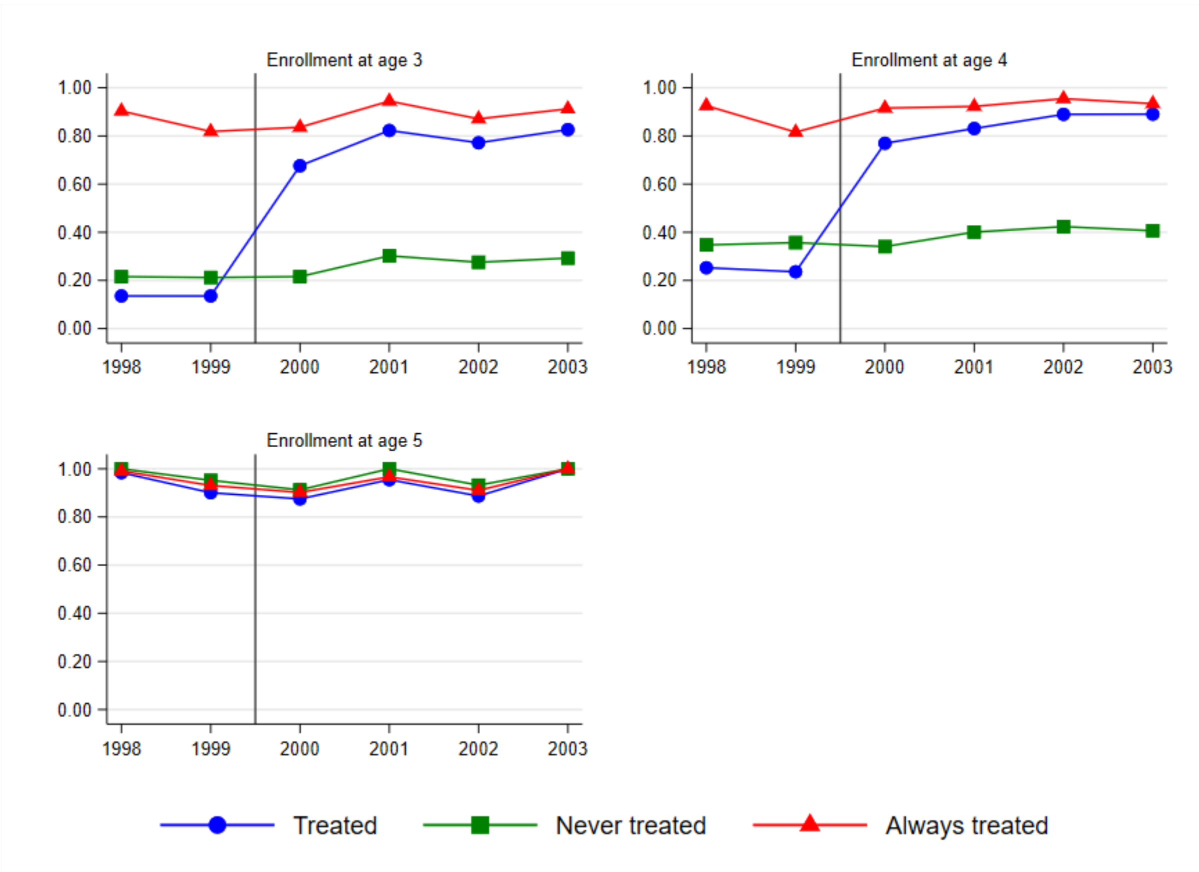
[4] We do not allow for a specific linear trend for each cluster or ranking as this would absorb most of the treatment effects (see, e.g., Meer and West, 2016; Goodman-Bacon, 2021).

sample of localities (37), we wanted to make sure that our results do not derive from a particular group of localities. We first reestimated our model by omitting the city of Nazareth, which accounts for 16% of the sample, and is by far the largest Arab locality in the sample (Column (4)). We then reestimated our model omitting all Druze localities, given that all of them are included in the comparison group (Column (5)). Finally, we reestimated our model omitting all Bedouin localities, given that most of them are included in the treatment group (Column (6)). Despite these changes in the composition of the localities in our sample, all estimates are highly similar to our main results, providing further support for the validity of our identification strategy. The robustness of our results across these different subsamples also suggests that our results are not driven by ethnic-specific trends within the Arab community in Israel.

As a final check to assess the sensitivity of our results, we reestimated our model by dropping one locality each time to make sure that our main results do not derive from any particular locality. In Figure A5 we plot estimates along 95% confidence intervals for our main outcomes from these subsamples along with our main results. All figures are reassuring in showing that our main results do not derive from any particular locality.

An additional concern is that perhaps other changes might have taken place during the same period that could have affected the performance of children in treatment or comparison localities. In particular, we should be concerned about other differential investments in educational inputs across treatment and comparison localities. We can examine one such potential input: average class size. Using supplemental data from local authorities' statistical yearbooks compiled by the CBS, we compute average class size for individuals in both the pre- and postreform cohorts throughout their elementary, middle, and high school years and estimate a simple DID specification that includes locality and cohort fixed effects using the average class size as an outcome. Estimates for the postreform cohorts in treatment localities, reported in Table A10, are inconsistent across schooling stages and none of them are statistically or economically significant.

