

**Public Preschool and the Labor Supply of Arab Mothers:
Evidence from a Natural Experiment**

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Abstract

Low labor force participation among mothers in North Africa and the Middle East is often attributed to cultural factors independent of economic considerations. I examine the effects of a reduction in child care costs on preschool enrollment, and Arab mothers' labor supply and fertility using variation in universal preschool provision generated by the gradual implementation of the free preschool law in Israel. The results show that universal preschool provision generated a sharp increase in preschool enrollment and mothers' labor supply without affecting short-run fertility. The increase in labor supply occurred mainly among more educated mothers.

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I. Introduction

Recent calls for policy efforts to raise female labor force participation in the developing world have led to an increased interest in the role that child care availability plays in determining women's employment decisions in low-income countries.¹ The motivation for studying the child care/labor force participation nexus in this case, is broader than traditional questions of income and welfare raised in industrialized countries: female labor force participation is often viewed as a fundamental key to the promotion of gender equality, women's empowerment, and, more broadly, to economic progress and modernization (World Bank, 2001).

While gender gaps in education and health have narrowed considerably in most developing countries, large differences in employment rates by sex persist, especially in Middle Eastern and North African countries (UNDP, 1995; World Bank, 2004). Much of this persistence has been attributed to cultural factors and traditional gender roles (e.g., Moghadam, 1998; United Nations, 2004; World Bank, 2004; Youssef, 1972), but little is known about the effects of economic incentives on female labor supply in the Arab world or other traditional societies. In particular, there is limited empirical evidence on the effects of child care costs for these populations.²

In contrast to the relative thin evidence available for less developed countries, the impact of child care costs on female labor supply has been studied extensively in the U.S. and Western Europe where most of the empirical evidence comes from observational studies. Few of these studies rely, however, on experimental or quasi-experimental research designs. This literature is reviewed by Anderson and Levine (1999), Blau (2003) and Blau and Currie (2006) who agree that there is still considerable uncertainty about the magnitude of the effects of child care costs on female labor supply.

This paper evaluates the impact of free preschool provision on children's enrollment and mothers' labor supply among Israeli Arabs, a group characterized by very low rates of both preschool enrollment and female labor force participation. In September 1999, the Israeli government began the gradual implementation of a policy change known as the Preschool Law, which provides free preschool education for all children aged 3 and 4. The law was implemented

¹ These calls have been put forth at several international conferences in the past decade (e.g. the Beijing Platform for Action, the Beijing+5 declaration and resolution, the Cairo Programme of Action, the Millennium Declaration, and the Convention on the Elimination of All Forms of Discrimination against Women [CEDAW]).

² Some exceptions include Conelly et al. (1996), Lokshin et al. (1999), and Wong and Levine (1992) for Brazil, Kenya and Mexico respectively who all find a significant impact of the price and availability of child care services on mother's labor supply.

in different towns at different times. This policy change provides a unique opportunity to assess the effects of child care costs using a quasi-experimental research design that compares towns that had and had not yet begun to supply free preschooling.

By looking at the impacts of preschool provision on the labor supply of Arab mothers in Israel, this paper also contributes to the limited literature on the determinants of female labor force participation within ethnic minorities that maintain a traditional lifestyle in the U.S. or in Western Europe.³ The labor force participation of ethnic minorities, and in particular, the labor force participation of Muslim women is widely seen as important for social and economic integration in western life (e.g., Commission of the European Communities, 2003; Doornik, 2000; Rudiger and Spencer, 2003).

The evidence presented here suggests that the provision of preschool services in Arab towns significantly increased preschool enrollment and mothers' labor supply. The increase in the labor supply occurred mainly among more educated mothers. At the same time, there is no evidence of labor supply effects among women who lived in affected towns but do not have children of preschool age. There is also no evidence of an effect of preschool provision on fertility. This last question is of interest because female employment is widely believed to be a causal factor that reduces fertility.

The remainder of this paper is organized as follows. Section II outlines a theoretical framework and summarizes the literature linking public preschool provision with female labor supply and fertility. Section III presents descriptive background on Israeli Arabs and describes the implementation of the Preschool Law. Section IV describes the data and sample used in this paper and details the identification strategy. Section V discusses the results and Section VI concludes.

II. Theoretical Framework

In the context of a static model of labor supply, and assuming that the mother is the provider of child care, that there is only one child in the family who needs care, and that the mother cannot take care of her child while she is working, the mother's budget constraint in the absence of free preschool can be written as $c=y+(w-p)h$ given the usual time constraint ($h+l=24$). Where c is consumption in commodities other than child care, y is non-labor income, w is the hourly wage, p

³ Recent studies (e.g. Fernández and Fogli, 2009; Reimers, 1985 and Read, 2004) have emphasized the importance of cultural values and beliefs in determining female labor force participation of different ethnic groups living in western countries. Few studies, however, have analyzed how public policies affect these communities.

is the hourly price of child care, h is hours of work, and l is hours spent on leisure/child care. The net wage of the mother is therefore, $w-p$.

The provision of free preschool generates an implicit subsidy of 100 percent for child care, in which the number of hours and quality provided are fixed. As shown in Figure 1, the provision of free preschool for a fixed amount of hours h^* changes the slope of the budget constraint up to h^* generating a kink at this point. This policy entails a substitution effect for mothers who would not have worked in the absence of this service ($h=0$) providing a work incentive for this group; It entails an income effect for mothers who would have worked $h>h^*$ hours in the absence of this service (i.e. more hours than those provided by public preschool); and a mixture of both effects for mothers who would have worked $0<h \leq h^*$ hours (i.e. fewer hours than those provided by public preschool).⁴ The provision of public preschool is therefore expected to increase female labor supply, while the effects on hours worked is ambiguous.

The empirical literature on the effects of child care costs on female labor supply is very extensive for the U.S. and European countries. Most studies are based on non-experimental data which usually lack information on three essential variables: the potential wages of mothers who do not work, the child care costs of mothers who do not purchase child care, and the quality of purchased child care. An approach frequently taken by researchers (e.g. Connelly, 1992; Kimmel, 1995 and 1998 and Ribar, 1992) has been to estimate the effects of child care costs on women labor supply with a sample of mothers who were employed and who paid for child care, applying corrections for selectivity. Others have formulated structural models (e.g. Michalopoulos, et al., 1992 and Ribar, 1995), exploited the nonlinear feature of the childcare tax credit (e.g. Averett, et al., 1997) or used geographic variation in child care costs (e.g. Blau and Robins, 1988).

These and other studies have provided distinctly different estimates for the elasticity of employment with respect to the price of child care ranging from 0 to values greater than -1. Part of the discrepancy between the estimates can be explained by differences in the population studied and the data sources used, but it mainly suggests the possibility of biases generated by misspecifications of the functional forms for the employment and child care equations or violations of the exclusion restrictions (e.g. identification through variations in child care costs that are not strictly exogenous to the employment decisions).

Several demonstration programs for low-income families have provided child care subsidies.⁵ The random assignment that characterizes these programs offers a compelling research

⁴ See Gelbach (1999) and Blau (2003) for similar analysis. For more general models on child care costs and female labor supply see, Heckman (1974), Michalopoulos et al. (1992) and Connelly (1992).

⁵ See Gennetian et al. (2001) for a review of recent demonstration programs.

design. However, since most of these programs offer additional services and economic incentives aimed to promote employment, it is not possible to isolate the effects of child care subsidies.

Perhaps the most compelling strategy, in the absence of a randomization, is to rely on natural experiments. One study that applies this approach is Berger and Black (1992), who compare differences in employment between women in Kentucky receiving child care subsidies with those enlisted on a waiting list for the program. Here, the main concern, noted also by the authors, is that the behavior of mothers on the waiting list does not provide an ideal counterfactual for the behavior of mothers accepted into the program.

A second study is Gelbach (2002), who uses children's quarter of birth as an instrument for public school enrollment of five-year old children and finds significant effects on mothers' labor supply. Gelbach (2000) appears to be the first to study the elasticity of mother's employment with respect to the provision of universal preschool, which is also the focus of this study.⁶ Fitzpatrick (2010) applies a similar strategy using children's exact date of birth to compare the labor supply of mothers of 4-year-old children located within a narrow window around the cutoff birth date for pre-K enrollment in Georgia and Oklahoma. Interestingly, she finds that universal pre-K raised preschool enrollment but has no effect on mothers' labor supply.

As an alternative strategy, this paper and three recent studies exploit variation across geographical areas over time in the provision of preschool services and apply a difference-in-difference identification strategy. Baker et al. (2008) rely on this approach to evaluate the effects of a program that provided universal and highly subsidized child care in Quebec and Cascio (2009) applies a similar strategy to study the effects of public preschool in the US. Both studies find sizeable labor supply effects on maternal employment.⁷ Berlinski and Galiani (2007) exploit variation in preschool provision generated by a national construction program of preschool facilities in Argentina and find that expansions of preschool provision increased maternal employment. Results reported in these studies might not necessarily apply for women from more traditional or non-western societies such as the Arab population analyzed in this paper.

