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Short and Long-Term Effects of Universal Preschool:

Evidence from the Arab Population in Israel[1]

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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 since 2000. 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 between cohorts of children in treated localities before and after the Law introduction relative to equivalent cohorts in comparison localities. We find that individuals benefitted from the provision of universal preschool along various dimensions: their academic performance in elementary, middle school, and high school improved significantly, and their post-secondary enrollment rates increased substantially. We also find beneficial effects of universal preschool on additional outcomes, such as a reduction in juvenile delinquency among boys and a decline in early marriage among women. These findings highlight the benefits of providing universal preschool education for disadvantaged communities.

[1] We thank seminar participants at IDC and the Geneva School of Economics and Management and participants at the Annual Conference of the Israeli Economic Association and the Early Childhood Education Conference at the Taub Center for Policy Research. We thank Avigail Sageev for her research assistance with the PISA data. This study was conducted in the research room of the Central Bureau of Statistics based on de-identified individual record files, from which identification details were omitted, prepared for this purpose by the Central Bureau of Statistics. This research was supported by the Israeli Science Foundation grant No. 1929/19. Schlosser gratefully acknowledges the financial support of the Foerder Institute for Economic Research and the Pinhas Sapir Center for Development at Tel Aviv University.

1. Introduction

Educational interventions at young ages can have large long-term impacts on adult outcomes (Heckman and Masterov, 2007; Almond and Currie, 2011; Cunha and Heckman, 2007; Heckman, Pinto, and Savelyev, 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, and all Nordic nations, offer publicly provided universal 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 human capital accumulation and 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 gradual expansion of universal public preschool for ages 3 and 4 in Israel that started in September 1999. We offer a unique causal analysis of the life-cycle effects of public preschool education, combining information from multiple datasets that cover individual histories for up to 20 years after treatment. We follow individuals throughout elementary, middle school, and high school years examining test scores and success in the matriculation exams, and proceed by examining their performance in psychometric exams and post-secondary education. In addition, we evaluate important social outcomes such as juvenile crime and early marriage.

We focus on one of the more disadvantaged segments in Israeli society – the Arab population residing in localities with low socio-economic 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 (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 more nuanced heterogeneity of the universal public preschool effect within this population by parents' education, fathers'

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

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 Compulsory and Free Preschool Law for Ages 3 and 4 (hereafter "the Law") implemented in Israel since September 1999, which stated that free preschool education should be provided to all Israeli children aged 3 and 4. The implementation of the Law began in localities classified into two lowest socio-economic clusters (1 and 2 out of 10), as defined by the Israeli Central Bureau of Statistics. Most of these localities were Arab, and the implementation of the Law 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 the population of these disadvantaged Arab localities. Due to budget limitations at the national level, the expansion of the Law was halted for several years, until 2015, when it was finally expanded to all localities in Israel.

Using a Difference-in-Differences (DID) research design, we examine changes in students' outcomes in treatment localities comparing exposed and unexposed cohorts, relative to changes in equivalent cohorts from the remaining Arab localities that were not covered in the first stage of the Law 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 the change in outcomes of exposed and unexposed siblings residing in treated localities relative 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 treated towns, 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 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 reflected by an increase in the number of subjects in Math, English, and Science. Concurrently, we find a significant increase in psychometric college-entrance exam participation and psychometric test scores, and a significant increase in post-secondary 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.

We find significant beneficial effects of the preschool going beyond educational attainment. Boys in cohorts exposed to the universal preschool 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 effects of universal preschool is relatively limited given the empirical challenges in isolating causal effects. Since it is unfeasible to randomize children's participation in universal preschool programs, causal effects are usually identified within a quasi-experimental approach. Most studies focus on a specific time horizon – for example, short-term outcomes in preschool (Cascio, 2021, Felfe and Lalive, 2018, Kottelenberg and Lehrer, 2014, 2017) or long-term outcomes such as high-school completion, years of schooling, and employment (Havnes and Mogstad, 2011; 2015). Only a small number of studies examine outcomes over several time-horizons. Notably, there is no consensus in these studies regarding such dynamic impacts. For example, Felfe et al. (2015) find that longer-term effects are stronger than short term effects based on a public preschool reform in Spain in 1990s, while Blanden et al. (2016) find exactly the opposite based on a reform in England in early 2000s. A recent study from the U.S. by Gray-Lobe, et al. (2021) is a notable exception from the above literature as it is the only study using randomization to measure the effects of a large-scale public preschool program in Boston, and it covers a wide range of outcomes throughout school years and until college graduation. The authors find significant positive effects on disciplinary outcomes during school years and on post-secondary educational outcomes, but no effect on test scores or grade-repetition during school.

As opposed to the scarce and inconclusive evidence on the life-cycle impact of universal preschool, the evidence on the life-cycle impact of small-scale targeted programs is ample. However, though such studies often cover a wide range of outcomes over long spans of the life-cycle (e.g., Schweinhart et al., 2005 and Anderson, 2008), they are usually based on very small samples and selected locations, two factors that limit their external validity. Even more crucially, targeted interventions are unlikely to be scalable to the entire population because of their high costs, difficulty in maintaining high quality standards, and administering individualized treatment. The existing evidence from these targeted interventions indicates that such programs have important benefits on cognitive and non-cognitive skills at different stages of the life-cycle (Heckman et al, 2010, 2013). This strengthens the need to investigate the impact of universal preschool over different time horizons and with respect to a variety of outcomes, particularly for disadvantaged communities.

Our paper contributes to the literature of early childhood education by providing a causal analysis of the life-cycle effects of universal preschool at a large-scale, combining information from multiple outcomes spanning individual histories for up to 20 years after treatment. Our results offer important insights on the impacts of universal education among disadvantaged populations. This is important, as targeted programs cannot always reach all children in need. Recent studies have addressed the question whether universal preschool programs constitute an effective policy tool to promote the development and integration of children from minority groups, such as ethnic minorities or immigrants. The existing evidence, though scarce and limited only to short-term effects, indicates that universal preschool programs have a potential to boost minority kids' language and motor skills improving their school readiness (Cornelissen et al., 2018; Felfe and Huber, 2017; Drange and Telle, 2015; and Gormley, 2008). We also contribute to this literature by analyzing a previously unstudied population – Arab children in Israel. Our results based on the Arab population in Israel can also be informative of the potential effects of universal preschool education in non-Western countries, for which the existing evidence is very limited.

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 heterogeneity analysis across several dimensions. 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 percent 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, 2020). Most of the 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 regarding gender relations and roles. The majority of Arab population in Israel are residentially segregated from the Jewish population. Nearly 70 percent live in Arab towns and villages (in which they compromise almost the entire population), 27 percent live in mixed towns (populated by Arabs and Jews), and 3 percent 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 language of instruction is Arabic and the majority of the staff are Arab.

Unlike the Jewish population, who already had a high preschool enrollment rate during the 1990s, only a small share of the Arab children attended public preschools during that period. In the school year of 1998/1999, prior to the implementation of the Preschool Law, enrollment rates in public preschools for Jewish children aged 3 and 4 were 79.7 percent and 89.1 percent, respectively, while the corresponding rates for the Arab population were only 21.3 and 32.2 percent (CBS, 2000). Enrollment of five-year old Arab children was significantly higher compared to that of younger children. For example, the enrollment rate of 5-year-olds in 1998/1999 was 81 percent, even though the rate was still 12 percentage points lower than the equivalent rate among the Jewish population (CBS, 2000). 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 Law to supply such services. The Ministry of Education provided some financial support to towns that supplied preschool services and offered substantial subsidies of 80-90 percent to children of new immigrants

[3] Data from 2020. Calculated from Table 2.3 from the 2021 Statistical Abstract of Israel, published by the Israeli Central Bureau of Statistics (CBS).

[4] Authors' calculations based on Table 1.2 in the Inaugural Annual Statistical Report on Arab Society in Israel, published by the Israel Democracy Institute (2021), and on Table III/5 in the Statistical Yearbook of Jerusalem published by the Jerusalem Institute for Policy Research (2022).

or children who resided in areas defined by the government as targets for development.[5] Given that the criteria for subsidies were not applicable to most Arab children, and that Arab local authorities were continuously facing financial distress, the majority of Arab 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. By contrast, public preschool for children at age 5, was compulsory and was provided to all Arab children, usually as an additional class in elementary schools.

Arab localities also suffered from an acute shortage of physical infrastructure and public buildings. The land available to public institutions in Arab towns was historically scarce due to complicated land property rights system in these towns and the lack of adequate government development plans (Alfasi, 2014). Furthermore, the Ministry of Education neither provided sufficient funding to build new preschool buildings, nor funded rent expenses of preschools that used existing buildings. (Israeli State Comptroller, 1992).

Arab children below the age 5 were mainly at home and did not attend any type of daycare (private or public). Note that the labor force participation of Arab women at that time was extremely low – 17 percent (for ages 25-64) in 1998 compared to 64 percent among Jewish women.[6] According to the PISA students' questionnaires of 2009 (which relates to the 1993 cohort), only 34 percent of Arab children reported that they attended preschool for more than one year compared to 86 percent of Jewish children.

In September 1999, the Israeli government began the gradual implementation of the Compulsory and Free Preschool Law for Ages 3 and 4 (hereafter Preschool Law). 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 to provide it. The implementation of the Law started in the most disadvantaged localities, with the intention to include additional localities each year, and covering the entire country within ten years.[7] The time-frame for the addition of localities was determined according to their classification into socio-economic clusters, which range from 1 (lowest) to 10 (highest).[8]

[5] These are localities classified under the status of "National Priority", "Confrontation Line", and disadvantaged neighborhoods and localities included in the Urban Renewal Project.

[6] Authors' calculations from the 1998 CBS labor force survey.

[7] For a review of the Law implementation, see Blas and Adler (2004) and Kop (2002).

[8] The Israeli Central Bureau of Statistics computes a socio-economic index for each locality, which reflects a combination of some basic characteristics such as financial resources of the residents, housing, education, employment, etc. Localities are then ranked according to this index and allocated into 10 clusters that are as homogeneous as possible according to a measure of distance in their socio-economic index. For more information, see CBS (2003).

Beginning in September 1999, universal free preschool was provided in localities classified into clusters 1 and 2, and in localities and neighborhoods that had received preschool subsidies of 80-90 percent prior to the Preschool Law. Most of the Jewish children covered by the Law would have been eligible for subsidies of 80-90 percent even without the Law. However, the Law did affect the Arab population to a great extent as 91% of the localities included in clusters 1 and 2 were Arab, and 77% of them did not receive preschool subsidies prior to the Law introduction. As a result, the majority of Arab children covered by the Law got access to preschool education for the first time.

The original intention of the government was to gradually extend the Law 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, the Law coverage was officially expanded to include the entire country. Throughout the whole period, there was no enforcement of compulsory education in any of the localities included in the Law mandate.

Figure 1, plots public preschool enrollment of children in Arab localities (not including mixed towns) by age over time stratifying localities into three groups: localities that received subsidies before the implementation of the preschool Law (special status localities), localities that were first included in the Law mandate in September 1999 and did not receive preschool subsidies before (treated), and the remaining Arab localities (non-treated). We only include localities with independent local authorities that have their own socio-economic cluster definition, as specified by the Israeli Central Bureau of Statistics (CBS).[9] To simplify the presentation and discussion, and in line with the MOE notation, we will define the first year of the Law implementation by 2000 (which corresponds to the 1999-2000 academic year).

In the years that preceded the Preschool Law (1998 and 1999) the enrollment rates of Arab children aged 3 and 4 in localities receiving subsidies of 80-90 percent was 86 and 87 percent while enrollment in other Arab localities was significantly lower – 18 and 35 percent respectively. Since 2000, there was a dramatic increase in the enrollment rate of

[9] Small Arab villages are excluded from the plot since they are grouped together into regional authorities and their SES status is less precise. This is because small Arabs villages are usually grouped in the same regional authority together with significantly more advantaged Jewish villages (Kibbutzim and Moshavim). We were not able to obtain information on the exact year of preschool opening in these small villages and data on enrollment rates is missing. We also exclude from the sample 5 localities, whose cluster definition was updated and were added to the Law mandate a few years after the initial Law implementation, 3 Druze localities from the Golan Heights which did not participated in the 1995 census and, as a result, did not have a CBS ranking, and 6 localities whose official status is inconsistent with the actual enrollment data.

Arab localities that were first provided with free preschool, reaching a rate of 83 percent for age 3 and 89 percent for age 4 in 2003. By contrast, the growth in enrollment among those not included in the Law was small, reaching a rate of 29 and 41 percent in 2003 for ages 3 and 4. There is also a slight increase in enrollment rates in localities that had received preschool subsidies before the Law, but the increase does not seem to be different from that experienced by those not included in the Law. The preschool Law did not affect the enrollment of Arab children aged 5 which remained relatively stable over the analyzed period in all three groups of localities.

Figure 2 plots the geographical distribution of Arab localities by treatment status. Treated localities are located in different areas than the rest of Arab localities. The Central district only contains Arab localities that were not included in the Law mandate. The Southern district is comprised exclusively by Bedouin localities that differ along many dimensions from the rest of the Arab population (see, e.g. Abu-Bader and Gottlieb, 2013), all of which belong to the treatment group. The Northern district of Israel is the only region that contains a significant number of localities that were included in the initial stage of the Law mandate and localities that were not.[10] Thus, we focus our study on the localities located in the Northern district of Israel. Our final analysis sample includes 15 treatment localities and 22 comparison localities. Within the latter group, 17 localities had a special status and received preschool subsidies of 80-90 percent before the Law was implemented (always treated), and 5 localities did not receive access to public preschool during the period of interest (never treated).[11]

Figure 3 presents enrollment rates for our analysis sample by age and year stratifying localities by treatment status: never treated, treated, and always treated. The figure highly resembles the trends observed for the full sample of Arab localities. Enrollment rates increased significantly for the treated group: from 18 and 31 percent to 91 and 93 percent 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.