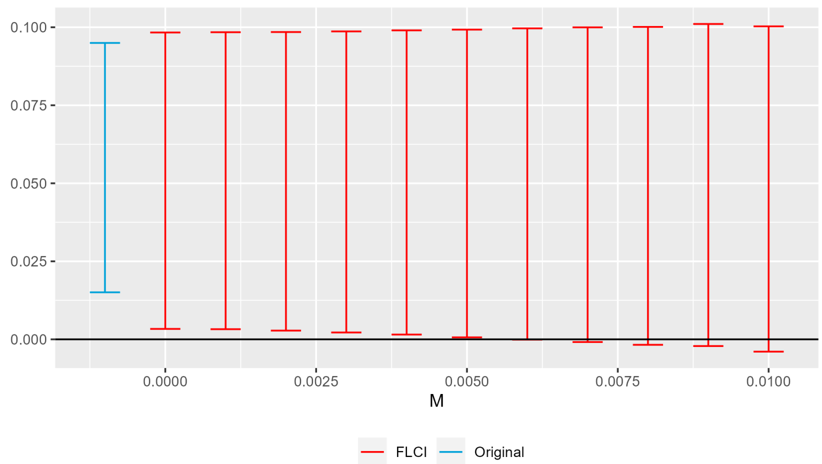
Figure A1: Preschool Enrollment in Arab Localities in Israel – 1998-2003



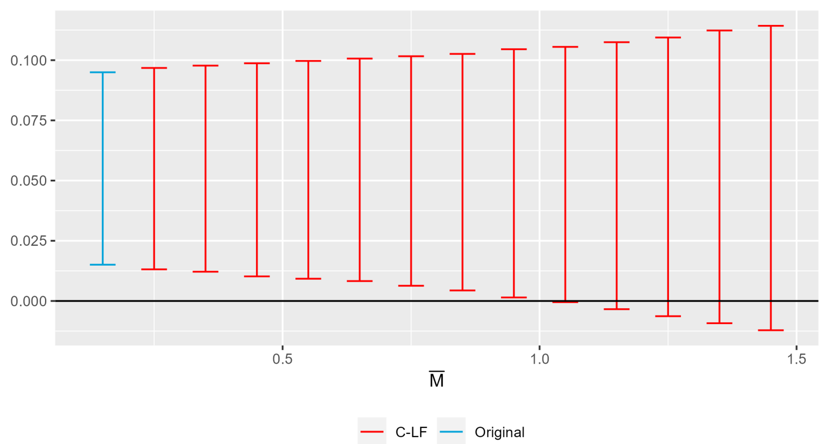
Notes: This figure shows preschool enrollment rates of Arab children by year in different groups of localities, according to their treatment status. The analysis is based on aggregated enrollment and population counts data by locality and year provided by the Israeli Central Bureau of Statistics. Treated localities received universal preschool education starting from the year 2000. Non-treated localities are those that were not included in the first phase of the Law implementation. Always Treated localities include localities that received preschool subsidies before the Law implementation.

Figure A2: Sensitivity Analysis for the Treatment Effect on High School Performance to Violations of the Parallel Trends Assumption