⁶ One concern about Gelbach (2000) is the validity of the exclusion restrictions related to the quarter of birth instrument. There are some reasons to suspect that child's quarter of birth could be directly related to mother's employment either because mothers who give birth at different seasons might have different demographic characteristics and employment patterns or because children born in different seasons might have different health and development trajectories (see, e.g. Bound et al., 1995 and Bound and Jaeger, 2000). In addition, this strategy assumes that child's age is unrelated to mothers' labor supply.

⁷ Cascio (2009) finds significant effects for single mothers with a kindergarten eligible child and no younger children but no effects for married mothers. On the other hand, Baker et al. (2008) focus on married mothers only and significant effects for this group.

In contrast to the large existing literature on child care costs and female labor supply, a relatively limited number of studies have looked at the effects of child care costs on fertility. In this case, the theoretical predictions are less clear. On the one hand, a reduction in the price of child care tends to reduce the cost of childrearing. On the other hand, lower child care costs are likely to enhance mothers' labor force participation, which in turn might increase the cost of children through an increase in the opportunity cost of women's time. Thus, the net (direct plus indirect) effect of child care costs on fertility is ambiguous.⁸

The empirical evidence on the effects of child care costs on fertility comes mainly from non-experimental studies which have provided mixed results. For example, Blau and Robins (1989) find that higher child care costs lowered the fertility for unemployed but not for employed women in the U.S. and Wong and Levine (1992) report that, the availability of child care increased female labor force participation in Mexico but did not affect the likelihood of their having more children. Hank and Kreyenfeld (2003) argue that the availability of informal child care (mainly from relatives) in Germany has a positive impact on fertility while the availability of public child care does not produce any effect. On the other hand, Del Boca (2002) finds a positive effect on the fertility of Italian women generated by the availability of both formal and informal child care.

Two recent studies examine the link between childcare costs and fertility using quasi-experimental research designs. Furtado and Hock (2010) find that low skilled immigration reduced childcare costs in the US, increasing fertility and reducing the labor force participation of college educated native females. Mork et al. (2010) exploit reductions in childcare costs generated by a Swedish reform and show that they were associated with an increase in fertility among married couples without previous children but a reduction in fertility for households with children already enrolled in childcare.

III. Background

Female Labor Supply and Preschool Services among the Arab Population in Israel

The Arab population in Israel accounts for 20 percent of the Israeli population and numbered 1.3 million people at the end of 2004. Arabs in Israel belong to three major religious groups: 83 percent are Muslims, 9 percent are Christians, and 8 percent are Druze (CBS, 2005). Most of the Arabs live residentially segregated from the Jewish population. Nearly 71 percent live in Arab towns and villages (in which they comprise almost the entire population), 25 percent live in mixed towns

⁸ One could argue, based on a similar mechanism, that the effects of child care costs on female labor supply are also ambiguous. However, it is reasonable to assume that the indirect effect of a reduction in child care costs on female labor supply driven by a possible increase in fertility is negligible.

(populated by Arabs and Jews), and 4 percent, mostly Bedouins, live in places that have not been officially recognized by the Ministry of Interior (CBS, 2002).

The Arabs in Israel are still considered a traditional society, highly influenced by the Muslim culture especially in regard to gender relations, sexuality and gender roles (Khattab, 2002). Despite a sharp increase in female labor force participation (LFP) among the Arab sector throughout the last decades (which has almost quadrupled since the 70's), the participation of Arab women is still remarkably low compared to that of their Jewish counterparts. For Arab women aged 25-54, for example, the LFP rate in 2001 was 22 percent, in contrast to 78 percent for Jewish women of the same age group. It is worth noting that the LFP rate of Israeli Arab women is similar to that of the neighboring Arab populations. For example, the LFP rate for the same period in Jordan was 17 percent, 26 percent in Lebanon, 17 percent in the West Bank and Gaza Strip, 20 percent in Syria and 24 percent in Egypt (ILO, 2002).

Besides cultural and traditional constraints, other factors often cited as hindering the labor force participation of Israeli Arab women are their low level of education, their high levels of fertility and limited employment opportunities available in proximity to their place of residence.⁹ It has also been argued that the lack of child care services in Arab towns constitutes an additional barrier for the employment of Arab mothers (Abu Jaber, 1992; Kamah, 1984).

In sharp contrast with the Jewish sector, where preschool attendance has been significantly high since the 1970s, preschool attendance of Arab children has been remarkably low. For example, in 1999, enrollment rates in public preschools and day-care centers for Jewish children aged 3 and 4 were 79.7 percent and 89.1 percent respectively while the equivalent rates for the Arab population were 21.3 and 32.2 percent. Enrollment rates in private preschools were 9.6 and 3.8 percent for Jewish children aged 3 and 4, and 1.2 and 1.4 percent among the Arabs (CBS, 2000).

Although data on enrollment in private institutions are probably incomplete, misreporting can explain only a small part of the low enrollment rates among Arab children as suggested by Figure 2. The figure describes the type of child care reported by Jewish and Arab mothers with preschool age children.¹⁰ While 78 percent of the Jewish mothers reported that the child spent part

⁹ See Ben-Porath (1966), Grossbard-Shechtman and Neuman (1998), Khattab (2002), Lewin-Epstein and Semyonov (1992), and Semyonov et al. (1999).

¹⁰ The data comes from a survey conducted by the Research Department of the Ministry of Industry, Trade and Labor during 2000 and is based on a probability sample designed to reflect the characteristics of the population of Israeli women.

of the day in some form of non-parental care, 67 percent of the Arab mothers reported being themselves the main providers of child care.¹¹

Enrollment of five-year old Arab children has been significantly higher compared to that of younger children. For example, the enrollment rate in 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 can be mainly attributed to the fact that free and compulsory kindergarten for this age has been endorsed by the Israeli government since the creation of the State.

In contrast, until 2000, provision of public preschools for ages 3 and 4 fell mainly under the auspices of the local authorities, who were not obliged by law to supply such service. 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 or children who resided in areas defined by the government as targets for development.¹² 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 towns 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.

In the absence of public preschool services, a private market for child care has not developed in the Arab sector as it has in the Jewish sector. The main cited explanations are a limited demand given the low labor force participation of Arab mothers, and a lack of parental awareness regarding the importance of early childhood education (e.g., Rosenthal, 1992 and Blas and Adler, 2004). Yet, no empirical evidence has been provided to show that the lack of demand has been the reason for the low preschool enrollment of Arab children.

¹¹ Interestingly, preschool enrollment rates for Israeli Arab children are similar to those reported by Chiswick and DebBurman (2006) for immigrant children of Middle Eastern origin in the U.S. The authors report a preschool enrollment rate of 34% among immigrant children from Middle East countries which, together with Mexican immigrants, have the lowest preschool enrollment rates compared to other immigrant or native populations.

¹² These are towns classified as National A Priority, as Border Towns, new settlements and disadvantaged neighborhoods and towns included in the Urban Renewal Project. More information (in Hebrew) can be found in the Budget proposal of the Ministry of Education at the Ministry of Finance website: www.mof.gov.il or at the Ministry of Education website: www.education.gov.il.

The Compulsory and Free Preschool Law for Ages 3 and 4

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. The implementation of the law started in the most disadvantaged towns, with the intention of including additional towns each year, and covering the entire country within ten years.¹³ The time frame for the addition of towns was determined according to their socio-economic cluster, which ranges from 1 (lowest) to 10 (highest).¹⁴

Beginning in September 1999, free preschool services were provided in towns classified into clusters 1 and 2, and in towns and neighborhoods that had received preschool subsidies of 80-90 percent prior to the Preschool Law.¹⁵ Because many towns (mainly Arab towns) were not completely set to provide preschool services to the whole population, they were formally included into the law two years after their informal inclusion. Most preschools operated in full-day basis (41 hours a week) during the official school calendar (from September 1st until June 30).

After 2000, the expansion of the law's coverage has been considerably limited due to government budgetary constraints. The only developments have been the addition of those towns that, according to new updates of the socio-economic index, have come to be classified in clusters 1 and 2 and a partial provision of free preschool services in the Bedouin unrecognized villages.¹⁶ To date, there has been no enforcement of compulsory education in any of the towns included in the law mandate.

Descriptive data on the population of children aged 3-4 in 2003 according to the eligibility of their place of residence for preschool subsidies are presented in Table 1.¹⁷ 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. The majority of the Arab

¹³ For a review about the implementation of the law see Blas and Adler (2004) and Kop (2002).

¹⁴ The Israeli Central Bureau of Statistics computes a socio-economic index for the towns which reflects a combination of some basic characteristics such as financial resources of the residents, housing, education, employment, etc. Towns are then 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).

¹⁵ These towns are those classified as National A Priority, as Border Towns, and disadvantaged neighborhoods and towns included in the Urban Renewal Project.