[10] Israel is divided into six administrative districts. The districts have no elected institutions but they possess councils composed of representatives of central government ministries and local authorities for planning and building purposes. Their administration is undertaken by a District Commissioner appointed by the Ministry of Interior. In Israel, the district is a branch of its central government and its role is to enable effective implementation of the government's policy.

[11] We exclude from our analysis six localities that could not be classified to the treatment or the comparison group.

3. Identification Strategy

To examine the impact of universal preschool on children's outcomes we apply a Difference-in-Differences approach (DID): 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 *pre-reform* cohorts were born in 1991-1994, while the *post-reform* cohorts were born in 1995-1999, since the first year of implementation was the 1999/2000 schoolyear. As described above, the treatment group is composed of localities in the Northern district that received access to universal preschool following the implementation of the preschool Law. The comparison group includes localities in the Northern district that did not experience a significant change in public preschool access in the first stage of the implementation of the preschool Law either because they already had access to preschool education or because they only gained access to public preschool education after the expansion of the Law mandate in later years.

To recover the causal effect of public preschool provision, we estimate the following equation:

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

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 equals 1 for an individual who 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 non-parametrically 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 public preschool

[12] We defined employment if monthly labor earnings are at least half of the minimum wage. Results are robust to an alternative definition of non-zero earners. As noted above, labor force participation of Arab women in this period was very low. Thus, instead of controlling for maternal wage deciles we control for mothers' employment.

provision. It is the parameter of interest from a policy perspective when the objective is to capture the effect of universal preschool provision. In section 8, we also present Local Average Treatment Effect (LATE) estimates by scaling the ITT estimates by the increase in public preschool enrollment that followed the reform 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. Specifically, even though the Preschool Law implementation was clearly correlated with outcome *levels* in the pre-period (since treatment localities were more disadvantaged than the comparison group), the identifying assumption requires that the reform is uncorrelated with differential outcome *trends* between treatment and comparison localities. In Section 7, we perform a battery of robustness checks to test the validity of the identification strategy. We verify that our estimates remain similar when excluding individual's background covariates or when adding time trends interacted with key baseline covariates to allow for differential trends in outcomes according to localities SES indicators. We estimate placebo regressions, pretending that the Preschool Law was implemented in the middle of the pre-reform period. We also assess whether the reform was associated with a change in additional educational inputs by estimating DID models where the outcome of interest is the average class size during schoolyears of relevant cohorts and find no evidence of any change in class size associated with the reform. In addition, we estimate separate models where we only use one comparison group: either never treated or always treated and show that our estimates are very similar across these two alternative setups. In this regard, it is important to note that since the group of localities labeled as "always treated" received preschool education since the late 1980s, we expect the effect of preschool to be stable in this sample and therefore not to bias our DID estimates in the form of dynamic treatment effects (see e.g. Roth et al., 2022).[13] Note that preschool enrollment in these "always treated" localities was relatively stable during the period of interest, further supporting the assumption of no dynamic treatment effect for that group during the years analyzed here. We also re-

[13] Historically, preschool subsidies in localities with 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 gradually included in this category. See for example, Government Decision 323 of April 1987, which equalized eligibility between Druze localities and nearby Jewish development localities, providing also preschool subsidies to Druze localities (12th Knesset Proceedings, Booklet 17, January 21, 1991, p. 2064) and Government Decision on equalization between Jewish and Arab localities that live on the border line (11th Knesset Proceedings, Booklet 35, July 6, 1988, p. 3591).

estimate our model dropping one locality at a time to make sure that our estimates are not sensitive to the inclusion of a particular locality given that we have a limited sample of localities. Finally, we apply an additional strategy based on family fixed effects. In this case, we compare differences in outcomes of older (unexposed) and younger (exposed) siblings residing in treated localities relative to differences observed among siblings of the same cohorts from comparison localities. These various robustness tests support the validity of our results.

Since our baseline DID specification in equation (1) summarizes the treatment effects over the entire post-treatment period, we also apply an event-study specification in order to account for the possibility of a treatment effect varying over time (e.g., Bailey and Goodman-Bacon, 2014). The event-study design also allows to address the question of whether the treatment (implementation of the Law) was correlated with some differential pre-trends in outcomes in the treated and the comparison localities. For the event-study specification, we estimate the following model:

$$Y_{ist} = \alpha + \sum_{\tau=-4,\tau\neq-1}^{\tau=4} \beta_\tau \cdot Treated_s \cdot D_{i,2000+\tau} + \gamma X_{ist} + \delta_s + \lambda_t + \varepsilon_{ist} \qquad (2)$$

where for a given τ, the indicator $D_{2000+\tau}$ equals 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 implementation. For $\tau = -4..4$, β_τ denotes the evolution of outcomes in treated localities *net* of equivalent changes in comparison localities.

4. Data and Descriptive Statistics

Data

Our dataset was created by linking administrative records from multiple sources stored at the research room of the Israeli Central Bureau of Statistics. We focus on the cohorts of children born between 1991 and 1999. The starting point is the Israeli population register, which contains information on all Israeli Arabs born in 1991-1999 including individuals' year of birth, gender, locality of residence, and marital status in adulthood.[14] Using

[14] In the best scenario, we would have observed the individuals' locality of residence when he/she was 2 years old, prior to reaching the preschool age. Unfortunately, we observe locality of residence only in specific years (1983, 1995, 1997-2001), and the data is missing sometimes. Therefore, we use an imputation method for the locality of residence in the nearest relevant time. This measurement error is probably negligible as the rate of internal migration of Israeli Arabs is very

personal identifiers, we link these data to Israeli educational registers, which provide information on individuals' enrollment in primary, secondary, and tertiary education.[15]

We proceed by linking the information with students' records from centralized exams administered by the Israeli Ministry of Education (MOE). The first set of exams is the GEMS (Growth and Effectiveness Measures for Schools—"Meizav" in Hebrew), conducted in the 5th and 8th grade in four subjects: Native Language (Arabic), Math, English, and Science. The second set is the Matriculation exams, which are national high-school exit exams taken in various core and elective subjects between 10th and 11th grade.[16] We also obtain information on students' performance on the Psychometric Exam, a standardized test (similar to the SAT in the US) used in combination with the Matriculation certificate as the main admission criterion in higher education institutions.

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

We enrich the students' data by adding family background characteristics adding information on parental education from the education registry and information on number of siblings registered at the population registry. In addition, we use administrative records provided by the Israel Tax Authority to obtain information on earnings and employment of the parents of the individuals in the main sample. Given that at the time of dataset construction, individual earnings data were only available up to the year 2018, we cannot analyze 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 outcomes variables used in this study and their definition.

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 (Hlihel, 2011).

[15] Every citizen in Israel has a unique ID number which is assigned at birth or upon immigration. The CBS assigned to each ID in the different data sets we required, a unique linkable key.

[16] The matriculation certificate is a prerequisite for post-secondary 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 NY Regents Examinations and to the French Baccalaureate exams.

Descriptive statistics

Table 1 presents socio-economic characteristics of the treatment and comparison localities based on data compiled in the 1995 Israeli Census, prior to the Law implementation. In column (3) of the table we report differences between the two groups. 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 percent lower, the dependency ratio, and unemployment rate were higher, and educational attainment was lower. This is unsurprising since the Law was first implemented in the two lowest socio-economic clusters of localities. Notably, treated and comparison localities are similar in terms of average population size.

Table 2 presents family background characteristics of the children in "pre" cohorts (born in 1991-1994) in 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 kids. Also, the ethnic composition is different between the two groups of localities: the share of Druze is higher in comparison localities, while the share of Bedouin is higher in treatment localities. Differences in the levels of the covariates do not necessarily undermine our identification strategy due to the following reasons. First, we control for these covariates in our estimating equation. Second, even if these differences reflect disparities in unobservables that also affect outcomes, they are not expected to bias our estimates as long as these disparities remained constant over time. Third, as part of our robustness tests, we add two specifications where we allow for an interaction between time trends and localities SES ranking or SES cluster, which reflect the main differences between these two groups of localities and constituted the treatment allocation mechanism. Estimates from these two additional models are highly similar to our main results.

In Panel B of Table 2 we examine differences in outcomes of the individuals in the "pre" cohorts (born in 1991-1994) between treatment and comparison localities. Most outcomes point to the relative advantage of the population in the comparison localities during the pre-treatment 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 pre-treated cohorts in treated localities. We find that the implementation of the Preschool Law significantly improved high-school graduation and matriculation outcomes of Israeli Arabs in treated towns. Universal preschool provision increased the likelihood of graduating from high school by 2.8 percentage points (an increase of 3.5 percent relative to pre-reform mean); it increased the participation rate in the Matriculation exams by 3.7 percentage points (5 percent). The likelihood of obtaining a Matriculation diploma rose by 4.3 percentage points (11 percent) and the probability obtain a diploma that meets university entrance requirements increased significantly as well by 11 percent. The improvement in the quality of the Matriculation diploma is also reflected in the increased average number of units awarded in English and Math (0.18 and 0.16 units, respectively – an improvement of 8-9 percent). Furthermore, the number of science subjects attained in the Matriculation diploma increased by 0.9 (13 percent increase). [17]

We find that both boys and girls benefitted from universal preschool and find some differences in the effects by gender for some outcomes. For example, we observe a higher impact on boys' participation rate in the Matriculation exams while for girls, the reform mainly increased the likelihood of obtaining a Matriculation diploma and a diploma that meets university entrance requirements.

Figure 4 presents estimates and 95% confidence bands for the same outcomes in the form of an event-study design (equation (2)) where year zero denotes the first year of the Law implementation. The estimates of the pre-treatment period are small in magnitude and not statistically different from zero and, mostly, they do not show any clear pattern of a differential trend in outcomes in treated versus comparison localities before the implementation of the Law. This is also consistent with the placebo exercise we discuss in Section 7 where we find no differential changes in outcomes between treated and comparison groups when we compare between the early and the late two years of the pre-treatment period. In contrast, the post-period estimates observed in Figure 4 show a substantial change in outcomes relative to the comparison group for the cohorts exposed to universal preschool relative to the pre-Law period.

[17] Science subjects include Physics, Chemistry, Biology, and Computer Science.

Post-Secondary 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 exam and the Psychometric test score. The Psychometric test is a standardized test, similar to the SAT in the US. The test includes three sections: quantitative, verbal, and English and is administered in various languages including Arabic. The positive effect of universal preschool on high school matriculation rates and the quality of the matriculation diploma enhanced access to higher education. We therefore expect the reform to increase the take-up rate of the Psychometric test. Indeed, as reported in the first row of Table 4, we find that participation in the Psychometric test increased significantly, by 2.8 percentage points (a 7 percent increase) when examining whether individuals ever took the Psychometric exam or by 3.3 percentage points (a 9 percent increase), when we focus on the uncensored outcome defined as whether individuals took the Psychometric exam by age 19. 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. To avoid selection bias due to the increase in the probability of taking the test, we define a series of indicators for performance above different quartiles of the test score distribution.[18] The indicators get a value of zero for students who did not take the test.[19] Estimates for the test scores indicators suggest that universal preschool improved analytical and verbal skills. For the total, quantitative, and verbal scores we observe positive effects not only for score threshold indicators at the bottom of the distribution (probably induced by the increase in test takers) but also increases at the middle part of the test score distribution. In contrast, the positive effect on English seems to be mainly generated by the increase in the share of test takers given that we only observe positive estimates at the lowest threshold. Generally, the effect is larger for boys than for girls.

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

[19] The quartiles are defined based on the full distribution of test scores of tests in the Arab language in 2015, which is roughly, the middle of the period (NITE, 2017, pp. 13 and 303). The quartiles are very similar in all years as the absolute test-scores are always scaled to achieve similar distribution across years. 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 among students who took the exam in Hebrew was 576 as opposed to an average of 477 among test takers of the Arab exam.

Enrollment in Post-Secondary Institutions

We next examine the effects of the Preschool Law on enrollment in post-secondary institutions. We cannot fully observe the realization of this outcome for all cohorts as the youngest cohort in this study (1999) is 18-19 years old in the last year of our data (2018). We therefore limit the analysis to the 1991-1998 cohorts and examine post-secondary 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 defined as post-secondary enrollment by age 19. Figure A1 shows that this is the most common age of starting higher education amongst the Israeli Arabs.

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 post-secondary education institution by 5.3 percentage points (an increase of 16 percent relative to the pre-reform mean). This effect is pronounced among almost all levels of post-secondary education: first-tier academic university education, second-tier academic college education, and vocational education. Additionally, we see a decrease in the probability of attending teacher training institutions.[20] Note that the decline in enrollment in teacher training colleges is smaller than the increase observed in other institutions, implying that the increase in post-secondary academic institutions stems both from an increase in post-secondary enrollment and from some switching of individuals from teachers colleges to academic institutions of a higher quality. Our findings are qualitatively similar when examining an uncensored outcome - post-secondary 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 among boys. For example, we see an increase of 24 percent in the probability of post-secondary enrollment by age 19 for boys and an equivalent increase of 21 percent for girls.

[20] Teachers training institutions are the least selective post-secondary academic institutions. In 2017/2018 the average Psychometric score of students in these institutions (488) was significantly lower than the average score of students enrolled in universities (628) and in academic colleges (521). (CBS, 2019a, 2019b).

Additional outcomes

Juvenile Crime

Small-scale targeted 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 of juvenile incarceration and Havnes and Mogstad (2011) who find some evidence for a delay in marriage and parenthood but no reduction in the probability of becoming a single parent. Our comprehensive data allows to shed light on some of these effects.

Arabs are disproportionally 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, 20% of Arabs reported that they do not feel safe from violence in their place of residence, compared to only 8% of the Jews in 2019 (CBS, 2021). Focusing on the population of our study, the share of males with at least one criminal juvenile record (until age 18) was 17% in the pre-reform cohorts in the treated localities.