(a) Smoothness Restriction

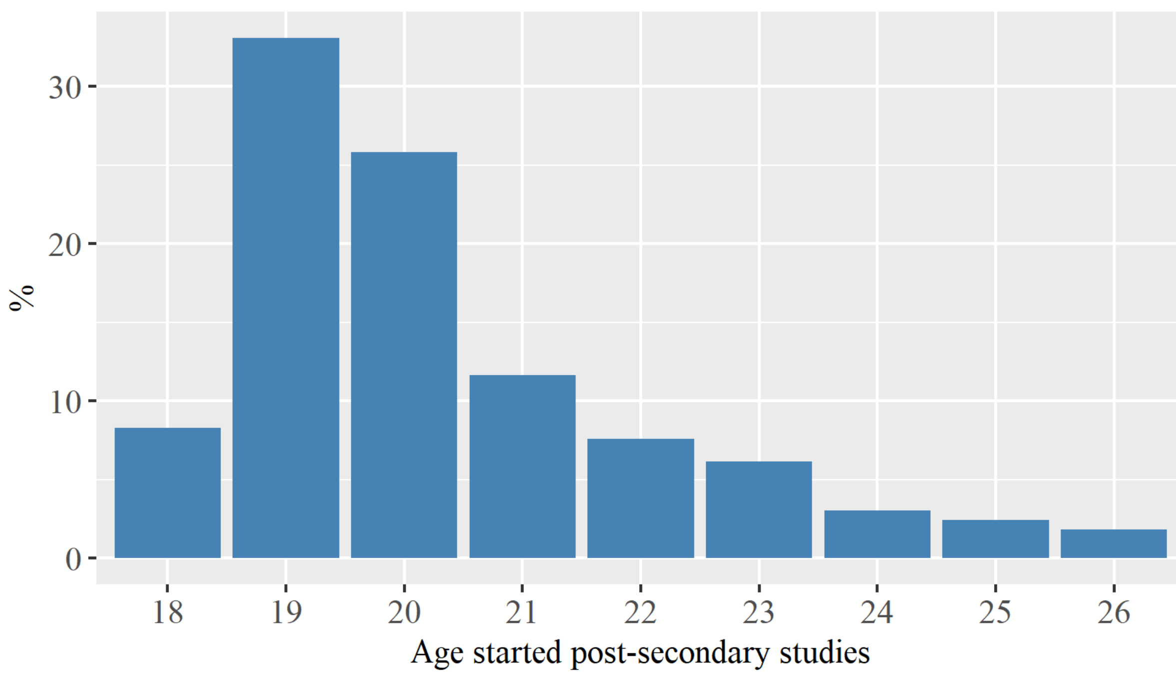


(b) Bounds on Relative Magnitude



Notes: The figure reports 90% confidence intervals for the effect of universal preschool on the index of high school outcomes in the first period after the reform in Blue and a sensitivity analysis for the effect under possible violations of the parallel trends assumptions following the method proposed by Rambachan and Roth (2023). Panel (a) plots in red the confidence intervals of the treatment effect allowing for violations of the linear pre-trend up to a parameter M (sensitivity analysis using smoothness restrictions). Panel (b) plots the confidence intervals for the treatment effect allowing for a post-treatment violation of parallel trends to be no larger than $\bar{M}$ times the maximum pre-treatment violation of the parallel trend (sensitivity analysis using relative magnitudes restrictions).

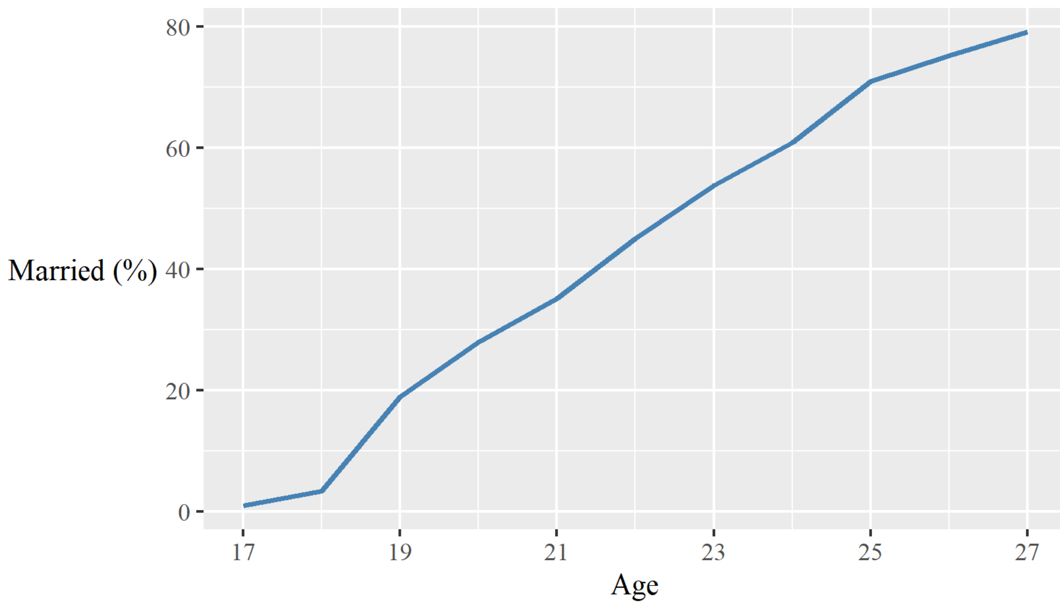
Figure A3: Age Distribution at Enrollment in Postsecondary Institutions (Prereform cohort born in 1991)