¹⁶ Bedouin unrecognized villages are those villages not officially recognized by the Ministry of Interior. Because of their special status, they could not be listed officially in the Preschool Law, but they were informally included.

¹⁷ Population data are based on estimates by religion, age, and town of residence for the year 2003 provided by the Israeli Central Bureau of Statistics.

children covered by the law enjoyed an enhanced access to preschool education for the first time. Still, about fifty percent of all Arab children and the vast majority of Jewish children were not covered by the law in 2003.¹⁸ The situation has not changed since then as there were no further expansions to the law implementation.

Figure 3 presents data on enrollment rates in Arab towns differentiated by the extent of public support for preschool education.¹⁹ In the years that preceded the Preschool Law (1998 and 1999) the enrollment rates of Arab children aged 3-4 in towns receiving subsidies of 80-90 percent was 76 percent while enrollment in other Arab towns was significantly lower - 21 percent. Since 2000, there was a dramatic increase in the enrollment rate of Arab towns that were first provided with free preschool, reaching a rate of 83 percent in 2003. In contrast, the growth in enrollment among those not included in the law was small, reaching a rate of 30 percent in 2003. There was also a rise in the enrollment rates in towns 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 provision did not affect the enrollment of Arab children aged 5 which remained almost stable over the analyzed period. In addition, it appears the Preschool Law did not produce any significant change in enrollment of Jewish children.

IV. Sample description and empirical strategy

The empirical analysis is based on a comparison between towns that were provided with free preschool services by the law and had not received previous preschool subsidies (hereafter: treated towns) and towns that have not yet been provided with free preschool (hereafter: comparison towns).

¹⁸ The actual size of the population with no free preschool is slightly smaller than the estimates presented here since some children who live in towns not included in the law coverage receive subsidies of 80-90 percent (i.e. children living in neighborhoods included in the Urban Renewal Project and new immigrants).

¹⁹ Data on preschool enrollment by town are based on administrative data from the Ministry of Education, Culture and Sport and was provided by the Israeli Central Bureau of Statistics. 1998 is the first year available for enrollment counts by town. Enrollment rates were computed by the author based on this data and the population data cited above.

The primary data source is the geographic version of the Israeli Labor Force Surveys (LFS) for the years 1998 through 2003.^{20, 21} For reasons of confidentiality, the Israeli Central Bureau of Statistics (CBS) scrambles the town codes for all towns numbering less than 10,000 inhabitants. Therefore, the sample of treated Arab towns that can be identified in the LFS is reduced to 11.²² All of these treated towns were informally provided with preschool services from September 1999 and were officially included in the Law in September 2001. The comparison group consists of two types of towns. The first type includes towns that belong to clusters 3 and 4 and were supposed to be provided with free preschool in the succeeding years (10 towns). The second type includes 3 towns that belonged to cluster 3 but, after a CBS update of the socio-economic index, they were reclassified into cluster 2 and were first provided with free preschool in 2003.²³

According to population estimates provided by the Israeli Central Bureau of Statistics, in 2003 there were 12,956 children aged 3-4 living in the treated towns included in my sample and 12,656 children in the comparison towns. Children from treated towns included in the sample represent 54 percent of all Arab children aged 3-4 who were first provided with free preschool services under the law. Children in comparison towns included in the sample represent 31 percent of the children of the same age who were not provided with free preschool services during the period of interest.

Figure 4 plots enrollment rates for ages 3-4 and 5 for the years 1998 through 2003 in treated and comparison groups. The figure resembles that obtained from the data based on all towns. The enrollment rate for ages 3-4 was similar in treated and comparison towns prior to the implementation of the Preschool Law. In 2000 there is a sharp increase in enrollment among treated towns (from 23 percent to 64 percent) which continues to rise in subsequent years reaching a rate of 85 percent in 2003. In comparison towns, there is a small increase in enrollment rates in 2001

²⁰ The LFS is composed of a rotating panel of households, similar to that of the U.S. CPS and is designed to reflect the characteristics of the Israeli civilian population aged 15 and older. Each year, quarterly surveys are conducted, with the interviewing spread over the entire quarter. Some key variables such as age are reported in grouped intervals for most of the years studied here. Thus, it is not possible to build a panel dataset of individuals.

²¹ Town codes can be identified from the years 1995 onwards. However, due to the population threshold limitation for the identification of towns, two towns included in the analysis can only be identified starting from 1998 onwards. In 1998 the CBS performed significant changes to the sampling and weighting schemes generating a structural break in the series. To keep a balanced panel across years and avoid the structural break in the data, I limit the analysis to the years 1998 through 2003. Results included previous years are highly similar to those reported in the paper.

²² One town is a mixed town (having both Jewish and Arab residents). The empirical analysis includes only the Arab population of this town. Results were not sensitive to the exclusion of this town.

²³ Since throughout most of the period covered by the empirical analysis these 3 towns did not have access to free Preschool services, they will be identified as comparison towns in the descriptive statistics based on cross-sectional comparisons. In the regression analysis these towns are identified as treated.

and the next increase takes place in 2003 when 3 towns are actually provided with free preschool. Preschool enrollment for the comparison group excluding the latter three towns reaches 29 percent in 2003. Enrollment rates for age 5 are almost identical in treated and comparison towns over the whole period and have not increased.

Although enrollment rates in public preschools before the Preschool Law were similar in treated and comparison towns, it is important to emphasize that treated towns are more disadvantaged than comparison towns as reflected in selected socio-economic indicators presented in Table 2. For example, treated towns have a higher dependency ratio, a higher rate of income support recipients, and a lower income per-capita.

To study the impacts of preschool provision on female labor supply, I define as the treated group all mothers of children aged 2-4 living in the 11 treated towns. The comparison group is composed of all mothers with children aged 2-4 living in the 13 comparison towns.²⁴ An alternative sample is composed of mothers whose youngest child is aged 2-4. The effect of the intervention is expected to be larger among this second sample since once provided with free preschool services, mothers whose youngest child is 2-4 face fewer constraints than those mothers who also have a younger child. A possible concern of basing the analysis on this group is that its composition could be endogenous. That is, the age of the youngest child could itself be an outcome of the intervention if fertility were also affected.²⁵ Nevertheless, as shown in section V.F., the provision of universal preschool has no effects on fertility.

Table 3 reports descriptive statistics and balancing tests for mothers of children aged 2-4 in treated and comparison towns for the period that preceded the Preschool Law (columns 1 through 4) and the period subsequent to it (columns 5 through 8). Focusing on the pre-law period we see that, treated and comparison mothers are about the same age (31) and they have also similar marriage rates (97 percent). However, reflecting the fact that towns selected to be treated first were the most disadvantaged, treated mothers seem to come from a lower socio-economic background. They have, on average, 4.3 children and 7.8 years of schooling. That is, one more child and 2.6 less years of schooling than mothers from the comparison group.

²⁴ Unfortunately, the LFS question about children's ages asks for an age range rather than the exact age of each child. Therefore, it not possible to distinguish those mothers of children aged 3-4 (who were actually affected by the intervention) from the group of mothers with children aged 2-4.

²⁵ Those mothers of children aged 2-4 who have joined the labor force as a result of the provision of free preschool might have a lower probability of having an additional child and, as a result, would more likely be included in the sample of those whose youngest child is 2-4.

The differences found in women's socio-economic background are reflected in their labor supply. The labor force participation of the treated group was only 6.0 percent as opposed to a participation rate of 16.1 percent in the comparison group. Treated mothers worked, on average, 1.46 hours a week while their counterparts from the comparison group, worked 4.81 hours.²⁶ While differences in labor supply are large and significant, once I control for the basic demographic characteristics reported in the table (age, age squared, years of schooling, marital status, number of children, and religion of head of household) the gaps between both groups narrow considerably and are not significant. For example, the controlled difference in labor force participation is -0.022 (s.e.= 0.031).

Differences in demographic characteristics between treated and comparison groups persist throughout the period following the Preschool Law. In this period, there was an increase in educational level of both groups but the gaps between them are almost the same. Treated mothers have 9.1 years of schooling as opposed to comparison mothers who have almost 11.²⁷ Other characteristics, like age, marital status and number of children, remain similar between both periods.

A remarkable change between the pre-law and post-law period is observed in female labor supply. While mothers of treated and comparison groups have increased their labor supply, the increase that has occurred among treated mothers is significantly higher. The LFP for the treated group has increased from 6.0 percent to 17.1 percent, whereas the equivalent increase for the comparison group was from 16.1 percent to 18.3 percent. A similar pattern is observed in weekly employment. While the proportion of employed mothers in the treated group has increased from 5.7 to 16.5 percent, the equivalent increase among mothers in the comparison group was from 15.8 to 17.3 percent.

It could be argued that part of the increase in the labor supply of the treated group is attributable to a differential change in women's characteristics or to a different growth rate in the LFP between treated and comparison towns. In order to assess these arguments and to control for additional confounding factors, I turn next to a regression analysis.

Empirical models

²⁶ Zero hours were assigned to non-working mothers.