There are several potential channels linking preschool education with the reduced likelihood of engaging in a criminal activity. First, early education may improve personality skills and reduce externalizing behavior, such as an aggressive or antisocial behavior, which is highly correlated with crime in adulthood, as shown by the Perry Preschool Program analysis (Heckman et al., 2013). Second, when preschool education reduces the probability of dropping out of high-school, as shown in Table 3, it mechanically keeps the young out of the streets during the schooldays (Lochner and Moretti, 2004). Third, education can directly affect individual preferences for crime, by instilling moral values, and increasing the psychic costs of breaking the law (Arrow, 1997). Fourth, schooling might also increase individuals' patience and induce them to avert risky behaviors (Becker and Mulligan, 1997).

Our results in Table 6 show that public preschool provision reduced the likelihood of having a juvenile crime record by 3 percentage points among boys (18 percent decrease from the pre-reform mean). The reduction in crime stems from a decline in life and body offenses and in sex and property offenses.[21] Interestingly, the effect on security and order offense is much smaller and not significant. 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 Maleckova, 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 level among women (less than half percent versus 17 percent among men).

Early Marriage

Although Israeli Arabs went through a rapid modernization process in the last half a century, the Arab population in Israel remains more traditional than most Western societies. In 2017, the average age of first marriage was 23 years for Israeli Arab women in contrast to an average of 26 years for Israeli Jewish women and 30 years for women in the OECD countries.[22] Figure A2 presents the cumulative share of married men and women by ages 17-27 for the 1991 cohort (pre-treatment cohort) for whom we can observe the longest time horizon. As the figure shows, a notable portion of women, about one-third, marry at early ages (18-21). In contrast, only 2 percent of men married by age 21. We examine the effect of preschool on marriage by age 21, since we can observe this outcome for several post-treatment cohorts without censoring and given the role of early marriage for women's educational investment and fertility decisions.

Preschool education could potentially delay the age of first marriage through the reduction of high school drop-out probability and the increased enrollment in higher education, documented above. In traditional societies, where childbirth usually takes place soon after marriage, educational attainment can affect the age of marriage through various mechanisms. First, it is costly to have children during school (Black et al, 2008, "Incarceration Effect"). Second, 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 vs non-market work, as in traditional societies (Becker, 1981; Blau et al. 2000). In addition, increased education might affect the age of marriage by

[21] Security and order offense records include offences against the security of the state or against public order. Life/body offence records include offences against person's life and body harm. Sex/property offense records include sexual offences and property offences. Other offense records include fraud, morality offences (usually files that deal with drugs), economic offences, licensing offences, and administrative offences. Our data does not include a more detailed breakdown of the offenses due to confidentiality reasons.

[22] 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 of OECD (2019).

reducing religiosity and eroding traditional values (Cesur and Mocan, 2018; Hungerman, 2014).

The effects of universal preschool on the probability to marry at an early age are presented in Figure 5, where we plot DID estimates and 95 percent confidence intervals from models where the dependent variable is marrying by age 18, 19, 20, and 21. Panel A report estimates for women. 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, the point estimate implies a decline of 5 percent relative to a baseline of 32 percent. Estimates for males reported in Panel B are very noisy with confidence bands that do not reject the hypothesis of a zero effect.[23]

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 across different groups is their counterfactual childcare. This is particularly important in the case of universal preschool provision as it might crowd out high quality targeted programs (e.g., Bassok et al., 2014). Alternatively, universal preschool might provide an educational framework to children who would have otherwise been at home or would have attended low quality childcare settings. Evidence regarding at home care versus formal childcare points to beneficial effects for children from lower SES families (Cascio and Schazenbach, 2013; Drange and Havnes, 2019; Felfe et al., 2015) and usually mixed or even negative impact for children from high SES families (Havnes and Mogstad, 2015; Herbst, 2013).

Motivated by the literature cited above, we examine the heterogeneous effects of universal preschool along various dimensions and report, in Table 7, DID estimates and outcome means for different subsamples.[24] To save space, we select a representative sample of outcomes reported in the main analysis that refer to each of the domains analyzed above. Results for other outcomes are highly consistent with the results discussed below. Given the extremely low incidence of juvenile crime among girls and early marriage among boys, we report estimates for the relevant genders for these two

[23] Estimates for marriage by age 18 are not defined since there are no married males by this age in the sample.

[24] We do not have estimates of preschool attendance at the individual level. Therefore, differences in the estimated effects between groups might derive from differences in compliance rates. Estimates should be interpreted as differences in the intention to treat effects.

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) provide a similar picture irrespective of whether we stratify the sample by mother's or by father's education. Overall, the effects are larger, both in absolute terms and relative to the outcome means, for children whose parents did not complete 12 years of schooling. Nevertheless, we observe that universal preschool had beneficial effects also for children of higher educated parents who benefited mostly by improving the quality of their Matriculation diploma, achieving more units in English and Math, and attaining more Science subjects.

We also examine heterogeneous effects along two additional dimensions: father's income and mother's employment, 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).[25] Interestingly, estimates reported in columns (5) and (6) of the table are largely similar for children of low versus high income fathers. This is remarkable in light of the different results we obtained when stratifying the sample by father's education and the fact that outcome means for the pre-treatment period do differ for high versus low income fathers. By contrast, we find important differences in treatment effects when we stratify the sample by mother's employment (columns (7) and (8)). Children of non-working mothers experienced a larger improvement in outcomes, both in absolute terms and relative to the outcome means, compared to children whose mothers worked when they were 2 years old.

Differences in the estimated effect between children of working and nonworking mothers cannot be explained simply by the lower baseline outcomes of the latter, as this also applies to children of high- and low-income fathers (where we find no significant differences in treatment effects). One possible explanation is that children whose mothers did not work when they were age 2 would have probably stayed at home if universal preschool had not been available. Another possible explanation is that preschool provision induced some mothers to work, providing additional sources of income to the household, so that the observed benefits of universal preschool are partly due to a positive income effect. In work in progress, we are investigating these possible mechanisms together with

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

an overall assessment of the impact of preschool provision on mothers' employment and household income.

The stratification presented in Table 7 suggests that different children were affected at different margins. To further explore this, we examine heterogeneity in treatment effects with respect to children's predicted outcomes. We predict outcomes for each individual based using a prediction model that uses student-level covariates for the pre-treatment 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 public preschool provision 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 graduation and on participation in the Matriculation exams are strongest, both in absolute terms and relative to the outcome means, among individuals with low predicted outcomes. This is probably due to the fact that the baseline outcomes for the groups with medium and high predicted outcomes are already relatively high (at least 85 percent). The effect on Matriculation eligibility rates and on the number of Math and English units is the largest in absolute terms for the medium achievement group but the improvement in terms of percentages relative to the outcome means is similar for the low and the medium achievement group. Interestingly, the impact of universal preschool was more modest among individuals located at the highest tertile of predicted outcomes, except for a substantial increase in post-secondary enrollment. Our results are similar when we stratify the sample using a single predicted outcome, the likelihood of obtaining a Matriculation diploma, and estimate our DID model for all outcomes based on this stratification. Again, we see that the most advantaged students benefited from preschool education by improving the quality of the Matriculation certificate and increasing their chances of attaining post-secondary education. The more disadvantaged students benefited at all margins (see Table A3).

The heterogeneity analysis presented above provides some interesting insights regarding the effects of universal preschool education. The most disadvantaged students benefited the most from the reform. At the same time, more advantaged students also gained from preschool education by improving on more selective outcomes. Overall, preschool education provided positive impacts for different individuals at different margins. These results stress the importance of studying multiple outcomes across different population groups to properly assess the effects of universal preschool provision.

Intermediate outcomes in Elementary and Middle School

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 primary and middle schools. The GEMS exams are standardized tests administered by National Authority for Measurement and Assessment of Education (RAMA) in Israel to students in 5th and 8th grade in 4 subjects: Language, Math, English, and Science.

The administration of the GEMS exams follows a special structure, where only a national representative sample of schools is tested each year.[26] Such setting imposes some challenges to our estimation methodology. First, it reduces our sample size 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) will be affected by the sample composition of the localities included into GEMS exams in each cohort.[27] 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 along with 90 percent confidence intervals are presented in Figure 6. We find that the most pronounced effect of universal preschool was on individuals' language skills (Arabic). Test scores in Arabic increased significantly by 0.12 standard deviations in 5th grade. Notably, the effect persisted also in 8th grade, where the test scores in Arabic improved by 0.18 standard deviations. We also find an effect on math test scores of 0.20 standard deviations in 5th grade but we find no equivalent effect in 8th grade. Thus, it seems that either the beneficial effects on Math achievements fade out

[26] All localities are grouped into four clusters, where each cluster 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 locality are tested in the same subject only once in four years. A further complication is that the compliance to this design was not perfect in the localities of our study, as we see in our data that some of the localities did not follow the official test-taking scheme, but rather followed a more idiosyncratic pattern of years in which its students took the test.

[27] Theoretically, the clusters of schools examined in each year are random, and each of them are a representative sample of the entire population of schools. Thus, the bias should vanish for a large sample of localities that fully comply to the original structure of the implementation of the tests. However, our analysis sample includes a limited number of localities (37), and not all localities comply to the official structure of the test-taking.

over time (as in Deming, 2009 and other studies that examined the short versus long term effects of preschool education) or that the Math skills that are tested in the 5th grade are not highly correlated with the Math skills tested in the 8th grade.[28] Our results are consistent with Felfe et al. (2015) who examined the effects of a similar preschool reform in Spain during the 1990s on 10th grade achievement scores, and found a 0.15 increase in reading scores, and no effect on Math achievements. The large improvement in verbal skills may explain the sharp increase in enrollment in higher education documented in Section 5, which is in line with the results by Aucejo and James (2021), who found that verbal skills are a primary factor for explaining variation in university enrollment between individuals, having a marginal effect that is more than twice as large than the effect of math skills.

We find no significant effect of public preschool provision on children's performance in English and Science in 5th or in 8th grade. At first blush, it seems to contradict some of our previous findings, which show a significant increase in the number of English units and Science subjects achieved 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 from 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 at the Matriculation exams. This explanation is also consistent with the fact that Matriculation exams are high-stakes tests, which affect access to higher education and some jobs, whereas GEMS tests are low-stakes for the students as they aim to assess general trends in the Israeli public education system, and they do not enter student's evaluations in school.

7. 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 treated and comparison localities. To save space, we select a subset of outcomes from each domain (high school graduation, achievement in the Psychometric exams, post-secondary education, crime, early marriage, and fertility) and report here the robustness tests on the selected set of outcomes.

[28] Fifth graders are tested mainly in arithmetic, while 8th graders are mainly tested in algebra and equations.

We begin by assessing the sensitivity of our results to the inclusion of the set of background characteristics used in our main specification. Results are reported in Table A4. 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 treated 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 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 locality's socioeconomic cluster (1 to 4) or socioeconomic ranking (1 to 203) (and the baseline linear trend).[29, 30] Estimates remain largely similar to our main results. Some of the estimates are smaller, but most remain highly significant. Note that the interaction between a time trend and socioeconomic raking or cluster is highly correlated with the interaction between post and treatment status, so it is not surprising that some of the estimated effects are smaller.

We also conduct a placebo analysis where we estimate baseline DID equation (2) on all main outcomes, including only the pre-treatment cohorts and pretend that the Preschool Law was implemented in the middle of the pre-period, two years before it actually came into effect. Estimates, shown in Table A5, are small, and insignificant and have inconsistent signs across outcomes. Thus, we find no evidence for significant differential pre-trends between treatment and comparison localities supporting our main identification assumption of no differential trends in the post-reform period.

An additional concern is that perhaps other changes might have taken place during the same period that could have affected performance of children in treated or comparison localities. In particular, we might worry about other differential investments in educational inputs across treated 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 sizes for the study cohorts at relevant

[29] The national ranking of the localities of the study lay within the range of 8 to 138. A lower ranking implies lower socioeconomic status.

[30] Note that we cannot 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, 2011).

elementary, middle, and high school ages and estimate a simple DID specification that includes locality and cohort fixed effects using the average class size as an outcome. Estimates for the post-reform cohorts in treated localities, reported in Table A6, are inconsistent across schooling stages and none of them are statistically or economically significant.

A last check we perform relates to experimental setup. Note that our comparison group is composed by two different groups of localities: those that did not receive universal preschool education during the period of interest (never treated) and those that already had universal preschool well before the implementation of the preschool Law due to their special status (always treated). If universal preschool had some dynamic effects over time that still persisted during the period of study among the always treated localities, our estimates might be biased. To assess this, we re-estimate our main model twice using only as a comparison group one type of localities: never treated or always treated. Results are reported in Table A7 where we also report in column (1) our main estimates to ease comparison across samples. Overall, most of our main results persist when using only never treated localities (column (2)) or always treated localities (column (3)) as comparison groups and are highly similar to our main results.

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 sample of localities (37), we wanted to make sure that our results do not derive from a particular group of localities. We first re-estimated 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 re-estimated our model omitting all Druze localities given that they are all included in the comparison group (column (5)). Finally, we re-estimated our model omitting all Bedouin localities given that the vast majority are included in the treatment group (column (6)). Despite changes in the composition sample of localities, 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 suggest that our results are not driven by ethnic-specific trends within the Arab community in Israel. Moreover, they provide some evidence for the external validity of our results.

As a final check to assess the sensitivity of our results, we re-estimated our model dropping one locality each time to make sure that our main results do not derive from a specific locality. In Figure A3 we plot estimates along 95% confidence intervals for high school outcomes from these alternative subsamples along with our main results. All figures are reassuring showing that our main results do not derive from any particular locality.