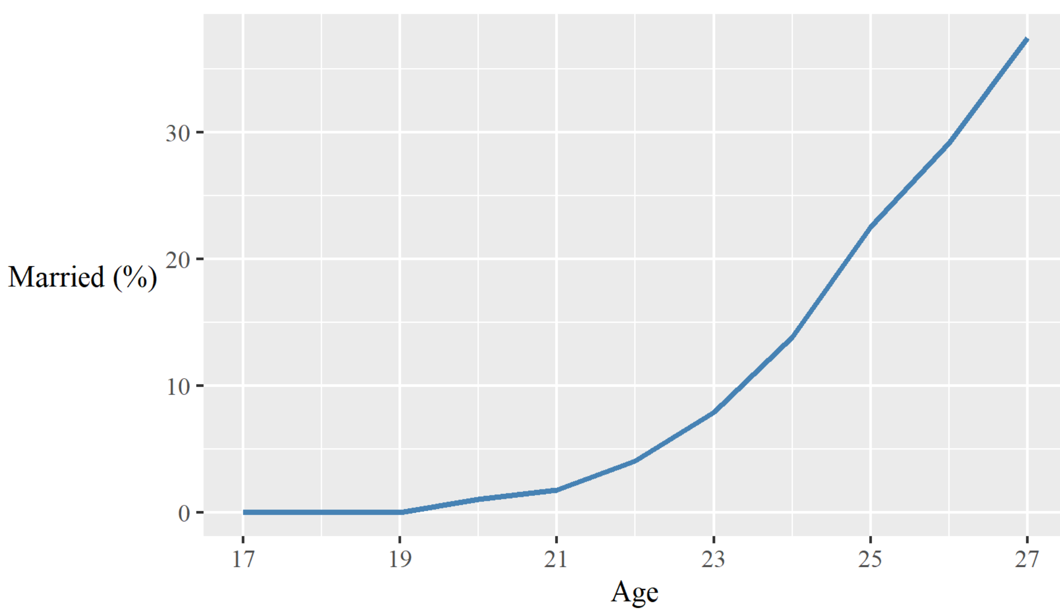
Notes: This figure reports the age distribution at first enrollment in a postsecondary education institution for the 1991 birth cohort included in our sample. Enrollment data is available until the 2017-2018 academic year.

Figure A4: Share of Married Individuals, by Age

(a) Women

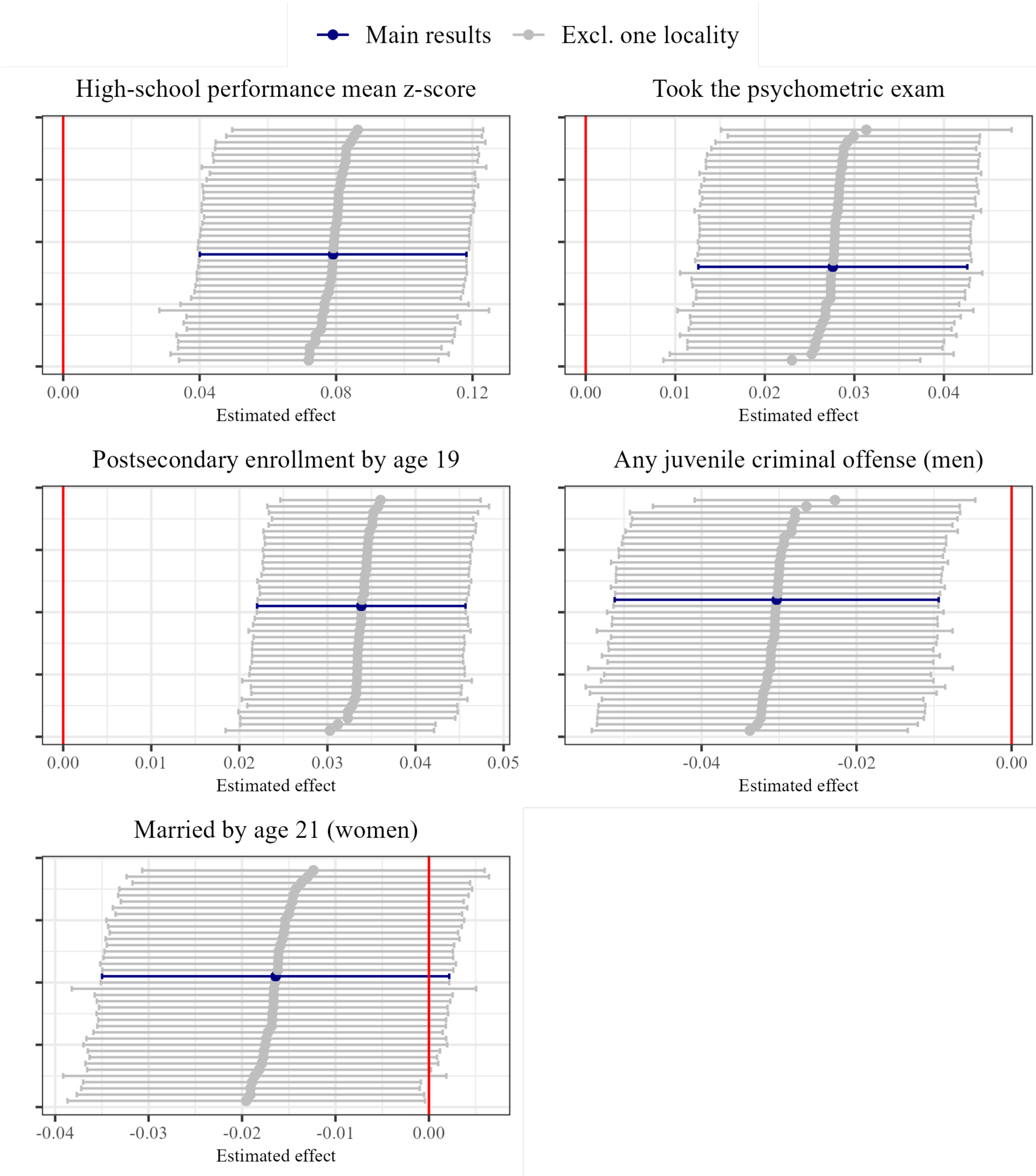


(b) Men



Notes: This figure plots the share of married individuals by age for the prereform cohort born (born in 1991) in the localities of this study.