²⁷ The increase in mothers' educational level is not unique to this sample. For example, the average years of schooling for the whole population of Arab mothers increased from 9.07 in 1998 to 10.08 in 2003.

To identify the effects of free preschool provision on female labor supply, I estimate alternative specifications of difference-in-differences models comparing the outcomes of women in treated and comparison towns before and after the Preschool Law. The estimating equation is:

$$(1) Y_{ijt} = X_{ijt}'\beta + \alpha D_{jt} + \varphi_j + \delta_t + \varepsilon_{ijt}$$

where i denotes individuals, j denotes towns, and t denotes time. Y_{ijt} is the outcome for woman i living in town j at time t ; X_{ijt} is a vector of women's characteristics that includes age and age squared, years of schooling, marital status, a set of dummy variables for the religion of the head of household, and a set of variables denoting the number of children in the following age groups: 5-9, 10-14 and 15-17;²⁸ D_{jt} equals 1 from the treatment day and forward and zero otherwise; φ_j is a town fixed effect; δ_t is a time effect (year-quarter fixed effect) and ε_{ijt} is the error term, which is composed of a town specific random element that allows for any type of correlation within observations of the same town across time, and an individual random element. The coefficient of interest is α which captures the effect of free preschool provision on female labor supply.

I estimated model (1) for the following measures of labor supply: labor force participation, working status from the week prior to the survey (which equals 1 whether the woman worked or was absent from work and 0 if she was unemployed or out of the labor force), weekly hours of work (which equals zero for those unemployed or out of labor force), and an indicator variable for doing any activity not related to housework (either studying or working).²⁹

The identification strategy relies on the assumption that in the absence of the intervention, differences in female labor supply between treated and comparison towns would have remained constant. Although this assumption cannot be verified, I performed in the next section several checks that confirm its feasibility. First, I estimate the impacts of universal preschool provision on mothers who do not have children of preschool age. Second, I repeat this check stratifying the sample by mother's age and looking at a narrower group of mothers who have children slightly older or younger than ages 2-4. In a third check, I perform a placebo experiment where I define treatment in a pre-treatment year. Finally, I perform additional checks that confirm that the results are unlikely to derive from a secular convergence in female labor supply between treated and comparison towns.

²⁸ I do not control for the number of children aged 0-4 because this could itself be an outcome of the intervention. For the same reason, I do not control for the presence of other adults in the household.

²⁹ Since only 1.3 percent of the mothers in my sample are studying, I define a single outcome for either studying or working. Results for the effect of the intervention on the probability of being studying (not presented, but available upon request) were not significant in any of the specifications. Unfortunately, the labor force survey does not include income data. So it is not possible to examine this outcome.

An additional assumption required for identification, is that the intervention did not affect employment in comparison towns. This assumption seems plausible since the vast majority of working mothers (70 percent) from treated and comparison towns worked in their town of residence throughout the period analyzed here. Moreover, it is unlikely that mothers from comparison towns enrolled their children in preschools located in treated towns because the eligibility for tuition exemption is based on place of residence.

Finally, I also assume that mothers in comparison towns did not increase their labor supply because they expected to receive free preschool in the following year (as was originally declared). This seems reasonable since there is a lot of uncertainty regarding the time that laws are implemented in Israel. For example, the preschool law passed in 1984 although its implementation has not begun until September 1999. Moreover, the expansion to additional towns has not started yet.

The analysis of female responses to economic incentives in the context of a traditional society raises the question of how much time it could take for changes to occur. In an attempt to explore this, I estimated the following model that allows a dynamic response to the intervention:

$$(2) Y_{ijt} = X_{ijt}'\beta + \sum_{k=1}^4 \alpha_k K_{kjt} + \varphi_j + \delta_t + \varepsilon_{ijt}$$

where K_{kjt} $\{k=1$ to $4\}$ equals 1 if free preschool services have been provided for k years in town j at time t , and zero otherwise.

V. Empirical Results

A. Effects on Labor Supply

Table 4 reports difference-in-differences estimates for the effects of the provision of free preschool on female labor force participation. The estimates in columns 1 through 3 are based on the sample of mothers of children aged 2-4 while the estimates in columns 4 through 6 are based on the sample of mothers whose youngest child is aged 2-4. Panel A reports the mean effect for the post-law period (model 1) and panel B, the differential effect by the number of years since free preschool was first provided (model 2). Three estimates are presented for each sample which are based on different specifications of the basic model: one excluding town fixed effects (while adding a treatment main effect), one excluding women's covariates and one including both town fixed effects and women's covariates.

The three estimates are similar although those based on the complete specification are smaller. The estimate based on the complete specification (column 3) suggests that free preschool provision increased the labor force participation of mothers of children aged 2-4 by 0.071

(s.e.=0.034) percentage points. This is a significant increase considering that the labor force participation of this group in the post-law period is only 17.1 percent.³⁰ The impact of the free preschool provision among mothers whose youngest child is 2-4 years old is 0.117 (s.e.=0.035). As expected, this effect is larger than the effect found among mothers of children aged 2-4.

Estimates of the differential impact of free preschool by the number of years since it was provided (panel B) are positive but less precise. Overall, they suggest that for mothers with children aged 2-4, significant effects on labor force participation begin to take place mostly in the fourth year of the provision of free preschool. The estimates for mothers whose youngest child is aged 2-4 show a clear picture of growing effects that begin to be significant in the third year of preschool provision.

Alternative explanations can be attributed to this finding. First, female labor force participation depends not only on tangible constraints, such as the price of child care, but also on non-tangible constraints, such as cultural norms regarding the role of women in the household. It is possible that once a tangible constraint is released, pressure to relax cultural constraints begins to take place, but it takes time to produce a change. Second, as previously described, treated towns that are included in this sample were formally incorporated into the law in the third year of free preschool provision. Mothers could have been wary about the stability of the provision of free preschool and could have decided to join the labor force only after becoming confident in the continuity of the service and its endorsement by law. Third, the second year of the provision of free preschool overlaps the beginning of the second *Intifada*. It could be that mothers did not search for jobs during the first period of political instability. Unfortunately, there is almost no variation in the initiation date of the free preschool provision or in the date of formal addition to the law in towns included in the sample. Thus, no distinction can be made between the alternative explanations proposed.

Table 5 shows difference-in-differences estimates for the effects of the Preschool Law on additional measures of labor supply for mothers of children aged 2-4 and for mothers whose youngest child is 2-4 years old. The coefficients presented in the table correspond to the full specification which controls for mother's covariates, time fixed effects and town fixed effects. Results are perfectly in line with those obtained for the labor force participation. Mothers of

³⁰ Throughout the discussion of the results, it is important to bear in mind that treatment effects are estimated for mothers of children aged 2-4 while preschool services were provided for children aged 3-4. Accordingly, assuming that the labor supply of mothers of children aged 2 did not change due to the intervention, the estimated effects among mothers of children 3-4 is expected to be larger than the reported treatment effects. However, since the baseline labor supply among mothers of children aged 3-4 is presumably higher, the percentage increase in labor supply is expected to be of the same magnitude.

children aged 2-4 living in treated towns increased the probability of being employed by 8.1 percentage points (s.e.=0.035) and the average weekly hours of work by 2.831 (s.e.=1.201).

Similar to previous findings, estimates based on the sample of mothers whose youngest child is 2-4 (columns 4 through 6) are larger than those based on the whole sample of mothers with children aged 2-4. For example, the estimate for being employed is 0.126 (s.e.=0.035) and the estimate for weekly hours of work is 3.885 (s.e.=1.253). Estimates for the number of years since free preschool was first provided (panel B) are also similar to those obtained previously. Again, the effects begin to be significant from the fourth year of free preschool provision for mothers of children aged 2-4 and from the third year for mothers whose youngest child is 2-4.

Table A1 in the Appendix presents treatment effects on female labor supply estimated from probit models. Estimates for the main treatment effects are analogous to those obtained in Tables 4 and 5. Estimates for the dynamic response to the intervention are slightly larger than those obtained previously and show a clear pattern of increasing effects.

B. Possible Threats to Identification

One may ask whether the labor supply effect found among treated mothers can be attributed to the intervention or whether it is simply a result of a convergence in the outcomes of treated and comparison towns or specific shocks that have distinctly affected both groups of towns. To address this, I performed several checks.

The first task is to check whether the effects found among mothers of children aged 2-4 residing in treated towns are also found among mothers living in the same towns who have no children of that age. Therefore, I estimated the same models (1) and (2) on a sample of mothers having no children of preschool age. However, if the intervention has had any impact on other women residing in treated towns, this test will be misleading.³¹ Nevertheless, in analyzing the impacts of labor market programs, it is also of interest to examine the impact on populations not directly affected by the intervention.