Family Fixed effects

Our comprehensive data allow us to identify siblings and estimate models with family fixed effects. In this case, we compare between outcomes of children who were young enough to have access to universal preschool and their older siblings who were already above age 4 when the reform was implemented in their locality of residence, relative to siblings born in the same years in localities from the comparison group. The high fertility rate among Arab families provides us with the opportunity to identify several affected and unaffected siblings within the same household.[31]

The comparison of the family fixed effects results to the results based on DID 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. On the contrary, a smaller impact obtained from the family fixed effects specification relative to our main results might suggest that families compensate human capital investments. Alternatively, it might point to unobserved trends or shocks at the locality level that could have biased our DID estimates upward.

In Table 9 we report the results from the family fixed effects specification on a representative set of outcomes. To ease comparison across the results, we replicate our main results from the DID specification in column (1). In column (2) we report our main results from the DID specification after we restrict the sample to families who have at least two children (82 percent of the main sample), since the family fixed effects model is based on this sample. Estimates are almost identical to our main results although they are slightly less precise due to the reduction in the sample size. In column (3) we report estimates from the family fixed effects specification. Estimates are remarkably similar to the DID specification although slightly noisier due to the addition of family fixed effects. The similarity between estimates from our main DID specification and the family fixed effects strategy provide further evidence for the validity of our main identifying assumption, suggesting that our results are not confounded by unobserved trends or shocks at the

[31] Arab families are quite large (compared to western families). The average number of children per household in our sample was higher than 3 (see Table 2).

locality level that led to an improvement in outcomes of children living in treated localities who were exposed to the preschool reform. They also suggest that our results are not driven by differential changes in the composition of families in treated and comparison localities.

8. Comparison with other preschool programs and with alternative school interventions implemented in Israel

To put our results in perspective, we compare them to estimates of the effects of universal or large-scale preschool programs obtained in the literature along with estimates from small-scale targeted programs. So far, we reported *intention to treat* (ITT) estimates for the effects of preschool education. They are interesting for policy purposes as they inform on the effect of universal preschool provision. They also provide information on the overall effect of universal preschool education on all children, including those who did not attend public preschool but live in treated localities and could have been indirectly affected. To compare our results with other studies, we report here treatment effect on the treated by scaling up our DID *intention to treat (ITT)* estimates by the increase in public preschool enrollment generated by the reform (about 60 percentage points).[32]

Table 10 reports a comparison between our results and estimates from other studies. 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 treatment effect on the treated of about 5 percentage points (a 6 percent increase relative to the baseline outcome mean). This effect is within the range of other studies that examined 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 relatively 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 outcome baseline mean when comparing across studies.

At the other end, we observe a much larger effect on college attendance in our study relative to other studies - 6.7 percentage points or 26 percent increase. This again, might

[32] Appendix Table A8 reports DID estimates for the effects of the Preschool Law on public preschool enrollment based on aggregate data at the locality level weighted by population size.

derive from the fact that baseline college enrollment was relatively low in our population of interest relative to 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 increased human capital for both genders.

We also compare our results with estimates from studies that examined 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.[33] Lavy and Schlosser (2005) examined 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 diploma among treated students. The effect in absolute terms is larger than the impact of universal preschool (13 ppt. versus 7 ppt.) and the improvement relative to the outcome means are 24 percent for remedial education and 17 percent for universal preschool. Nevertheless, the effect of preschool education is substantially larger in the long term: Lavy et al. (2022) find a 8 percentage point increase (13% relative to the outcome mean), in enrollment to low-tier higher education institutions (colleges), with no effect on enrollment in high-tier institutions (universities). In our study, we find that preschool education increased enrollment in higher education institutions by 9 percentage points (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 costs of the intervention was relatively low, only $385 per student, as it only provided the monetary award to students who achieved the target. The authors find a significant increase of 13 percentage points in the probability

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

of obtaining a Matriculation certificate among 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 only find a localized effect on post-secondary enrollment for girls located at the top quartile of the achievement distribution.

Overall, our comparison with these two high school interventions implemented in Israel suggest that universal preschool education is costlier than targeted interventions towards high school students but the longer term benefits 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. In future work, we plan to assess this, when the cohorts exposed to universal preschool enter the labor market.

9. **Summary and Conclusions**

This study presents a rich set of findings on the effects of public preschool provision in a disadvantaged population, the Arab population in Israel. Our results show that access to public preschool at ages 3 and 4 benefited individuals over multiple horizons. It improved children's language skills during elementary and middle school and raised performancein 5th grade math exams. In high-school, public preschool provision decreased the likelihood of dropping out of school, raised participation in the Matriculation exams, improved the eligibility to a Matriculation diploma, and the quality of the diploma achieved, as reflected in accumulated Math and English units, and the number of Science subjects. The probability to enroll in post-secondary education also increased significantly, for both academic and vocational institutions. We also find beneficial effects of public preschool on additional long-term outcomes: a decline in boys' probability to engage in juvenile crime and in women's likelihood to marry at an early age.

We find that the universal preschool affected different children at different margins. It had a larger impact among children from low- or medium- socioeconomic backgrounds for most outcomes, whereas children from higher socioeconomic background benefitted by improving the quality of their Matriculation diploma and increasing the likelihood of attaining post-secondary education. The long-term impact of universal preschool on post-secondary 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 public preschool, even in the absence of well-targeted educational programs. Free universal preschool 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 public preschool on individuals' outcomes and achievements, there are almost no studies that examined 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 public preschool to disadvantaged communities.

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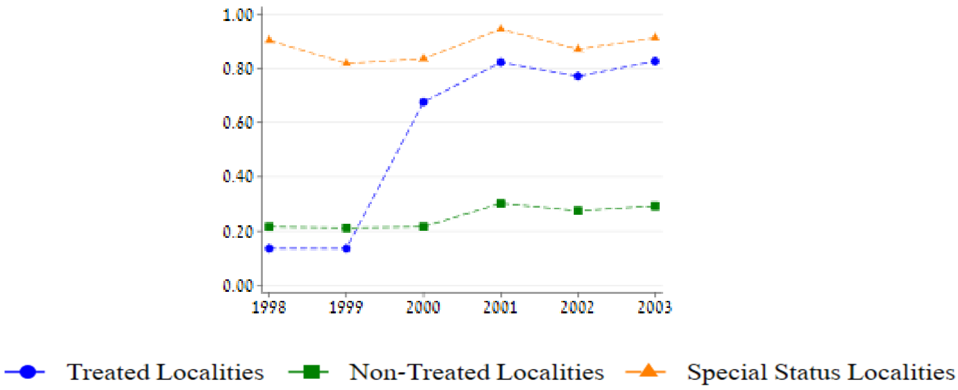
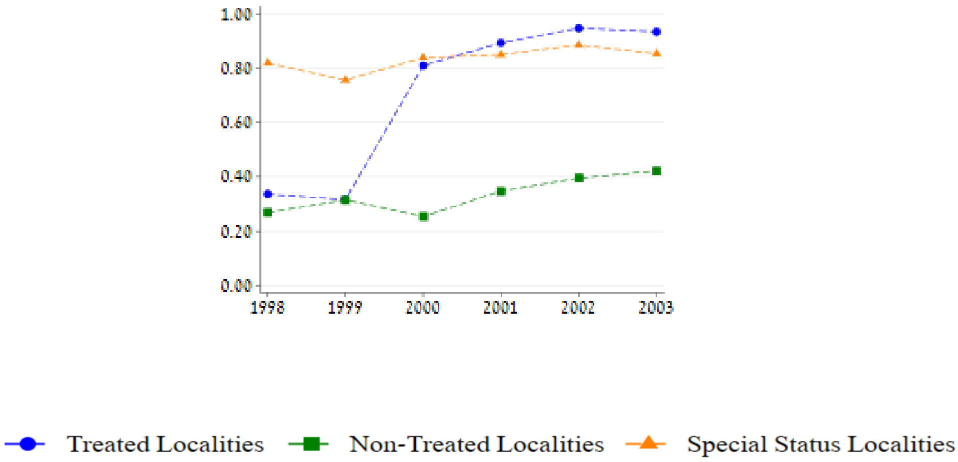
Figure 1: Preschool Enrollment in Arab localities in Israel - 1998-2003

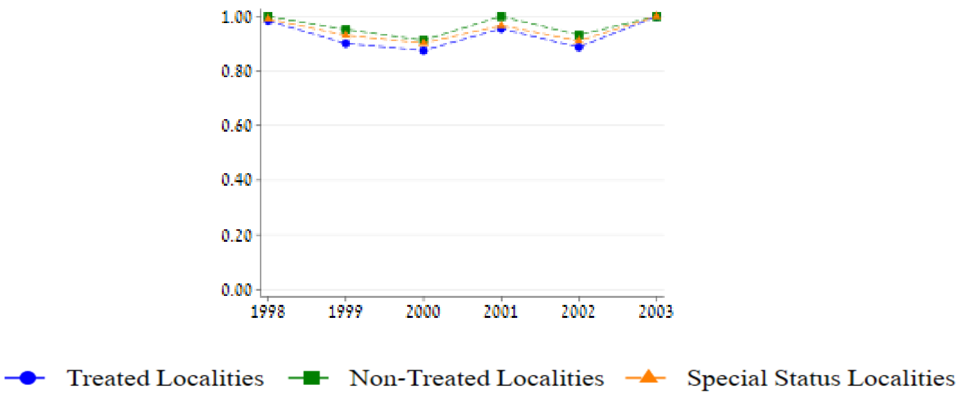
a. Enrollment at Age 3:



b. Enrollment at Age 4:



c. Enrollment at Age 5:



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 since year 2000. Non-treated localities are those that were not included in the first phase of the Law implementation. Special Status localities include localities that received preschool subsidies before the Law implementation.

Figure 2: Geographical Distribution of the Localities of the Study

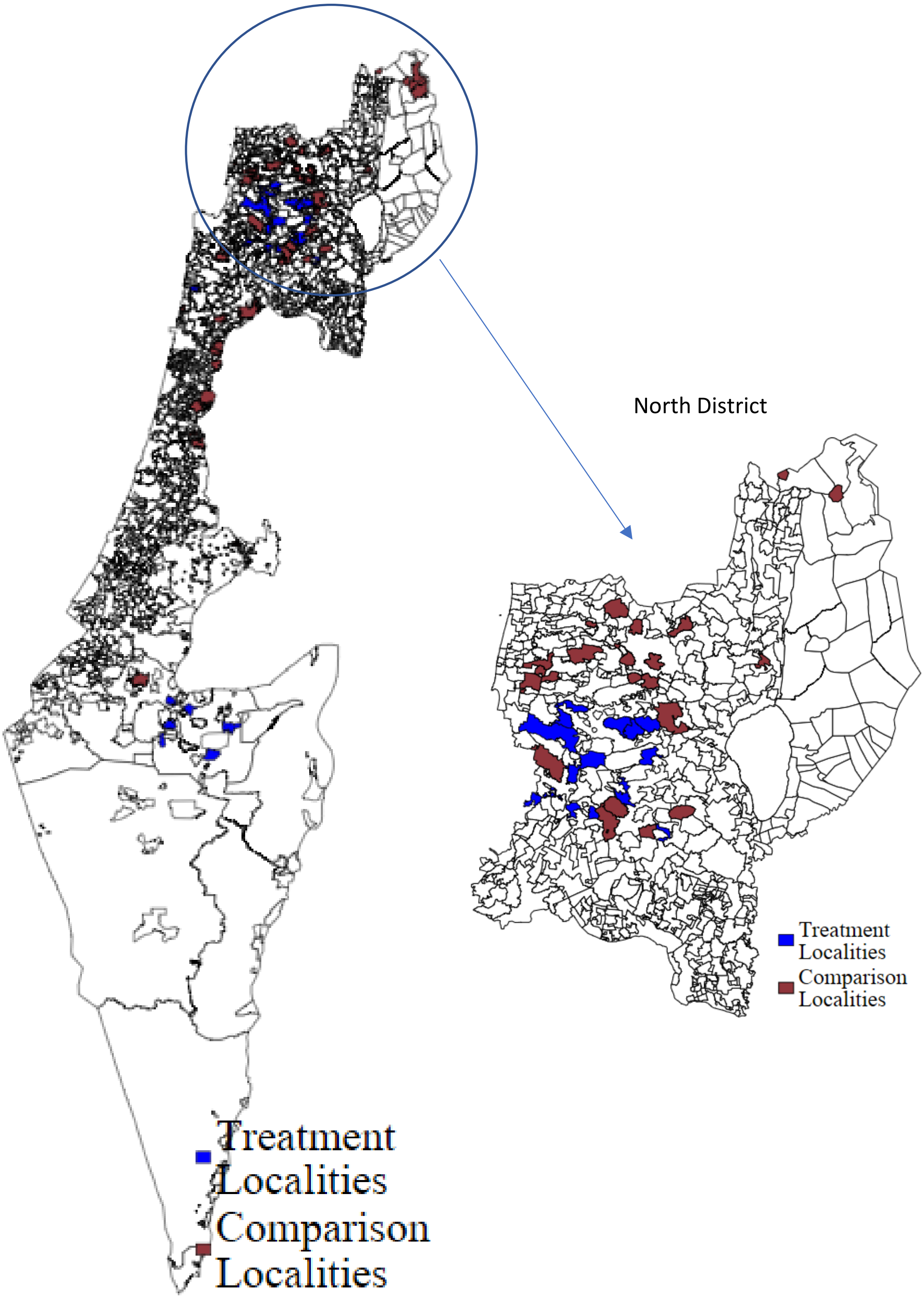
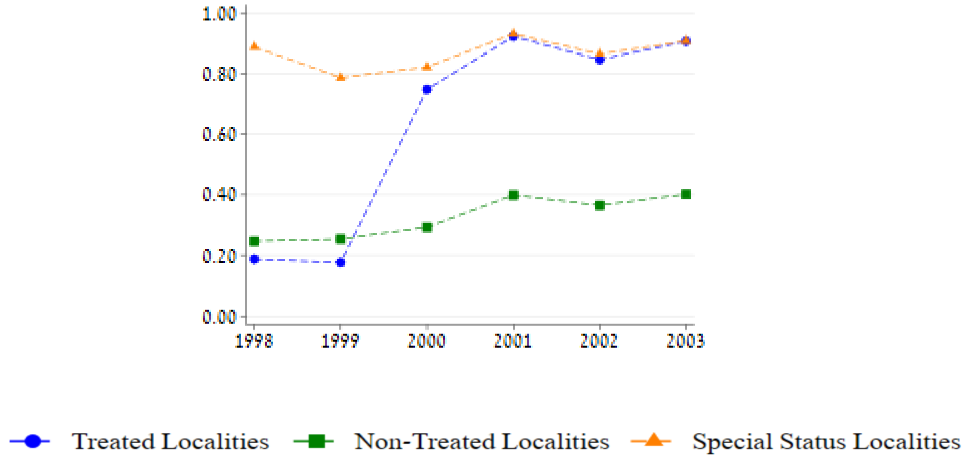
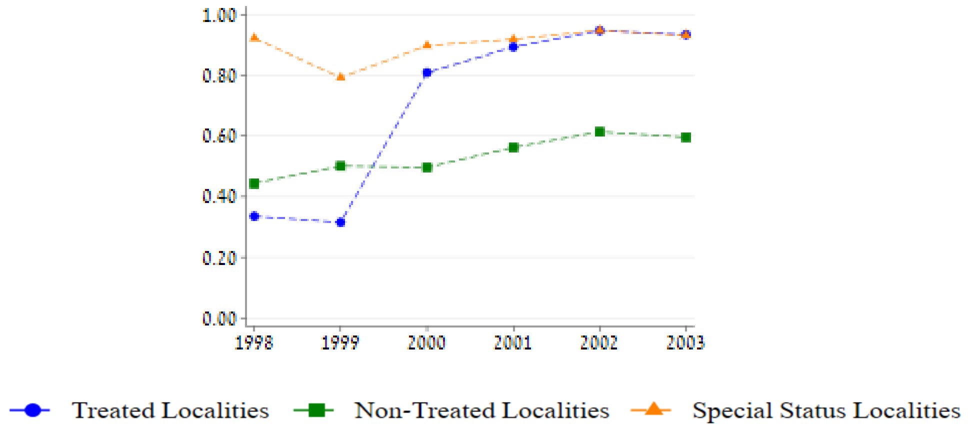