Figure A5: Sensitivity Analysis of the Impact of Universal Preschool



Notes: The figures plot the distribution of estimates and 95% confidence intervals of our baseline DID specification in equation (1). The blue bars represent estimates for our main sample, and the grey bars represent estimates obtained by excluding one locality from the sample at a time. The specification includes locality and cohort fixed effects and controls for parental education, mother's employment and father's earnings (in deciles) when the child was 2 years old, number of siblings, and religion. The sample includes Israeli Arabs from localities in the north, born between 1991-1999. Standard errors are clustered at the locality level.

Table A1: Prereform and Postreform Cohorts of the Study by Age

Birth Cohort									Age	Outcomes	
PRE				POST							
1991	1992	1993	1994	1995	1996	1997	1998	1999			
1993	1994	1995	1996	1997	1998	1999	**2000**	2001	1-2		
1994	1995	1996	1997	1998	1999	**2000**	2001	2002	2-3		
1995	1996	1997	1998	1999	**2000**	2001	2002	2003	3-4		
1996	1997	1998	1999	**2000**	2001	2002	2003	2004	4-5		
1997	1998	1999	**2000**	2001	2002	2003	2004	2005	5-6		
1998	1999	**2000**	2001	2002	2003	2004	2005	2006	6-7		
1999	**2000**	2001	2002	2003	2004	2005	2006	2007	7-8		
2000	2001	2002	2003	2004	2005	2006	2007	2008	8-9		
2001	2002	2003	2004	2005	2006	2007	2008	2009	9-10		
2002	2003	2004	2005	2006	2007	2008	2009	2010	10-11	GEMS 5	
2003	2004	2005	2006	2007	2008	2009	2010	2011	11-12		Juvenile Crime
2004	2005	2006	2007	2008	2009	2010	2011	2012	12-13		
2005	2006	2007	2008	2009	2010	2011	2012	2013	13-14	GEMS 8	
2006	2007	2008	2009	2010	2011	2012	2013	2014	14-15		
2007	2008	2009	2010	2011	2012	2013	2014	2015	15-16		
2008	2009	2010	2011	2012	2013	2014	2015	2016	16-17		
2009	2010	2011	2012	2013	2014	2015	2016	2017	17-18	High School Graduation, Matriculation, Psychometric Exams, Postsecondary education, Marriage	
2010	2011	2012	2013	2014	2015	2016	2017	2018	18-19		
2011	2012	2013	2014	2015	2016	2017	2018		19-20		
2012	2013	2014	2015	2016	2017	2018			20-21		
2013	2014	2015	2016	2017	2018				21-22		
2014	2015	2016	2017	2018					22-23		
2015	2016	2017	2018						23-24		
2016	2017	2018							24-25		
2017	2018								25-26		
2018									26-27		

Notes: This table shows the prereform and postreform cohorts of the study and their ages at different years in which the outcomes of the study are measured.

Table A2: Description of the Outcome Variables

Variable name	Variable description
High School	
Graduated highschool	=1 if individual was enrolled in 12^{th} grade; 0 otherwise
Participated in the matriculation exams	=1 if individual took at least one matriculation exam; 0 otherwise
Matriculation certificate	=1 if individual obtained a Matriculation certificate; 0 otherwise
University-eligible certificate	=1 if individual has obtained a Matriculation diploma with at least 3 units in math, 4 units in English and at least one subject with 4 units; 0 otherwise
4+ English units	Four or more matriculation units earned in English (0-5).
4+ Math units	Four or more matriculation units earned in math (0-5).
Number of Science Subjects	Number of science subjects taken, as defined by the Israeli Ministry of Education: physics, chemistry, biology, and computer Science.
Psychometric Exam	
Took the psychometric exam (any time/ by age 19)	=1 if individual took the psychometric exam at least once; 0 otherwise (any time/ by age 19)
Psychometric total score	Total score in the psychometric exam (200-800)
Psychometric verbal score	Total score in the verbal (Arabic) section of the psychometric exam (0-150)
Psychometric quantitative score	Total score in the quantitative section of the psychometric exam (0-150)
Psychometric English score	Total score in the English section of the psychometric exam (0-150)
Postsecondary Outcomes	
Postsecondary enrollment	=1 if individual was enrolled in any Israeli postsecondary institution; 0 otherwise
Academic institution	=1 if individual was enrolled in any postsecondary institution with academic degree credentials (university, academic college, or teacher training institution) ; 0 otherwise
University (first tier)	=1 if individual was enrolled in a university, which is a first-tier academic institution in Israel; 0 otherwise
Academic college	=1 if individual was enrolled in an academic college, which is a second-tier academic institution in Israel; 0 otherwise
Teacher training tnstitution	=1 if individual was enrolled in a teacher training institution; 0 otherwise
Vocational institution	=1 if individual was enrolled in a vocational postsecondary institution; 0 otherwise
Juvenile Crime	
Any Juvenile criminal offense	=1 if individual had at least one criminal offense by age 18; 0 otherwise
Security/order criminal offense	=1 if individual had at least one criminal security or order offense by age 18; 0 otherwise
Life/body criminal offense	=1 if individual had at least one criminal life or body offense by age 18; 0 otherwise
Sex/property criminal offense	=1 if individual had at least one criminal sex or property offense by age 18; 0 otherwise
Other criminal offense	=1 if individual had at least one criminal offense in other categories by age 18; 0 otherwise
Marriage	
Married by age 18/19/20/21	=1 if individual was officially married according to the Israeli Marriage Register by age 18, 19, 20, or 21
GEMS exam ("*Meitzav*")	
Arabic (native) language grade	Grade in the Arabic Language GEMS exam (in terms of s.d. units, original scale is 0-100)
Math grade	Grade in the math GEMS exam (in terms of s.d. units, original scale is 0-100)
English grade	Grade in the English GEMS exam (in terms of s.d. units, original scale is 0-100)
Science grade	Grade in the science exam (in terms of s.d. units, original scale is 0-100)