Estimates presented in Table 6 suggest that the provision of preschool services did not affect the labor supply of mothers with no preschool age children. Estimates for the mean treatment

³¹ This intervention could generate various types of externalities among untreated populations: first, an increase in female labor supply could affect the employment of other women through general equilibrium effects in local labor markets; second, the provision of free preschool has generated an increase in the demand for child care staff that could in turn increase employment opportunities for all women; third, if the provision of free preschool has long-lasting effects on employment, it could affect the employment of women with older children (because some of them were treated in the past); fourth, the increase in the labor supply of mothers with young children could change some cultural norms of the town, raising also the labor supply of other women with no preschool age children.

effect are essentially zero for all outcomes and estimates for differential effects by the number of years since free preschool services were provided are insignificant and show no clear rising trend like that found for mothers whose youngest child is aged 2-4.

In an alternative specification, I pooled the sample of all mothers and estimated a *triple difference* model in which the difference in outcomes for mothers of children aged 2-4 between treated and comparison towns and the pre and post law period is contrasted with the equivalent difference among mothers who do not have children of that age. The models distinguish any confounding factors that could be distinctly affecting women from treated and comparison towns over the period of interest. The estimating equations are

$$(3) Y_{ijt} = X_{ijt}' \beta + \alpha_1 D_{jt} + \alpha_2 D_{jt} C_{ijt} + \mu_1 C_{ijt} + \mu_2 T_j C_{ijt} + \varphi_j + \delta_t C_{ijt} + \delta_t + \varepsilon_{ijt}$$

$$(4) Y_{ijt} = X_{ijt}' \beta + \sum_{k=1}^4 \alpha_{1k} K_{kjt} + \sum_{k=1}^4 \alpha_{2k} K_{kjt} C_{ijt} + \mu_1 C_{ijt} + \mu_2 T_j C_{ijt} + \varphi_j + \delta_t C_{ijt} + \delta_t + \varepsilon_{ijt}$$

where C_{ijt} is a dummy variable that indicates whether a woman i , in town j , and time t has a child aged 2-4; and T_j is the treatment status for town j . I estimated these models using two alternative samples: one that includes all mothers in treated and comparison towns and one that excludes mothers of children aged 2-4 who also have a younger child.³²

Results reported in Table 7 are highly consistent with results from previous tables. Estimates for the overall effects of preschool provision reported in panel A are practically zero while estimates attached to mothers of children aged 2-4 are significant and large.

Estimates for the dynamic impacts of the intervention reported in panel B suggest that the effects of preschool provision begin to be significant after four years for mothers with children aged 2-4 and after three years for mothers whose youngest child 2-4 years old. The analogous estimates for mothers with no preschool age children show no clear rising trend and provide additional support for the validity of the identifying assumptions. The absence of any effect among mothers with no preschool children also weighs against the existence of peer effects in labor supply decisions or any general equilibrium effects of preschool provision on employment.

The absence of any effect among women with no children of preschool age suggests that the increase in labor supply observed among mothers with children aged 2-4 is unlikely to derive

³² I also estimated an alternative specification where I replaced the main treatment indicator with a vector of town-specific time fixed effects including also state fixed effects, state fixed effects interacted with an indicator for having a child aged 2-4, time fixed effects and time fixed effects interacted with a child 2-4 indicator. Estimates from this specification (not reported here but available upon request) are highly similar to the main results.

from female labor force participation in treated towns simply catching up with that of comparison towns. However, it could be argued that convergence between the two groups have taken place solely among the younger population. Younger women also happen to have younger children, a fact that could generate a spurious treatment effect if the labor supply of treated and comparison towns is converging among younger women.³³

To assess this, I estimated a modified version of model (1), allowing treatment effects to vary with the age of the mother. Specifically, I broke up the main treatment effect into two effects: one for younger mothers (aged 30 or less) and one for older mothers (older than 30). I also added to the model controls for a main effect for each age group (young and old) in treated and comparison towns and allowed the two age groups to have different time trends. I estimated this model for the sample of mothers of children aged 2-4 as well as for the sample of mothers with no children of that age. If treatment effects are being confounded with a convergence in the labor supply of younger mothers, we should expect a higher treatment effect among younger mothers.

The results shown in Appendix Table A2 provide no evidence of a differential increase in the labor supply of young mothers residing in treated towns following the provision of free preschool, neither among those having children aged 2-4 (columns 1 through 4) nor among those having no children of that age (columns 5 through 8). Treatment effects for young mothers with children aged 2-4 are almost identical to those obtained for older mothers with children of the same age. For example, the effect on labor force participation is 0.069 for younger mothers and 0.065 for older mothers. Treatment effects for mothers with no children aged 2-4 are neither significant for younger nor for older mothers.

I also performed some additional robustness checks to assess the validity of the main identifying assumption. Results of these checks are reported in Appendix Table A3. First, I estimated an additional placebo test limiting the sample to mothers of children aged 0-1 or 5-9 (who do not have children aged 2-4). These mothers are similar to mothers of preschool children and might therefore provide a good indication for the existence of differential trends in labor supply between treated and comparison towns. However, if preschool provision had any long-lasting effect on employment, it could affect the employment of mothers of children aged 5-9 living in treated towns (as some of them received treatment in previous years). Estimates for mothers with younger or older children are not significant and suggest that the main results are unlikely to be driven by some time-varying factors that affected women in treated towns.

³³ The average age of mothers with children aged 2-4 is around 30, while the average age of the mothers with no preschool age children is around 38.

I perform an additional test for absence of differential trends where I limit the sample to the two pre-treatment years (1998 and 1999) and examine the impact of a placebo treatment on the labor supply of mothers of children aged 2-4 and the labor supply of mothers with no children of preschool age. In this placebo experiment, treatment towns are assigned a treatment indicator one year before the actual provision of universal preschool. Estimates for mothers of children of ages 2-4 (reported row 2 of Appendix Table A3) and estimates for mothers with no children of preschool age (reported in row 3) show no impact of future preschool provision on the labor supply of mothers.

Because treated towns are more disadvantaged than comparison towns and have a lower baseline labor supply, one might be concerned that the main results could just derive from labor supply in treated towns “catching up” with labor supply in comparison towns. The absence of any impact on mothers with no preschool children or in the pre-treatment year suggests that this situation is unlikely. To further assess this issue, I examine the differential effect of preschool provision according to the town SES ranking. Recall, that treated towns have a lower SES ranking as this index was used to determine treatment eligibility. In addition, there is a clear negative association between the town’s ranking and female labor supply in pretreatment years.

I therefore estimate a model where I include, in addition to the main treatment indicator, an interaction between treatment and the town SES ranking in 1999 (a pre-treatment year).³⁴ If treated towns were simply “catching up” with comparison towns, we should expect a negative estimate for the interaction between the town SES ranking and treatment. Estimates for mothers of children aged 2-4 reported in row 4 of Table A3 show exactly the opposite. Namely, the increase in labor supply among mothers of children aged 2-4 appears to be larger in towns with a higher SES index. In contrast, in the sample of mothers with no children aged 2-4, the main treatment effect and its interaction with town SES ranking (reported in row 5 of the table) are jointly insignificant.

As a final check, to control for possible differences in trends across towns that might be spuriously correlated with the treatment effect, I estimated the basic model including also interactions between the town SES ranking and a time trend. The results for mothers of children aged 2-4 and for mothers with no children of preschool age reported in rows 6 and 7 of table A3, are virtually unaffected by the inclusion of time trends.

³⁴ To ease the interpretation of the results, I standardized the index using the data in analysis sample. Note that the main effect of the town SES ranking is absorbed by the town fixed effect.

Could the increase in mothers' employment simply reflect an adjustment to the increased demand for child care staff? To assess this issue, I would ideally like to examine the impacts of preschool provision on employment in preschool institutions. Unfortunately, the geographic version of the labor force survey for the years 1998-2000 details the economic branches at a 2-digit level. I can therefore only examine the impacts of preschool provision on the likelihood of working in the education sector.³⁵

In Panel A of Appendix Table A4 I first examine the main sectors where mothers worked during the period that preceded the preschool law. A significant share of mothers (40 percent) worked in the education sector, followed by the health sector which employed about 21 percent of mothers. The next prevalent sectors for Arab mothers were retail sales and welfare and social work, which employed 9 and 7 percent of working mothers respectively.

In Panel B of Table A4, I estimate a difference-in-differences model where the dependent variable is an indicator for working in the education sector. The estimate for mothers of children aged 2-4 is marginally significant and shows an increase in the likelihood of employment in education of about 4 percentage points.³⁶ Recall that the increase in employment reported in Table 5 is 8 percentage points. In contrast, there is no parallel increase in employment in education among mothers with no children aged 2-4. Overall, the absence of an impact for mothers with no children at preschool age and the impact observed among mothers of children aged 2-4 suggest that the increased demand of workers in the preschool sector had negligible effects on local employment and can only explain a small portion of the increase in employment of mothers of preschool age children.

C. Treatment Effects Based on Alternative Samples

In an attempt to improve the balance between the treated and the comparison group, I dropped from the sample all Bedouin towns (three towns in total) since they are the most disadvantaged among Arab towns and all of them belonged to the treated group. This approach is in the spirit of a regression discontinuity strategy where the analysis sample is based on those towns that are just below and just after the cutoff threshold for program eligibility (the town SES index).