Figure 3: Preschool Enrollment in the Localities of the Study (North district only) - 1998-2003

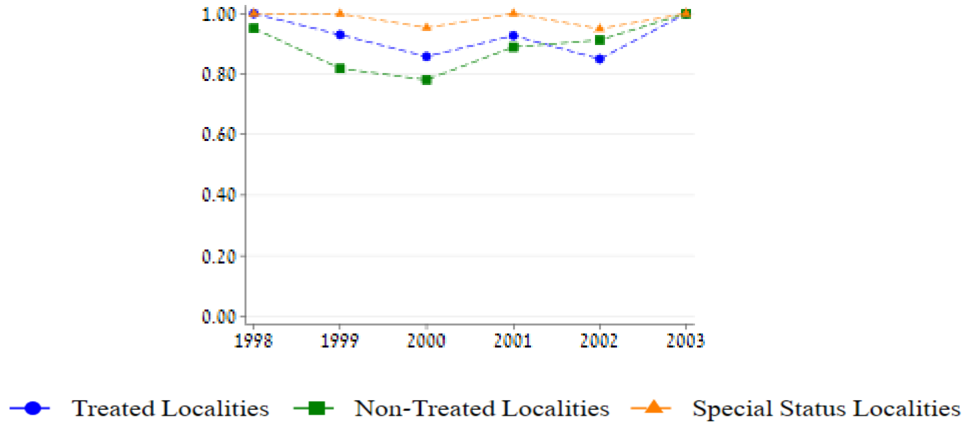
a. Enrollment at Age 3:



b. Enrollment at Age 4:

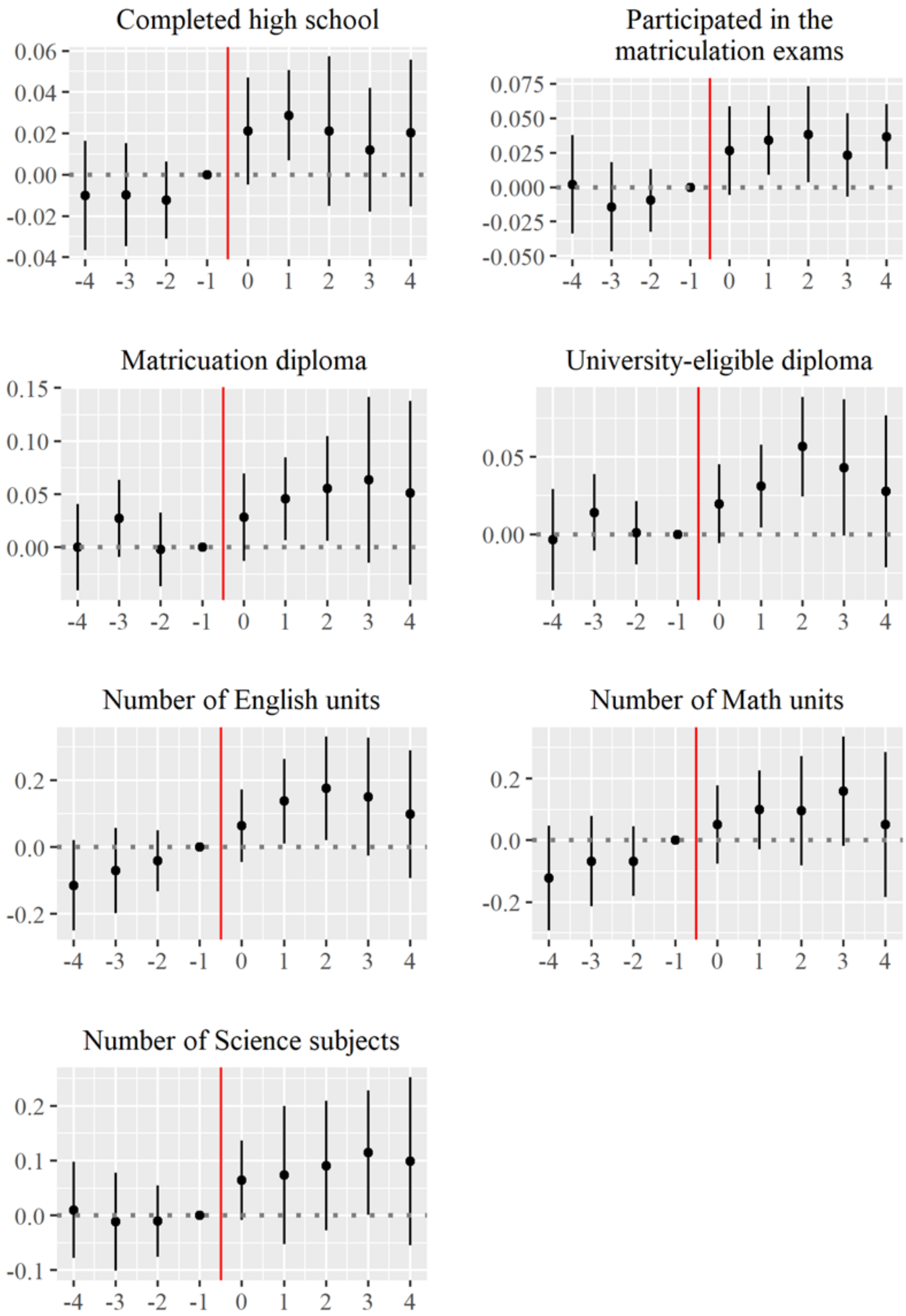


c. Enrollment at Age 5:



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 data and population counts by locality and year provided by the Israeli Central Bureau of Statistics. Treated localities received universal preschool education since year 2000. Non-treated localities are those that were not included in the first phase of the Law implementation. Special Status localities include localities that received preschool subsidies before the Law implementation.

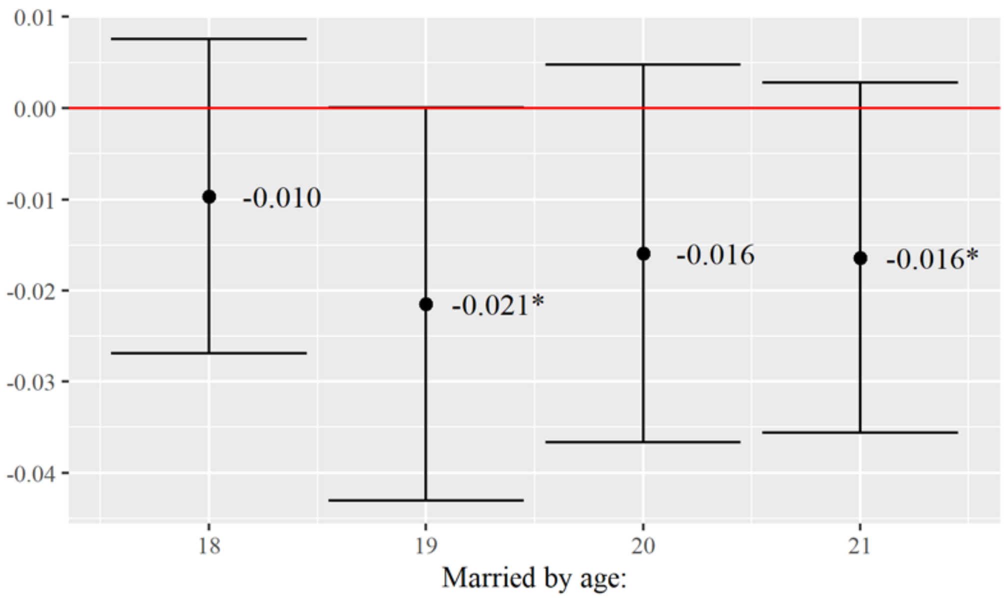
Figure 4: Event-Study Estimates of the Effects of the Implementation of the Preschool Law



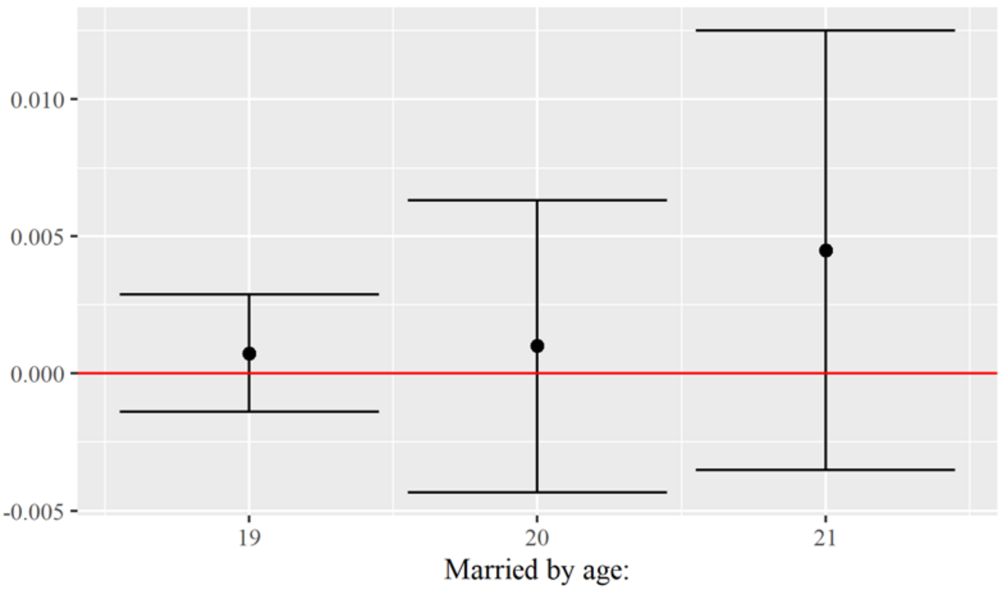
Notes: The figures plot the pre-and post- treatment effects along 95 percent confidence bars on various educational outcomes, based on an event-study specification (Equation 2). The x-axis denotes years before or after the Law implementation. Year zero denotes the first year of the Law implementation. 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.