Table A3: Preschool Attendance in Treatment and Never Treated Localities

	Preschool enrollment at age 3	Preschool enrollment at age 4
	(1)	(2)
Father's educ. 12+	-0.018**	-0.013
	(0.009)	(0.009)
Mother's educ. 12+	0.012	0.027
	(0.020)	(0.019)
Siblings above median	-0.016	-0.028*
	(0.011)	(0.016)
Female	0.001	-0.002
	(0.004)	(0.005)
Treatment x		
Father's educ. 12+	0.021*	0.009
	(0.011)	(0.011)
Mother's educ. 12+	0.029	-0.013
	(0.022)	(0.020)
Siblings above median	0.017	0.039**
	(0.012)	(0.017)
Female	-0.007	-0.003
	(0.005)	(0.006)
Outcome mean	0.655	0.814
Cohort FE x Treatement	Yes	Yes
Locality FE	Yes	Yes
Number of observations	26,204	26,204

Notes: This table reports estimates from a regression where the dependent variable is an indicator for preschool attendance at age 3 (Column (1)) and age 4 (Column (2)) and the explanatory variables are family background characteristics and child gender. The models include also interactions between these covariates and a treatment indicator, locality fixed effects, and cohort fixed effects interacted with a treatment indicator. The sample includes treatment and never treated localities. Enrollment data is from the postreform period.

Table A4: Heterogeneous Effects of Universal Preschool by Predicted Likelihood of Matriculation

	Predicted Likelihood of Matriculation		
	Low	Medium	High
Dependent Variable	(1)	(2)	(3)
High school performance z-score	0.083***	0.104***	0.032
	(0.030)	(0.028)	(0.023)
	-0.449	*0.031*	*0.588*
Took the psychometric exam	0.020**	0.040***	0.013
	(0.010)	(0.012)	(0.016)
	0.183	*0.430*	*0.742*
Postsecondary enrollment by age 19	0.016**	0.033***	0.045***
	(0.006)	(0.010)	(0.012)
	0.069	*0.149*	*0.352*
Any juvenile criminal offense (men)	-0.019	-0.032**	-0.029***
	(0.013)	(0.012)	(0.010)
	0.194	*0.163*	*0.099*
Married by age 21 (women)	-0.005	-0.017	-0.023
	(0.016)	(0.017)	(0.021)
	0.392	*0.293*	*0.151*

Notes: This table shows the estimated effect of universal preschool, by tertiles of predicted matriculation eligibility defined by the prereform relationship between matriculation eligibility and background characteristics. The specification includes locality and cohort fixed effects, and controls for parental education, mother's employment and father's earnings (in deciles) when the child was 2 years old, number of siblings and religion. Mean outcomes of the prereform cohorts (1991-1994) in the treatement localities are presented in italics. Standard errors in parentheses are clustered at the locality level. * p<.0.10, **p<0.05, *** p<0.01.

Table A5: Effects of Universal Preschool on Maternal Employment
mothers of the sample of children of the study, born in 1991-1999