³⁵ Occupation data is also aggregated at the 2-digit level for the years 1998-2000. I therefore face the same limitation when examining occupations. I chose to focus on economic branch (and not occupation) as there are also jobs in additional occupations (such as janitors and cooks) that might have been created by the provision of universal preschool.

³⁶ Labor demand in the Arab education sector increases continuously due to the population growth.

Unfortunately, I cannot make more refinements to this strategy as my sample includes only 24 towns.

Table A5 in the Appendix gives descriptive statistics for the sub-sample of towns that excludes Bedouin towns. Mothers from treated towns are still more disadvantaged than mothers from comparison towns, but the gaps between the two groups have significantly narrowed. For example, in the period that preceded the Preschool Law, the difference between the treated and the comparison group in the average years of schooling has decreased from -2.5 to -0.7 and the difference in the number of children has decreased from 1 to 0.4. The difference in the labor force participation has decreased from -0.101 to -0.070 and is not significant.

Table A6 in the Appendix shows the difference-in-differences estimates for this sub-sample. Results are similar to those obtained for the whole sample, but suggest a larger effect for the intervention. For example, among mothers of children aged 2-4, the estimate for labor force participation is 0.097 (s.e.=0.034) and the estimate for weekly hours of work is 3.675 (s.e.=1.197). The larger impacts observed among the relatively more advantaged towns suggest that my main results are unlikely to be driven by a secular convergence in employment between treated and comparison towns.³⁷ Estimates for mothers with no children of preschool age suggest the absence of any labor supply effect among this group.

In an additional attempt to find a comparison group as similar as possible to the treated group, I estimated difference-in-differences models using solely on the sample of treated towns comparing mothers with children aged 2-4 to women who have no children of preschool age.³⁸ This sample is more homogenous and it has the advantage that women share equal background conditions and face the same idiosyncratic shocks.

Results reported in Table 8, are virtually identical to previous results. For example, the estimate labor force participation using the sample of all mothers is 0.076 (s.e.=0.027) and the estimate using the sample that includes also childless women is 0.078 (s.e.=0.023) while the estimate obtained using the sample of children aged 2-4 in treated and comparison towns reported in Table 4 is 0.071 (s.e.=0.034). The similarity of the results obtained from these alternative comparison groups provides additional evidence for the causal impacts of the intervention.

³⁷ Moreover, descriptive statistics reported in Appendix Table A5 show that mothers in this subsample of treated towns end up having higher employment rates relative to mothers in comparison towns in the post-law period.

³⁸ This comparison is valid under the assumption that the intervention did not affect the labor supply of women with no children of preschool age. Results reported previously suggest that this is a reasonable assumption.

D. Heterogeneous Treatment Effects

I examined heterogeneous treatment effect of preschool provision by estimating a modified version of model (1) allowing interactions of the treatment effect with women's education (less than 12 years of schooling and 12 or more years of schooling) while adding the corresponding main effects for treated and comparison towns and specific time trends for each educational group.³⁹

The results reported in panel A of Table 9 show that the provision of free preschool services only affected the labor supply of the more educated mothers (those having at least 12 years of schooling). This could be a surprising finding given that, according a basic labor supply theoretical model, the provision of child care subsidies is expected to increase the labor force participation of mothers at the margin, who are typically associated with low earnings potential and low educational levels. The case of Arab women in Israel may be somewhat different.

Arab mothers with low education belong to more traditional families in which the roles of women are still restricted to activities within the house and the family. The labor force participation for this group in my sample is only 4.2 percent. Therefore, it seems unlikely that solely the provision of preschool services will induce them to work. For skilled Arab mothers the situation is different. The difficulties faced by them in joining the labor force are not directly comparable to those faced by Jewish mothers. Their earnings are lower than their Jewish counterparts and, as noted earlier, there were almost no preschool services in their town of residence before the Preschool Law.⁴⁰

The labor supply effect found among skilled mothers may reflect a heterogeneous response to the intervention according to family size given the correlation between schooling and family size.⁴¹ In order to disentangle the response according to education from the response according to family size, I estimated a model (presented in panel B) that includes, in addition to the main treatment effect, two additive interactions of treatment. Specifically, I included an interaction of treatment with an indicator for 12 or more years of schooling and an interaction of treatment with an indicator for 3 or fewer children, while adding also the main effects for these covariates.

Results for mothers of children aged 2-4 suggest that the response to the intervention is mostly related to mother's education. There is also a positive effect for mothers who have fewer

³⁹ 39 percent of the treated mothers have 12 or more years of schooling in the post-law period, most of them have exactly 12 years of schooling.

⁴⁰ The monthly gross income from salaried work for Arab women aged 18-54 who have 12 or more years of schooling was NIS 3,908 in 2001 while the equivalent among Jewish women was NIS 5,540 (author's calculations from 2001 Income Survey).

⁴¹ Skilled mothers in treated towns have, on average, 3.04 children while unskilled mothers have, on average, 3.86 children.

children, although this effect is small and imprecisely measured. Estimates for the main treatment effect are small and not significant suggesting that the labor supply response of unskilled mothers or mothers with more than three children is practically zero. Results for mothers who do not have children aged 2-4 are not significant and provide further support for the validity of the identification strategy.

Because the wife's and husband's education are correlated, I also examined whether only educated mothers responded to the intervention or whether only mothers who have educated husbands were encouraged to enter the labor force. I estimated in panel C an additional model that allows for treatment interactions with the educational level of both partners based on a subsample of married mothers. The results suggest that public preschool provision affected the more educated mothers irrespective of their husband's level of education. This finding is in line with Grossbard-Shechtman and Neuman (1998) who also found that husband's schooling has no effect on the labor force participation of Muslim wives in Israel.

F. Short-Term Effects on Fertility

Based on a similar specification of models (1) and (2) I estimated the short-term effects of the provision of preschool services on recent fertility among those mothers who were directly affected by the intervention. I estimated a difference-in-differences model for the probability that a mother who has a child aged 2-4 has an additional child aged 0-1, while controlling for the covariates used in the previous models. I estimated both a linear probability model and a probit model. Since I found that the preschool provision mainly affected the labor supply of skilled mothers, I also estimated the differential effects for skilled and unskilled mothers.

The results, reported in Table 10 suggest that the provision of free preschool has not affected the fertility of mothers with children aged 2-4 in the short-run. The estimates for the average effect of the post-law period are almost zero and not significant. Estimates for the differential effects on fertility according to the number of years since free preschool services were first provided are of mixed sign and not significant in any of the years. Perhaps surprisingly, despite the increase in the labor force participation of skilled mothers, the fertility of this group also appears to have been unchanged. However, it is worth noting that the analysis presented above refers only to the short-term effects on fertility. Other impacts of the intervention such as the effects on child spacing or on total fertility will only become visible in a number of years.

VI. Conclusions

Low labor force participation of Arab mothers is often attributed to cultural factors independent of economic considerations. However, there is little evidence on the effects of economic incentives on female labor supply. In this paper, I fill some of this void by evaluating the effects of free preschool among the Israeli Arabs. I use a quasi-experimental research design based on the gradual implementation of the Preschool Law.

The results suggest that the provision of free preschool services increased both children's preschool enrollment and their mothers' labor supply. However, mothers' fertility appears to have been unchanged in the short-run. Interestingly, there were no labor supply effects among women with no preschool age children, a finding that enhances the credibility of the identification strategy and provides some evidence against the widely held belief on the central role of peer effects in the determination of work behavior.

The relatively high response to the intervention, which increased mothers' labor force participation by 7 percentage points, points to strong economic incentives effects on the labor supply of Israeli Arab mothers. On the other hand, this labor supply effect comes from educated women only.⁴²

⁴² This is consistent with Blau and Currie (2006) who report that within low-income households, recipients of child care subsidies tend to have higher education.