Figure 5: Effects of the Preschool Law on individuals' Probability to Marry at Young Ages

A. Women



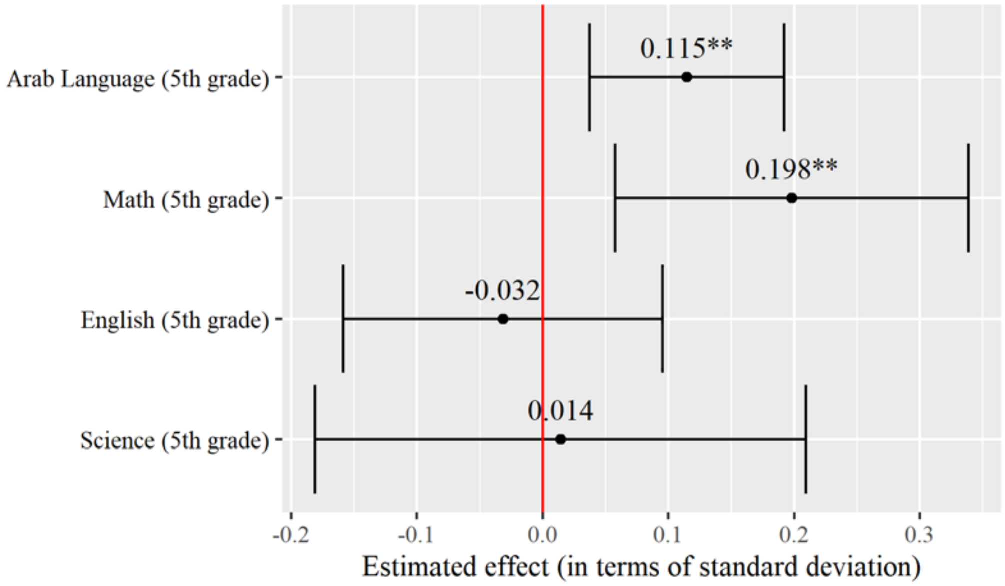
B. Men



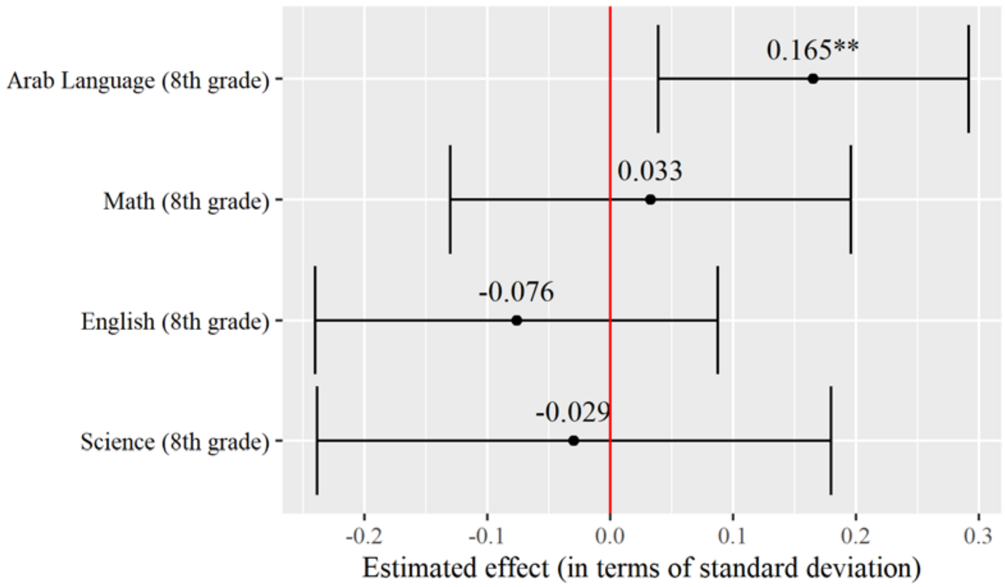
Notes: The figure reports DID estimates along with 95 percent confidence intervals of the effects of universal preschool on the probability to marry by age 18, 19, 20, and 21, based on the specification in Equation (1). The specification includes locality and cohort fixed effects and controls for parental 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 6: Estimates of the Effects of the Preschool Law on GEMS Test Scores in 5^{th} and 8^{th} grade

a. 5^{th} Grade



b. 8^{th} Grade



Notes: The figure DID estimates along with 95 percent confidence intervals of the effects of universal preschool on GEMS test scores in 5^{th} and 8^{th} 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. p*<.0.10, **p<0.05, *** p<0.01

Table 1: Descriptive Statistics - Treated and Comparison Localities

	Treated (1)	Comparison (2)	Difference (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 and more kids (%)	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 bagrut among aged 17-18 (%)	0.28	0.42	-0.14***
	(0.09)	(0.16)	(0.04)
Work-seekers among aged 15 and above (%)	0.05	0.03	0.02***
	(0.02)	(0.01)	(0.01)
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)
Receive unemployment insurance (%)	0.02	0.02	0.00**
	(0.00)	(0.01)	(0.00)
Receive income support (%)	0.03	0.02	0.01***
	(0.01)	(0.01)	(0.00)
Receive income supplements to old age pension (%)	0.46	0.27	0.19***
	(0.09)	(0.07)	(0.03)
Number of Localities	15	22	

Notes: This table presents balance tests between the treatment and the comparison localities according to characteristics from the 1995 Census. Columns 1 and 2 display the means (and standard deviation in parentheses) in each category. Difference of means between treatment and comparison localities is reported in Column 3 , with robust standard errors in parentheses. *p<.0.10, **p<0.05, *** p<0.01

Table 2: Descriptive Statistics Pre-Treatment Cohorts

	Treated	Comparison	Difference		Treated	Comparison	Difference
	(1)	(2)	(3)		(1)	(2)	(3)
Panel A: pre-treatment covariates				**Panel B: outcomes**			
Father's years of education	9.92	10.65	-0.73***	Completed high-school	0.80	0.83	-0.03
	(3.19)	(3.20)	(0.24)		(0.40)	(0.37)	(0.03)
Mother's years of Education	9.42	10.13	-0.71*	Participated in the matriculation exams	0.76	0.79	-0.03
	(3.09)	(3.04)	(0.38)		(0.43)	(0.40)	(0.03)
Father employed in 1998	0.67	0.66	0.01	Any matriculation diploma	0.40	0.46	-0.06
	(0.47)	(0.47)	(0.02)		(0.49)	(0.50)	(0.04)
Mother employed in 1998	0.13	0.18	-0.05***	University-eligible matriculation diploma	0.30	0.37	-0.07***
	(0.33)	(0.38)	(0.02)		(0.46)	(0.48)	(0.02)
Father's monthly wages in 1998	4,942	5,941	-999***	Number of English units	2.13	2.46	-0.32**
	(3,926)	(4,780)	(177)		(1.91)	(1.95)	(0.13)
Mother's Monthly Wages in 1998	2,743	2,973	-230	Number of Math units	1.75	1.94	-0.19
	(1,979)	(2,368)	(164)		(1.80)	(1.83)	(0.12)
Number of siblings	3.65	3.06	0.59***	Number of science subjects	0.51	0.52	-0.01
	(2.11)	(1.80)	(0.14)		(0.74)	(0.70)	(0.07)
Share female	0.49	0.48	0.00	Any juvenile criminal record (Boys)	0.17	0.13	0.03*
	(0.50)	(0.50)	(0.00)		(0.37)	(0.34)	(0.02)
Share Druze	0.00	0.25	-0.25***	Participated in the psychometric exam	0.39	0.41	-0.02
	(0.01)	(0.43)	(0.09)		(0.49)	(0.49)	(0.03)
Share bedouin	0.21	0.03	0.18*	Average psychometric score	471.67	483.67	-11.99
	(0.40)	(0.17)	(0.10)		(111.65)	(113.02)	(8.29)
				Any post-secondary enrollment	0.33	0.39	-0.06**
					(0.47)	(0.49)	(0.03)
Number of Localities	15	22					
Number of observations	14,454	21,253		Married by age 19 (women)	0.15	0.10	0.05*
					(0.35)	(0.29)	(0.03)

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

Table 3: High School Results

Dependent Variable	Full Sample (1)	Boys (2)	Girls (3)
Completed 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*
Matricuation diploma	0.043*	0.022	0.066**
	(0.023)	(0.022)	(0.030)
	0.396	*0.278*	*0.522*
University-eligible diploma	0.033**	0.020	0.048**
	(0.013)	(0.013)	(0.018)
	0.300	*0.198*	*0.407*
Number of English units	0.181***	0.136**	0.233***
	(0.052)	(0.066)	(0.065)
	2.133	*1.580*	*2.718*
Number of Math units	0.156**	0.121*	0.196**
	(0.060)	(0.066)	(0.078)
	1.752	*1.323*	*2.206*
Number of Science subjects	0.092**	0.098**	0.089*
	(0.041)	(0.038)	(0.046)
	0.688	*0.484*	*0.904*
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 exposure to the Preschool Law 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 pre-treatment cohorts (1991-1994) in the treated 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 4: Psychometric Test Results

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 (≥80)	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 (≥95)	0.019***	0.024***	0.014*
	(0.006)	(0.006)	(0.009)		(0.005)	(0.006)	(0.007)
	0.177	*0.126*	*0.230*		*0.181*	*0.137*	*0.227*
above third quartile (≥580)	0.009	0.015***	0.002	above third quartile (≥115)	0.012**	0.018***	0.006
	(0.005)	(0.005)	(0.008)		(0.005)	(0.005)	(0.008)
	0.069	*0.051*	*0.088*		*0.102*	*0.083*	*0.122*
Verbal Score				English Score			
above first quartile (≥80)	0.016**	0.030***	0.002	above first quartile (≥80)	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 (≥95)	0.015**	0.023***	0.008	above second quartile (≥95)	0.021**	0.024***	0.018
	(0.006)	(0.006)	(0.010)		(0.008)	(0.008)	(0.011)
	0.175	*0.115*	*0.239*		*0.137*	*0.096*	*0.180*
above third quartile (≥115)	0.010	0.014**	0.006	above third quartile (≥115)	0.005	0.009	0.001
	(0.006)	(0.005)	(0.009)		(0.007)	(0.006)	(0.011)
	0.114	*0.077*	*0.154*		*0.077*	*0.055*	*0.101*
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 exposure to the Preschool Law 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 pre-treatment cohorts (1991-1994) in the treated 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: Post-Secondary Education

	Ever Enrolled			Enrolled by Age 19		
	Full Sample	Boys	Girls	Full Sample	Boys	Girls
Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)
Post-secondary student	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*
Academic college (second-tier)	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*
Vocational post-secondary	0.020***	0.030***	0.010**	0.007**	0.009**	0.004
institution	(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 Observations	37	37	37	37	37	37
Number of Localities	74,452	38,198	36,254	74,452	38,198	36,254

Notes: This table shows DID estimates of the impact of exposure to the Preschool Law on post-secondary 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 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 6: Juvenile Crime

Dependent Variable	Full Sample (1)	Boys (2)	Girls (3)
Any juvenile offense record	-0.015**	-0.030***	-0.000
	(0.006)	(0.011)	(0.001)
	0.087	*0.166*	*0.004*
Security/order offense record	-0.004	-0.008	-0.000
	(0.004)	(0.007)	(0.001)
	0.046	*0.088*	*0.002*
Life/body offense record	-0.011***	-0.022***	0.001
	(0.003)	(0.006)	(0.001)
	0.047	*0.089*	*0.002*
Sex/property offense record	-0.008*	-0.017**	-0.000
	(0.004)	(0.008)	(0.001)
	0.040	*0.077*	*0.001*
Other offense record	-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 exposure to the Preschool Law 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 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 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)
Completed high school	0.032**	0.016*	0.029*	0.022**	0.027*	0.026**	0.030**	0.017*
	(0.015)	(0.008)	(0.015)	(0.010)	(0.014)	(0.011)	(0.013)	(0.010)
	0.757	*0.924*	*0.762*	*0.899*	*0.771*	*0.847*	*0.790*	*0.868*
Participated in the matriculation exams	0.046***	0.017*	0.043***	0.024***	0.035***	0.037***	0.040***	0.025**
	(0.014)	(0.009)	(0.014)	(0.008)	(0.012)	(0.012)	(0.012)	(0.011)
	0.710	*0.908*	*0.716*	*0.878*	*0.727*	*0.816*	*0.748*	*0.843*
Matriculation diploma	0.051*	0.020	0.048*	0.035	0.044*	0.040*	0.051**	0.013
	(0.026)	(0.021)	(0.025)	(0.022)	(0.025)	(0.022)	(0.025)	(0.021)
	0.313	*0.623*	*0.318*	*0.585*	*0.349*	*0.466*	*0.368*	*0.550*
University-eligible diploma	0.043**	0.016	0.039***	0.028*	0.030*	0.034**	0.040**	0.013
	(0.016)	(0.015)	(0.014)	(0.016)	(0.015)	(0.014)	(0.015)	(0.015)
	0.212	*0.537*	*0.215*	*0.502*	*0.254*	*0.368*	*0.270*	*0.466*
Number of English units	0.204***	0.116*	0.214***	0.126**	0.163***	0.202***	0.204***	0.091
	(0.062)	(0.060)	(0.062)	(0.058)	(0.054)	(0.058)	(0.059)	(0.059)
	1.752	*3.169*	*1.777*	*2.988*	*1.945*	*2.412*	*2.006*	*2.831*
Number of Math units	0.163**	0.116*	0.175***	0.115	0.147**	0.155**	0.188***	0.039
	(0.069)	(0.059)	(0.063)	(0.070)	(0.065)	(0.059)	(0.062)	(0.078)
	1.410	*2.679*	*1.421*	*2.547*	*1.559*	*2.037*	*1.631*	*2.416*
Number of Science subjects	0.089**	0.083	0.082*	0.106**	0.069*	0.112**	0.096**	0.063
	(0.038)	(0.055)	(0.041)	(0.046)	(0.041)	(0.045)	(0.040)	(0.054)
	0.537	*1.099*	*0.556*	*1.005*	*0.632*	*0.771*	*0.638*	*0.961*
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*
Post-secondary 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 record (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 exposure to the Preschool Law 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 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 8: Heterogenous Effects of Universal Preschool by Predicted Outcomes

	Level of Predicted Outcome		
	Low	Medium	High
Dependent Variable	(1)	(2)	(3)
Completed high school	0.034	0.026**	0.010*
	(0.026)	(0.012)	(0.005)
	0.626	*0.882*	*0.971*
Participated in the matriculation exams	0.058**	0.028**	0.009
	(0.022)	(0.012)	(0.005)
	0.561	*0.857*	*0.962*
Matricuation diploma	0.038	0.074**	0.017
	(0.026)	(0.034)	(0.021)
	0.202	*0.436*	*0.727*
University-eligible diploma	0.040**	0.056***	0.011
	(0.015)	(0.019)	(0.017)
	0.120	*0.315*	*0.650*
Number of English units	0.175**	0.247***	0.081
	(0.076)	(0.074)	(0.058)
	1.225	*2.392*	*3.619*
Number of Math units	0.132*	0.218**	0.073
	(0.070)	(0.086)	(0.064)
	1.010	*1.913*	*3.072*
Number of Science subjects	0.084**	0.098*	0.071
	(0.032)	(0.054)	(0.053)
	0.325	*0.708*	*1.247*
Took the Psychometric Exam	0.025***	0.030**	0.015
	(0.009)	(0.013)	(0.014)
	0.173	*0.420*	*0.726*
Post-secondary 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 record (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 exposure to the Preschool Law, 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 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 9: Effects of Universal Preschool - Sibling's Fixed Effects Model