Dependent Variable	All Mothers	Mother's Years of Education<12	Mother's Years of Education>=12
Mother Employed (age 3)	0.010	0.011	0.016
	(0.008)	(0.009)	(0.013)
	0.163	*0.099*	*0.334*
Mother Employed (age 4)	0.007	0.005	0.018
	(0.009)	(0.009)	(0.013)
	0.169	*0.105*	*0.342*
Mother Employed (age 5)	0.013	0.012	0.020
	(0.009)	(0.009)	(0.014)
	0.174	*0.106*	*0.358*
Mother's Months Worked (age 3)	0.029	0.040	0.148
	(0.078)	(0.066)	(0.136)
	1.294	*0.633*	*3.085*
Mother's Months Worked (age 4)	0.024	0.018	0.159
	(0.086)	(0.065)	(0.148)
	1.367	*0.682*	*3.224*
Mother's Months Worked (age 5)	0.048	0.007	0.235
	(0.084)	(0.071)	(0.150)
	1.430	*0.726*	*3.339*
Mother's Log Annual Wages in (age 3)	0.032	0.020	0.039
	(0.049)	(0.083)	(0.064)
	8.933	*8.238*	*9.487*
	0.032	0.042	0.020
Mother's Log Annual Wages (age 4)	(0.039)	(0.066)	(0.048)
	9.173	*8.491*	*9.733*
	-0.019	-0.072	0.014
Mother's Log Annual Wages (age 5)	(0.048)	(0.069)	(0.057)
	9.376	*8.747*	*9.878*
Number of localities	37	37	37
Number of observations	84,394	50,745	33,649

Notes: This table shows DID estimates of the impact of exposure to the Preschool Law on maternal employment in ages 3-5. The basic unit of observation are individuals who were children in the years before and after the implementation of universal preschool (born in 1991-1999). The specification includes locality and cohort fixed effects, and controls for parental education, number of siblings and religion. Mean outcomes of the pre-treatment cohorts (1991-1994) in the treated localities are presented in italics. Standard errors in parentheses are clustered at the locality level. * p<.0.10, **p<0.05, *** p<0.01

Table A6: Effects of Universal Preschool on Maternal Employment

Panel Data of mothers living in the localities of the study, 1995-2004

	All		Years of Education<12		Years of Education>=12	
Dependent Variable	Mothers of Children aged 3-5	Other Mothers	Mothers of Children aged 3-5	Other Mothers	Mothers of Children aged 3-5	Other Mothers
Employed	0.003	0.006	0.010	0.008	0.004	0.013
	(0.007)	(0.007)	(0.007)	(0.008)	(0.011)	(0.011)
	0.175	*0.202*	*0.106*	*0.121*	*0.354*	*0.379*
Number of Months of Work	-0.070	-0.018	-0.012	-0.015	0.005	0.079
	(0.068)	(0.056)	(0.057)	(0.070)	(0.104)	(0.092)
	1.435	*1.631*	*0.702*	*0.828*	*3.349*	*3.384*
(Log) Annual real wages	-0.034	0.015	-0.053	-0.006	-0.032	0.012
	(0.038)	(0.027)	(0.055)	(0.041)	(0.040)	(0.026)
	9.214	*9.234*	*8.508*	*8.654*	*9.758*	*9.638*
Number of localities	37	37	37	37	37	37
Number of observations	216,596	206,275	125,930	114,719	90,666	91,556
Number of observations with positive wages	54,874	60,109	17,589	18,051	37,285	42,058

Notes: This table shows DID estimates of the impact of the Preschool Law on mothers who live in the localities of the main sample of the study . The basic unit of observation is at the mother-year level. The specification includes locality and year fixed effects, and controls for education, age, age-squared and religion. Mean outcomes in the pre-treatment years (1995-1999) in the treated localities are presented in italics. Standard errors in parentheses are clustered at the locality level. * p<.0.10, **p<0.05, *** p<0.01

Table A7: Robustness Checks - Alternative Specifications

Dependent Variable	Main results (1)	No controls (2)	Linear trends X SES ranking (3)	Linear trends X SES cluster (4)
High school performance z-score	0.079***	0.097***	0.063***	0.073***
	(0.020)	(0.024)	(0.023)	(0.026)
	-0.058	*-0.058*	*-0.058*	*-0.058*
Took the psychometric exam	0.028***	0.036***	0.017**	0.021**
	(0.008)	(0.009)	(0.008)	(0.009)
	0.389	*0.389*	*0.389*	*0.389*
Postsecondary enrollment by age 19	0.034***	0.037***	0.027***	0.027***
	(0.006)	(0.007)	(0.007)	(0.009)
	0.157	*0.157*	*0.157*	*0.157*
Any juvenile criminal offense (men)	-0.030***	-0.033***	-0.036**	-0.033**
	(0.011)	(0.011)	(0.013)	(0.013)
	0.166	*0.166*	*0.166*	*0.166*
Married by age 21 (women)	-0.016*	-0.020**	0.005	0.003
	(0.009)	(0.010)	(0.011)	(0.011)
	0.318	*0.318*	*0.318*	*0.318*
Number of localities	37	37	37	37
Number of observations	84,457	84,457	84,457	84,457

Notes: This table shows various robustness checks. Column (1) reproduces our main results. Column (2) reports estimates from a simple DID specification, controlling only for locality and cohort fixed effects. Columns (3) and (4) report estimates from our main specification that controls also for an interaction between the socioeconomic ranking/cluster of the locality and a time trend. Mean outcomes of the prereform cohorts (1991-1994) in the treatement localities are presented in italics. Standard errors in parentheses are clustered at the locality level. * p<.0.10, **p<0.05, *** p<0.01.