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c = consumption
 h = hours worked
 h^* = hours provided by preschool
 l = leisure
 p = price of child care

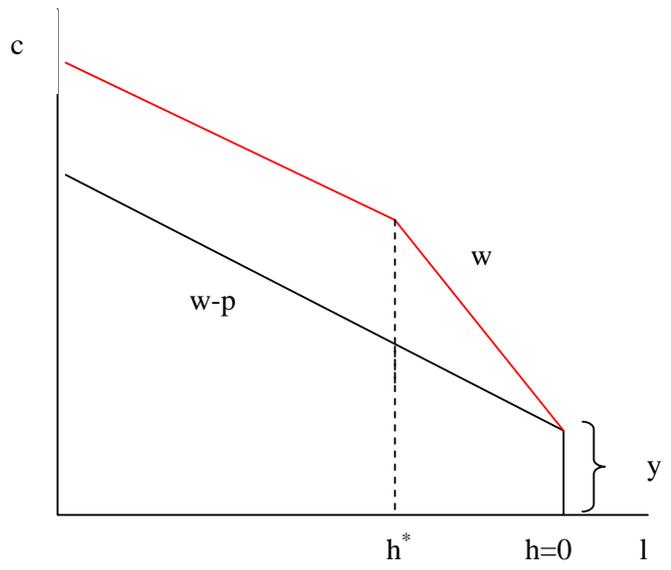


Figure 1: The effects of public preschool on mother's budget constraint

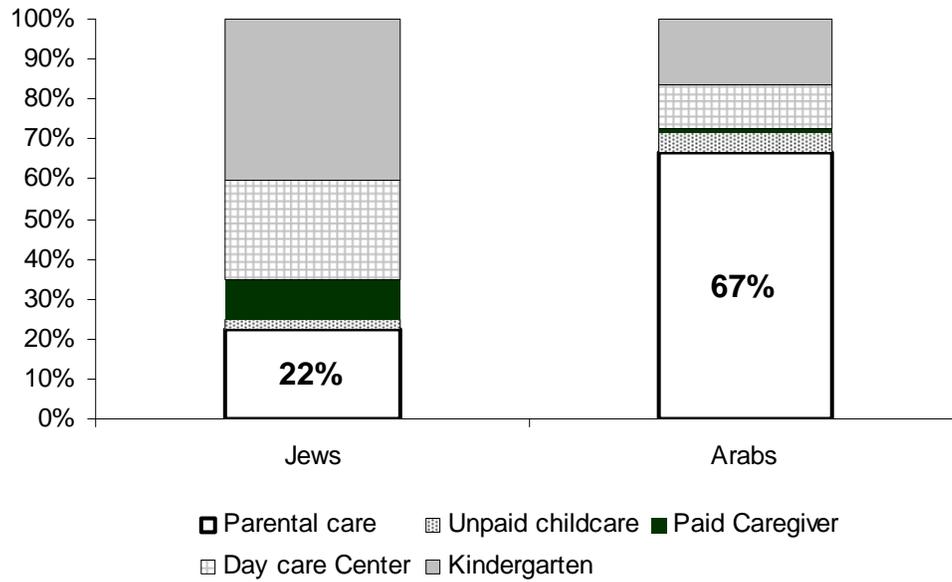


Figure 2. Type of Child Care for Children Aged 1-4

Notes: The figure presents data on the type of child care reported by Jewish and Arab mothers for their youngest child. The data comes from a survey conducted by the Research Department of the Ministry of Industry, Trade and Labor during 2000 and is based on a probability sample designed to reflect the characteristics of the population.

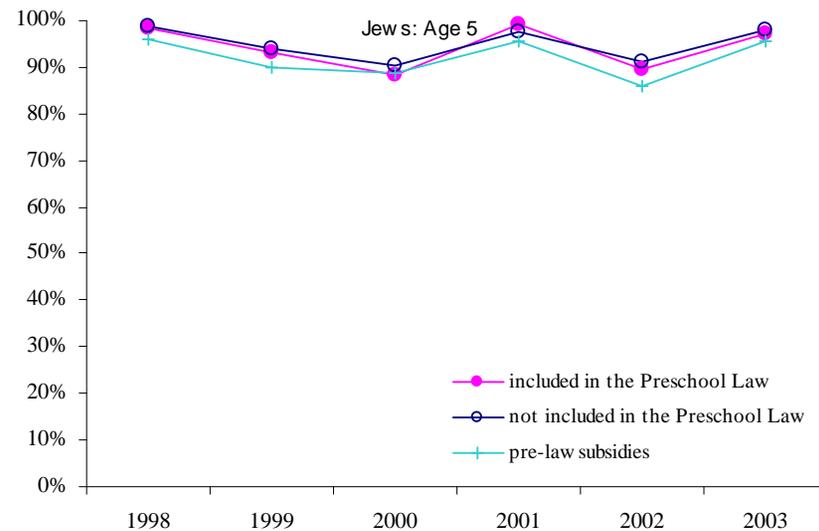
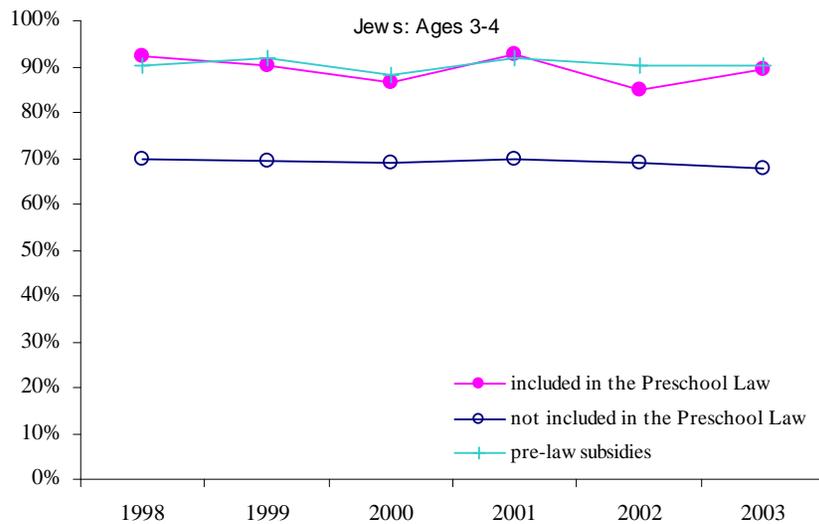
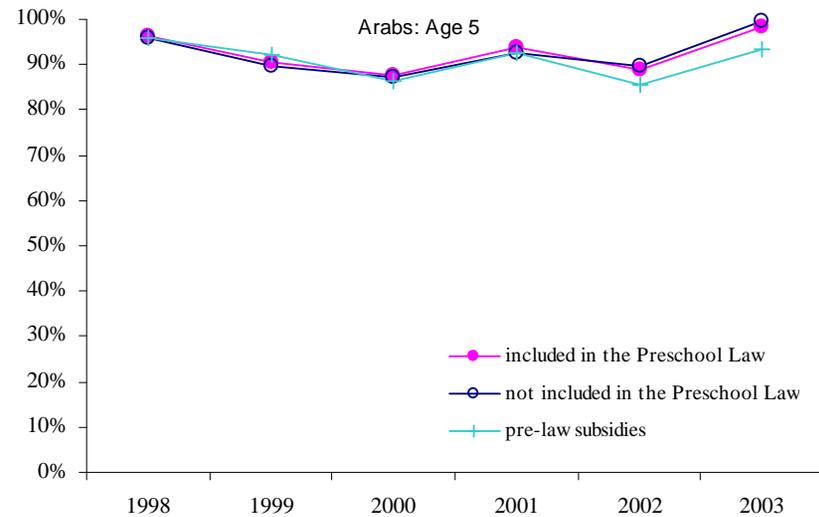
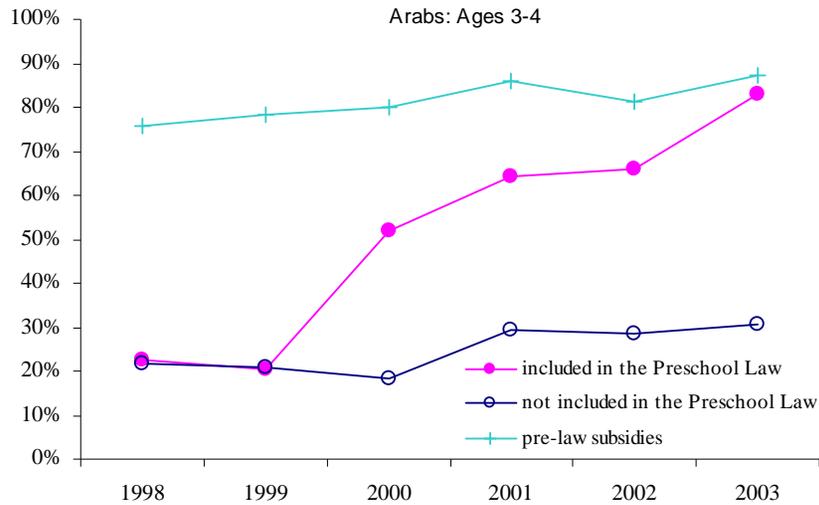


Figure 3: Enrollment rates in public preschools and kindergartens by year and type of public financial support.

Notes: The label "included in the Preschool Law" refers to towns that received free preschool services due to the Preschool Law; "not included in the Preschool Law" refers to towns not included in the law mandate; "pre-law subsidies" refers to towns that received subsidies of 80-90 percent before the Preschool Law. Towns that received subsidies in some neighborhoods are not included in the figures.

Source: Author's calculations from raw data on population and enrollment by year, age and town of residence provided by the Israeli Central Bureau of Statistics.