Dependent Variable	LocalityFE Main Sample (1)	LocalityFE Siblings Sample (2)	SiblingsFE Siblings Sample (3)
Completed high school	0.028**	0.024**	0.023
	(0.012)	(0.012)	(0.014)
	0.802	*0.808*	*0.808*
Participated in the matriculation exams	0.037***	0.032***	0.031**
	(0.011)	(0.011)	(0.014)
	0.763	*0.771*	*0.771*
Matricuation diploma	0.043*	0.044*	0.039
	(0.023)	(0.023)	(0.034)
	0.396	*0.404*	*0.404*
University-eligible diploma	0.033**	0.038***	0.038**
	(0.013)	(0.013)	(0.018)
	0.300	*0.303*	*0.303*
Number of English units	0.181***	0.182***	0.159*
	(0.052)	(0.052)	(0.082)
	2.133	*2.161*	*2.161*
Number of Math units	0.156**	0.154**	0.157*
	(0.060)	(0.059)	(0.086)
	1.752	*1.780*	*1.780*
Number of Science subjects	0.092**	0.086**	0.079
	(0.041)	(0.039)	(0.050)
	0.688	*0.698*	*0.698*
Took the Psychometric Exam	0.028***	0.031***	0.040***
	(0.008)	(0.007)	(0.013)
	0.389	*0.395*	*0.395*
Post-secondary 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 record (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 effect of exposure to the Preschool Law. 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 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,

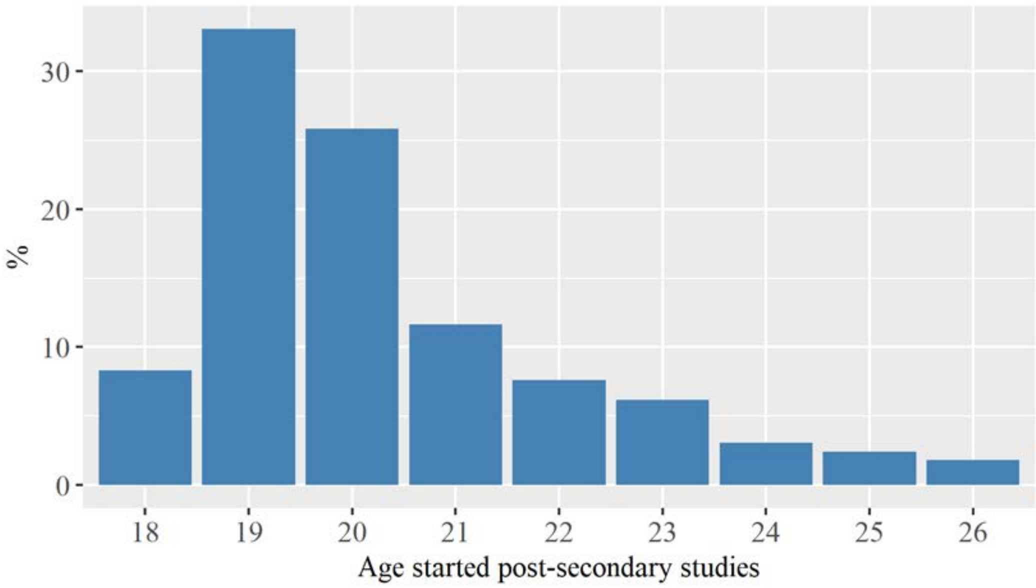
Table 10: Comparison to Similar Studies - Local Average Treatment Effects

Study	Country and type of preschool	Age at intervention	High-School Graduation		Higher Education (academic)	
			Effect	Baseline mean	Effect	Baseline mean
		A. Large Scale Programs				
Gray-Lobe et al , 2021	Universal, US (Boston)	4	0.060	0.64	0.054	0.65
Havnes and Mogstad, 2011	Universal, Norway	3-6	0.058	0.74	0.069	0.37
Deming, 2009	Head Start, US	3-5	0.086	Unknown	0.057	Unknown
Bailey et al, 2020	Head Start, US	3-5	0.024	0.92	0.054	0.64
This study	**Universal, Israeli Arabs**	**3-4**	**0.047**	**0.80**	**0.067**	**0.26**
		B. Targeted programs				
Belfield et al. (2006)	Perry Preschool, US	3-5	0.165	0.61 (at age 40)		
Campbell et al. (2012)	Abecedarian, US	0-6	0.068	0.82	0.17	0.06
Heckman et al., 2010	Perry Preschool, US	3-5	0.61 (girls) -0.03 (boys)	0.23 (girls) 0.51 (boys)		
Anderson, 2008 (high-school) Elango et al., 2016 (higher educ)	Abecederian, US	0-6	0.23 (girls) -0.10 (boys)	0.61 (girls) 0.74 (boys)	0.193	Unknown

Table 11: Comparison to Other Educational Interventions Implemented in Israel at Older Ages

Study	Intervention	Target population	Age	Cost per student (2000)	Matriculation diploma: Effect	Matriculation diploma: Baseline mean	Post-secondary enrollment: Effect	Post-secondary enrollment: Baseline mean
Lavy and Schlosser (2005) Lavy (2021)	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)	0.63
Angrist and Lavy (2009)	Monetary awards to students	Students in 39 low achieving high schools (10 Arab schools)	15-18	$385	0.13 girls (school take up: 75%) no effect for boys	0.24 all 0.29 girls 0.2 boys	No effect overall. No effect on university attendance. localized increase in post secondary for girls in the top quartile: 0.123	0.43 (girls in top quartile)
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**

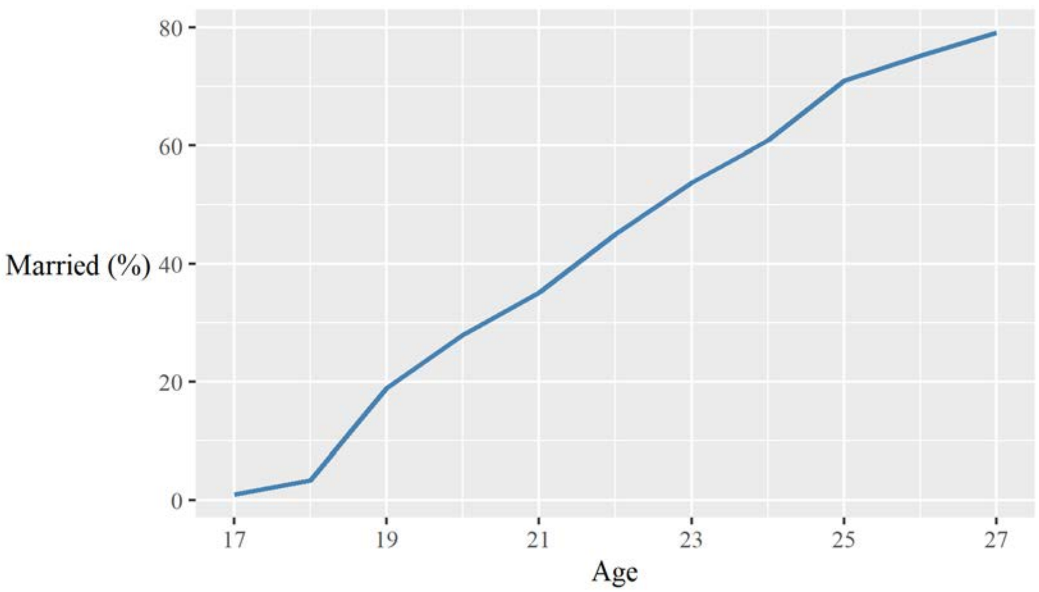
Figure A1: Distribution of Age of First Enrollment in Post-secondary Studies
(1991 Cohort)



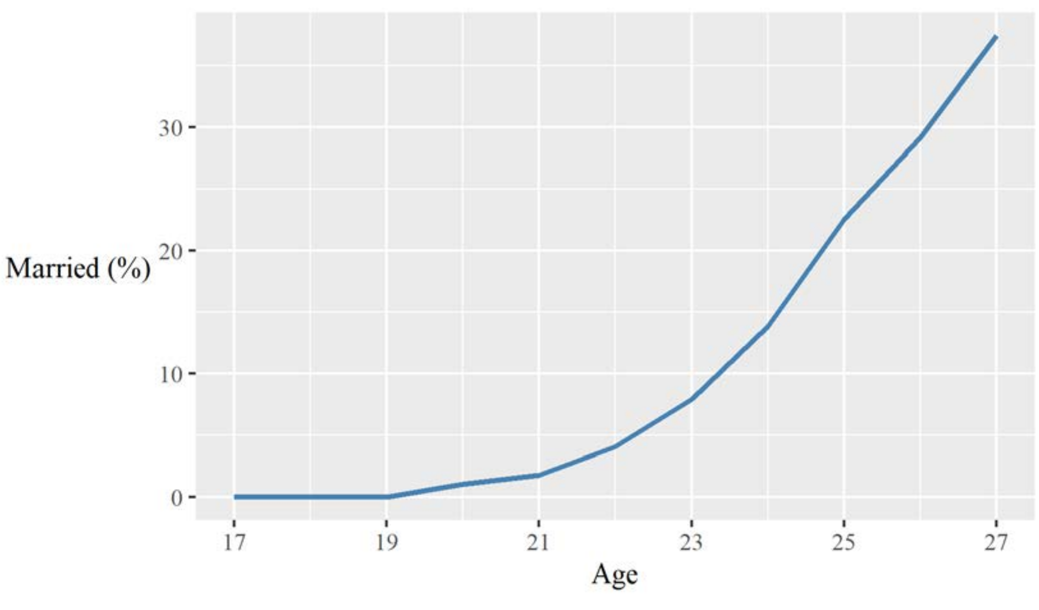
Notes: This figure reports the age distribution at first enrollment in a post-secondary education institution for the 1991 birth cohort included in our sample. Enrollment data is available until the 2017-2018 academic year.