Table A8: Robustness Checks - Placebo Treatment

Dependent Variable	Main results (1)	Prereform 'placebo' effect (2)
High school performance z-score	0.079***	-0.000
	(0.020)	(0.016)
	-0.058	*-0.091*
Took the psychometric exam	0.028***	0.016
	(0.008)	(0.012)
	0.389	*0.378*
Postsecondary enrollment by age 19	0.034***	0.015*
	(0.006)	(0.008)
	0.157	*0.145*
Any juvenile criminal offense (men)	-0.030***	0.010
	(0.011)	(0.012)
	0.166	0.167
Married by age 21 (women)	-0.016*	-0.009
	(0.009)	(0.013)
	0.318	*0.348*
Number of localities	37	37
Number of observations	84,457	35,707

Notes: This table shows estimates of the placebo effect of universal preschool on various outcomes. The sample includes the prereform cohorts only. The placebo treatment is defined for the year 1998 - 2 years before the actual treatment. The specification includes locality and cohort fixed effects, and controls for parental education, mother's employment and father's earnings (in deciles) when the child was 2 years old, number of siblings and religion. Mean outcomes of the prereform cohorts (1991-1994) in the treatement localities are presented in italics. Standard errors in parentheses are clustered at the locality level. * p<.0.10, **p<0.05, *** p<0.01.

Table A9: Robustness Checks - Alternative Comparison Groups

Dependent Variable	Main Sample (1)	Never Treated (2)	Always Treated (3)	No Nazareth (4)	No Druze (5)	No Bedouin (6)
High school performance z-score	0.079***	0.093***	0.061*	0.076***	0.087***	0.086***
	(0.020)	(0.017)	(0.031)	(0.025)	(0.018)	(0.021)
	-0.058	*-0.043*	*-0.039*	*-0.050*	*-0.040*	*-0.057*
Took the psychometric exam	0.028***	0.020***	0.037***	0.031***	0.023***	0.034***
	(0.008)	(0.006)	(0.011)	(0.008)	(0.007)	(0.007)
	0.389	*0.389*	*0.389*	*0.389*	*0.389*	*0.403*
Postsecondary enrollment by age 19	0.034***	0.035***	0.031***	0.030***	0.031***	0.036***
	(0.006)	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)
	0.157	*0.157*	*0.157*	*0.157*	*0.157*	*0.173*
Any juvenile criminal offense (men)	-0.030***	-0.022**	-0.040***	-0.032**	-0.023**	-0.032***
	(0.011)	(0.010)	(0.013)	(0.012)	(0.010)	(0.012)
	0.166	*0.166*	*0.166*	*0.166*	*0.166*	*0.161*
Married by age 21 (women)	-0.016*	-0.016*	-0.017	-0.017	-0.021**	-0.019*
	(0.009)	(0.008)	(0.014)	(0.011)	(0.009)	(0.010)
	0.318	*0.318*	*0.318*	*0.318*	*0.318*	*0.310*
Number of localities	37	20	32	36	29	30
Number of observations	84,457	61,916	57,274	70,798	72,044	75,158

Notes: This table shows DID estimates of the the estimated effect of universal preschool in different subsamples. The specification includes locality and cohort fixed effects, and controls for parental education, parental employment at age 2, father's labor income at age 2 (indicators of deciles), number of siblings and religion. Mean outcomes of the prereform cohorts (1991-1994) in the treatement localities are presented in italics. Standard errors in parentheses are clustered at the locality level. * p<.0.10, **p<0.05, *** p<0.01.

Table A10: Differential Changes in Class Size

	Elementary school	Middle School + High School	Middle school	High school
	(1)	(2)	(3)	(4)
Class size	0.201	-0.100	-0.075	0.462
	(0.402)	(0.384)	(0.596)	(0.426)
	29.361	*30.066*	*33.436*	*27.832*
Number of localities	37	35	32	34

Notes: This table shows DID estimates using average class size as an outcome. The estimation is based on aggregated data at the locality-cohort level. The specification includes cohort and year fixed effects. Mean outcomes of the prereform cohorts (1991-1994) in the treatement localities are presented in italics. Standard errors in parentheses are clustered at the locality level. * p<.0.10, **p<0.05, *** p<0.01.

Table A11: Effect of the Preschool Law on Preschool Enrollment at the Locality Level

	Age 3 (1)	Age 4 (2)	Age 5 (3)
	A. All Arab Localities		
Preschool Law exposure	0.603***	0.555***	0.009
	(0.050)	(0.051)	(0.033)
Number of localities	52	52	52
	B. Localities of the Study		
Preschool Law exposure	0.597***	0.492***	-0.043
	(0.056)	(0.062)	(0.026)
Number of localities	36	36	36

Notes: This table shows DID estimates of the impact of the Preschool Law on preschool enrollment at different ages. The estimation is based on aggregated data at the locality-year level weighted by population size. The specification includes locality and year fixed effects. Standard errors in parentheses are clustered at the locality level. * p<.0.10, **p<0.05, *** p<0.01.