Figure A2: Share of Married Arab Individuals, by Age

A. Women

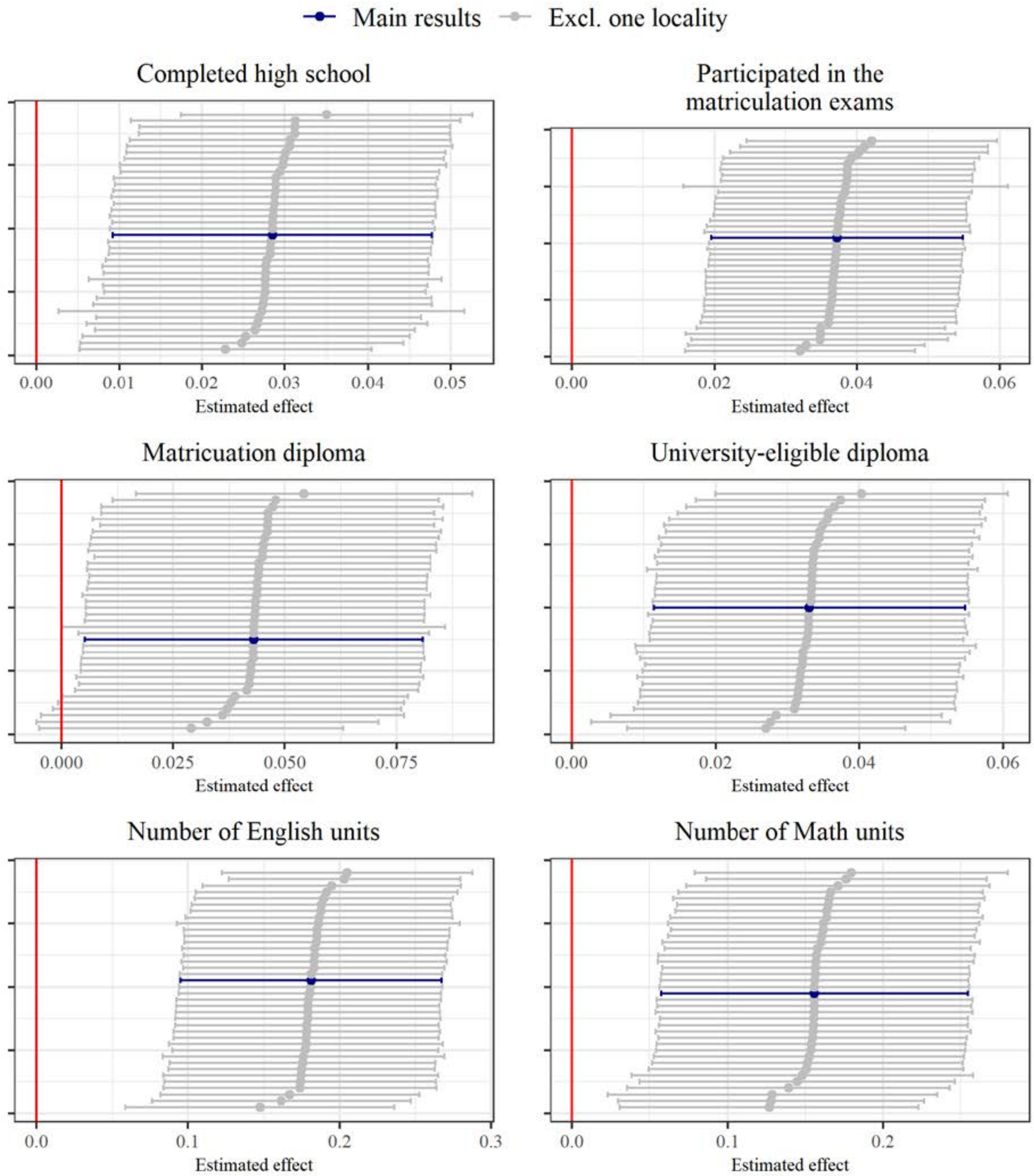


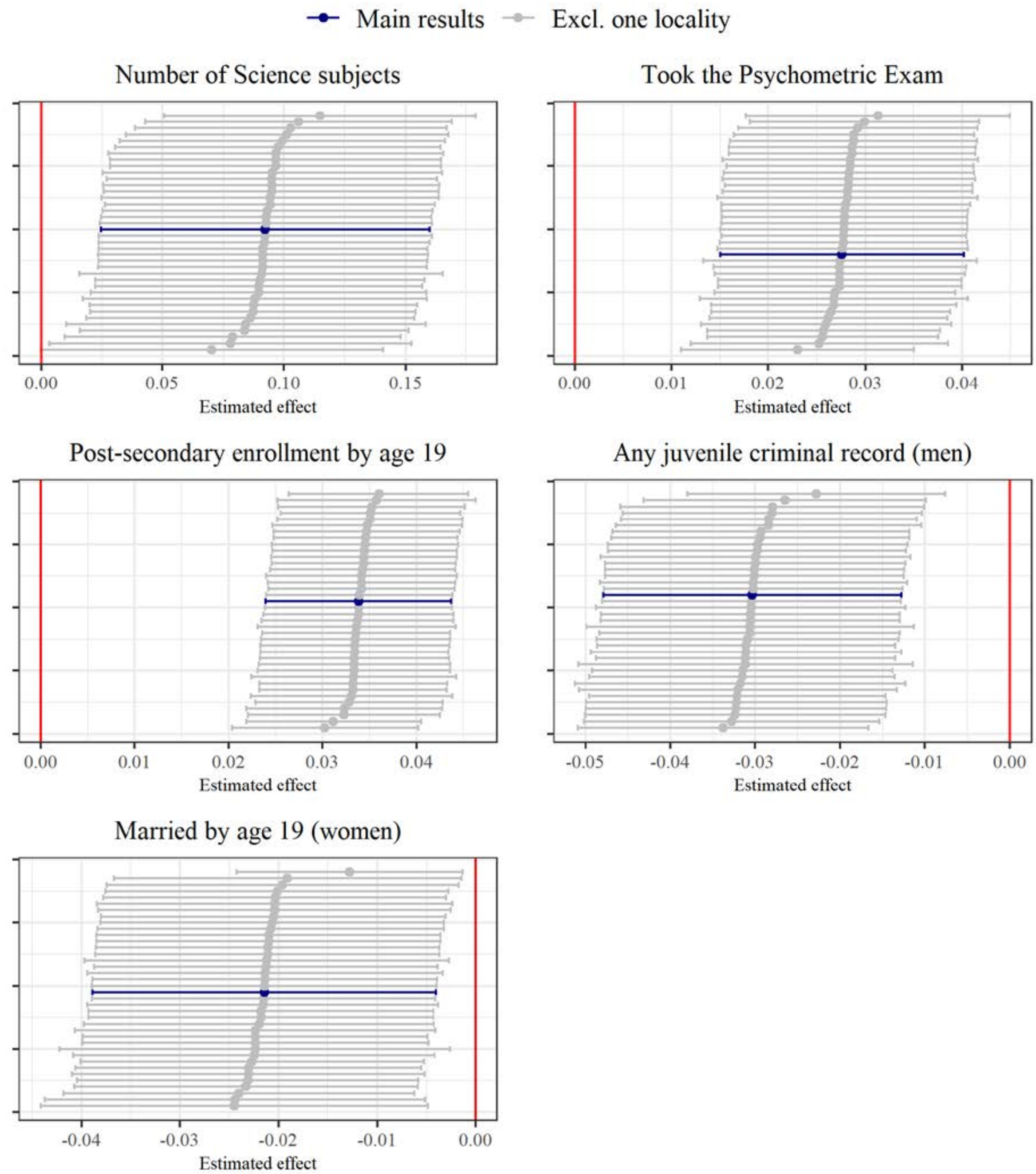
B. Men



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

Figure A3: Effects of Universal Preschool on Various Outcomes, Excluding One Locality at a Time





Notes: The figures plot the distribution of estimates and 95% confidence intervals of our baseline DID specification in equation (1). The blue bars represent the estimate for our main sample, and the grey bars represent estimates obtained by excluding one locality from the sample each 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 in 1991-1999. Standard errors are clustered at the locality level.

Table A1: Pre- and Post-Reform Cohorts of the Study in Different Ages

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, Post-Secondary 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 pre- and post- treatment 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 of the Study

Variable name	Variable description
High School	
Completed High-School	=1 if individual was enrolled in 12th grade; 0 otherwise
Participated in the Matriculation Exams	=1 if individual took at least one matriculation exam; 0 otherwise
Matriculation Diploma	=1 if individual obtained a Matriculation diploma; 0 otherwise
University-eligible diploma	=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
Number of English units	Number of Matriculation units the individual was awarded in the English subject (1-5)
Number of Math units	Number of Matriculation units the individual was awarded in the Math subject (1-5)
Number of Science Subjects	Number of subjects in scientific fields, 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 has taken the psychometric exam at least once; 0 otherwise (any time/ by age 19)
Psychometric Total Score	Aggregate score in the Psychometric Exam (200-800)
Psychometric Verbal Score	Aggregate score in the Verbal (Arabic) Section of the Psychometric Exam (0-150)
Psychometric Quantitative Score	Aggregate score in the Quantitative Section of the Psychometric Exam (0-150)
Post-Secondary Outcomes	
Post-secondary student	=1 if individual was enrolled in any Israeli post-secondary institution; 0 otherwise
Academic Institution	=1 if individual was enrolled in any post-secondary institution with academic degree credentials (University, academic college or teacher training institution) ; 0 otherwise
University	=1 if individual was enrolled in an Israeli University, which are the first-tier academic institutions in Israel; 0 otherwise
Academic College	=1 if individual was enrolled in an Israeli Academic College, which are the second-tier academic institutions in Israel; 0 otherwise
Teacher Training Institution	=1 if individual was enrolled in a teacher training institution; 0 otherwise
Vocational post-secondary institution	=1 if individual was enrolled in a vocational post-secondary institution; 0 otherwise
Juvenile Crime	
Any Juvenile Offense Record	=1 if individual had at least one criminal record by age 18; 0 otherwise
Security/order offense record	=1 if individual had at least one criminal record in security or order offenses by age 18; 0 otherwise
Life/body offense record	=1 if individual had at least one criminal record in life or body offenses by age 18; 0 otherwise
Sex/property offense record	=1 if individual had at least one criminal record in sex or property offenses by age 18; 0 otherwise
Other offense record	=1 if individual had at least one criminal record in other offenses 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 ages 18, 19, 20 and 21
GEMS exam ("Meitzav")	
Arab language grade	Grade in the Arab Language GEMS exam (in terms of std. dev, original scale is 0-100)
Math grade	Grade in the Math GEMS exam (in terms of std. dev, original scale is 0-100)
English grade	Grade in the English GEMS exam (in terms of std. dev, original scale is 0-100)
Science grade	Grade in the Science exam (in terms of std. dev, original scale is 0-100)

Table A3: Heterogeneous Effects of Universal Preschool by Predicted Matriculation Rate

Dependent Variable	Level of Predicted Matriculation Rate		
	Low	Medium	High
	(1)	(2)	(3)
Completed high school	0.035	0.025**	0.006
	(0.024)	(0.012)	(0.006)
	0.647	*0.888*	*0.974*
Participated in the matriculation exams	0.057***	0.032**	0.006
	(0.020)	(0.012)	(0.006)
	0.583	*0.861*	*0.965*
Matriculation diploma	0.038	0.074**	0.017
	(0.026)	(0.034)	(0.021)
	0.202	*0.436*	*0.727*
University-eligible diploma	0.036**	0.058***	0.014
	(0.015)	(0.019)	(0.017)
	0.119	*0.311*	*0.650*
Number of English units	0.167**	0.290***	0.061
	(0.078)	(0.071)	(0.062)
	1.221	*2.354*	*3.614*
Number of Math units	0.131*	0.251***	0.051
	(0.073)	(0.080)	(0.068)
	1.005	*1.862*	*3.081*
Number of Science subjects	0.059	0.112**	0.086
	(0.035)	(0.053)	(0.052)
	0.357	*0.734*	*1.280*
Took the Psychometric Exam	0.020**	0.040***	0.013
	(0.010)	(0.012)	(0.016)
	0.183	*0.430*	*0.742*
Post-secondary 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 record (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 effects of exposure to the Preschool Law, by tertiles of predicted matriculation eligibility defined by the pre-treatment 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 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 A4: Robustness Checks - Alternative Specifications

Dependent Variable	Main Results (1)	No controls (2)	Linear trends X ranking (3)	Linear trends X SES cluster (4)
Completed high school	0.028**	0.034**	0.009	0.013
	(0.012)	(0.013)	(0.013)	(0.016)
	0.802	*0.802*	*0.802*	*0.802*
Participated in the matriculation exams	0.037***	0.044***	0.021	0.025
	(0.011)	(0.011)	(0.014)	(0.016)
	0.763	*0.763*	*0.763*	*0.763*
Matriculation diploma	0.043*	0.052**	0.037	0.048**
	(0.023)	(0.025)	(0.022)	(0.022)
	0.396	*0.396*	*0.396*	*0.396*
University-eligible diploma	0.033**	0.042***	0.035**	0.037**
	(0.013)	(0.015)	(0.015)	(0.014)
	0.300	*0.300*	*0.300*	*0.300*
Number of English units	0.181***	0.226***	0.158**	0.156**
	(0.052)	(0.065)	(0.065)	(0.063)
	2.133	*2.133*	*2.133*	*2.133*
Number of Math units	0.156**	0.194***	0.116*	0.140**
	(0.060)	(0.071)	(0.059)	(0.059)
	1.752	*1.752*	*1.752*	*1.752*
Number of Science subjects	0.092**	0.105**	0.087*	0.114***
	(0.041)	(0.041)	(0.044)	(0.040)
	0.688	*0.688*	*0.688*	*0.688*
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*
Post-secondary 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 record (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) reports estimates from our main specification controlling also for an interaction between ranking/cluster index of the locality and a time trend. Mean outcomes of the pre-treatment cohorts in the treated localities are reported in italics. Standard errors in parentheses are clustered at the locality level. * p<.0.10, **p<0.05, *** p<0.01

A5: Robustness Checks - Placebo Treatment

Dependent Variable	Main Results (1)	Early Period 'placebo' effect (2)
Completed high school	0.028**	-0.001
	(0.012)	(0.011)
	0.802	*0.790*
Participated in the matriculation exams	0.037***	-0.004
	(0.011)	(0.015)
	0.763	*0.744*
Matriculation diploma	0.043*	-0.016
	(0.023)	(0.016)
	0.396	*0.362*
University-eligible diploma	0.033**	-0.005
	(0.013)	(0.012)
	0.300	*0.278*
Number of English units	0.181***	0.061
	(0.052)	(0.049)
	2.133	*1.994*
Number of Math units	0.156**	0.054
	(0.060)	(0.061)
	1.752	*1.585*
Number of Science subjects	0.092**	-0.005
	(0.041)	(0.033)
	0.688	*0.694*
Took the Psychometric Exam	0.028***	0.016
	(0.008)	(0.012)
	0.389	*0.378*
Post-secondary enrollment by age 19	0.034***	0.015*
	(0.006)	(0.008)
	0.157	*0.145*
Any juvenile criminal record (males)	-0.030***	0.007
	(0.011)	(0.006)
	0.166	*0.087*
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 exposure to the Preschool Law on various outcomes. The sample we include only the pre-treatment cohorts and the placebo treatment is defined at 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 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: 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 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

Appendix Table A7: 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)
Completed high school	0.028**	0.034***	0.022	0.027*	0.034***	0.033**
	(0.012)	(0.008)	(0.021)	(0.015)	(0.010)	(0.012)
	0.802	*0.802*	*0.802*	*0.802*	*0.802*	*0.790*
Participated in the matriculation exams	0.037***	0.040***	0.033*	0.038***	0.040***	0.038***
	(0.011)	(0.009)	(0.019)	(0.014)	(0.011)	(0.011)
	0.763	*0.763*	*0.763*	*0.763*	*0.763*	*0.757*
Matriculation diploma	0.043*	0.052**	0.031	0.036	0.050**	0.052*
	(0.023)	(0.023)	(0.027)	(0.025)	(0.023)	(0.027)
	0.396	*0.396*	*0.396*	*0.396*	*0.396*	*0.411*
University-eligible diploma	0.033**	0.044***	0.020	0.028*	0.038***	0.033**
	(0.013)	(0.012)	(0.018)	(0.015)	(0.012)	(0.015)
	0.300	*0.300*	*0.300*	*0.300*	*0.300*	*0.319*
Number of English units	0.181***	0.215***	0.138*	0.147***	0.222***	0.175***
	(0.052)	(0.050)	(0.074)	(0.054)	(0.044)	(0.058)
	2.133	*2.133*	*2.133*	*2.133*	*2.133*	*2.218*
Number of Math units	0.156**	0.201***	0.099	0.129*	0.185***	0.173**
	(0.060)	(0.056)	(0.071)	(0.064)	(0.056)	(0.069)
	1.752	*1.752*	*1.752*	*1.752*	*1.752*	*1.808*
Number of Science subjects	0.092**	0.085*	0.099**	0.115***	0.083*	0.129***
	(0.041)	(0.048)	(0.043)	(0.039)	(0.043)	(0.038)
	0.688	*0.688*	*0.688*	*0.688*	*0.688*	*0.707*
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*
Post-secondary 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 record (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 impact of exposure to the Preschool Law 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 pre-treatment cohorts 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 A8: 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	0.603***	0.555***	0.009
	(0.050)	(0.051)	(0.033)
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