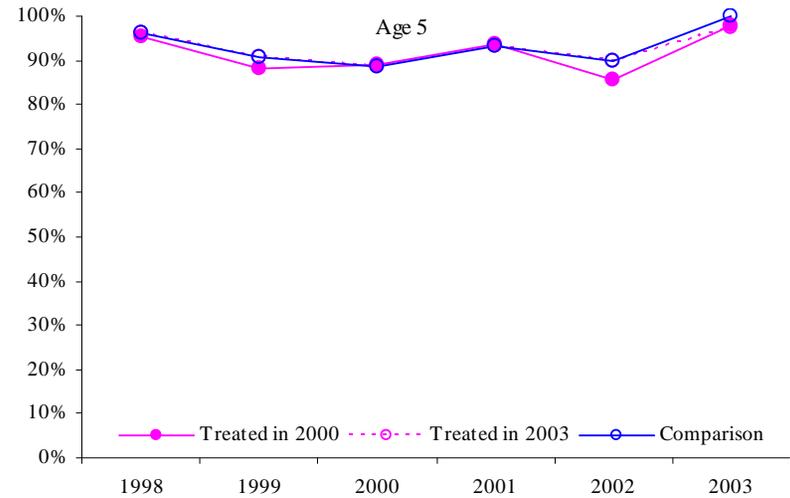
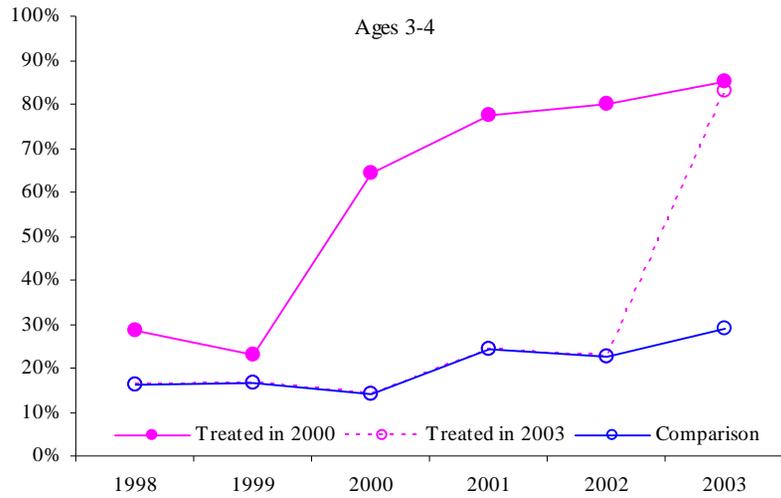


Figure 4: Enrollment rates in public preschools and kindergartens by year in treated and comparison towns.

Source: Author's calculations from raw data on population and enrollment by year, age and town of residence provided by the Central Bureau of Statistics.

Table 1. Preschool Provision for Ages 3 and 4 in Year 2003

	Towns		Children Aged 3-4	
	Number	Percentages	Number	Percentages
A. Jewish Sector				
Had subsidies before the Preschool Law	374	40%	29,088	15%
First provided with free preschool after the Preschool Law	118	13%	11,373	6%
Preschool Law not yet implemented	447	48%	148,024	79%
Total	939	100%	188,485	100%
B. Arab Sector				
Had subsidies before the Preschool Law	27	23%	8,215	11%
First provided with free preschool after the law	42	35%	24,078	33%
Preschool Law not yet implemented	51	43%	40,741	56%
Total	120	100%	73,034	100%

Notes: The population data is based on estimates by religion, age and town of residence for the year 2003 provided by the Israeli Central Bureau of Statistics. Towns that receive preschool subsidies in some neighborhoods are classified as town where the Preschool Law has not yet been implemented. The population living in mixed towns is presented separately according to their ethnicity.

Table 2. Socio-Economic Indicators in Treated and Comparison Towns

	Treated	Comparison
<i>1999 Indicators</i>		
Socio-Economic Cluster	1.71	3.04
Socio-Economic Ranking	23.3	65.6
Population	15,007	15,407
Dependency Ratio	141	108
Income per Capita (NIS)	1,224	1,401
% of Income Support Recipients	3.97	2.24
<i>2001 Indicators</i>		
Socioeconomic Cluster	1.71	3.03
Socio-Economic Ranking	21.4	58.8
Population	16,173	16,421
Dependency Ratio	141	108
Income per Capita (NIS)	1,330	1,548
% of Income Support Recipients	4.96	4.39
Number of towns	11	13

Note: The table reports selected socio-economic indicators in treated and comparison towns based on towns' indicators published in CBS (2003) and CBS (2004).

Table 3. Descriptive Statistics for Mothers of Children Aged 2-4

	Pre-law period				Post-law period			
	treated (1)	comparison (2)	difference (3)	controlled difference (4)	treated (5)	comparison (6)	difference (7)	controlled difference (8)
Age	31.36	30.76	0.601 (0.670)	-	30.30	31.11	-0.805 (0.480)	-
Years of schooling	7.842	10.393	-2.551 (1.015)	-	9.124	10.955	-1.832 (0.820)	-
Completed high school	0.266	0.481	-0.215 (0.071)	-	0.386	0.573	-0.187 (0.067)	-
Married	0.967	0.977	-0.010 (0.016)	-	0.980	0.987	-0.008 (0.008)	-
Number of children	4.259	3.202	1.057 (0.350)	-	4.036	3.318	0.717 (0.355)	-
Druze head of household	0.000	0.019	-0.019 (0.017)	-	0.000	0.036	-0.036 (0.033)	-
Christian head of household	0.038	0.056	-0.018 (0.041)	-	0.029	0.057	-0.028 (0.031)	-
Muslim head of household	0.962	0.925	0.037 (0.056)	-	0.970	0.904	0.065 (0.060)	-
Labor force participation	0.060	0.161	-0.101 (0.050)	-0.022 (0.031)	0.171	0.183	-0.012 (0.034)	0.098 (0.019)
Studies or works	0.059	0.161	-0.102 (0.050)	-0.021 (0.030)	0.172	0.189	-0.017 (0.035)	0.099 (0.018)
Worked last week	0.057	0.158	-0.101 (0.050)	-0.021 (0.030)	0.165	0.173	-0.007 (0.034)	0.102 (0.018)
Hours worked	1.462	4.810	-3.348 (1.587)	-1.243 (0.959)	4.449	4.454	-0.006 (0.957)	2.635 (0.576)
Number of towns	11	13	-	-	11	13	-	-
Number of observations	524	396			1,371	1,040		

Notes: The table reports descriptive statistics and balancing tests for the sample of mothers of children aged 2-4 residing in treated and comparison towns. The pre-law period is from January 1998 to September 1999. The post-law period is from October 1999 to December 2003. Columns 3 and 4 present simple and controlled differences between column 1 and column 2. Columns 7 and 8 presents simple and controlled differences between column 5 and column 6. Controlled differences include controls for age, age squared, years of schooling, marital status, a set of dummy variables for the religion of head of household, and a set of variables indicating the number of children under the following age groups: 5-9, 10-14 and 15-17. Standard errors for the differences adjusted for clustering at the town level are reported in parenthesis. Data is weighted using sampling weights.

Table 4. Difference-in-Differences Estimates of the Effect of Free Preschool Provision on Female Labor Force Participation

	Mothers of children aged 2-4			Mothers whose youngest child is 2-4 years old		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>A. Mean effect in post-law period</i>	0.115 (0.031)	0.090 (0.044)	0.071 (0.034)	0.148 (0.031)	0.126 (0.050)	0.117 (0.035)
<i>B. Free preschool provided for</i>						
one year	0.111 (0.033)	0.086 (0.040)	0.076 (0.033)	0.072 (0.050)	0.077 (0.062)	0.052 (0.048)
two years	0.065 (0.050)	0.054 (0.056)	0.022 (0.047)	0.077 (0.060)	0.072 (0.073)	0.052 (0.059)
three years	0.094 (0.045)	0.061 (0.060)	0.044 (0.048)	0.197 (0.040)	0.139 (0.061)	0.159 (0.052)
four years	0.169 (0.046)	0.147 (0.068)	0.128 (0.057)	0.216 (0.048)	0.206 (0.079)	0.198 (0.057)
Control variables	✓	×	✓	✓	×	✓
Town fixed effects	×	✓	✓	×	✓	✓
Time fixed effects	✓	✓	✓	✓	✓	✓
Number of observations	3,331	3,331	3,331	1,905	1,905	1,905
Number of towns	24	24	24	24	24	24

Notes: The table reports difference-in-differences estimates. The dependent variable is a binary variable indicating whether the mother participates in the labor force. Where indicated, a set of control variables indicated in Table 3 are included in the models. Data is weighted using sampling weights. Standard errors are reported in parenthesis and are adjusted for clustering at the town level.

Table 5. Difference-in-Differences Estimates of the Effect of Free Preschool Provision on Alternative Labor Supply Outcomes

	Mothers of children aged 2-4			Mothers whose youngest child is 2-4 years old		
	studies or works (1)	worked last week (2)	hours worked (3)	studies or works (4)	worked last week (5)	hours worked (6)
<i>A. Mean effect in post-law period</i>	0.069 (0.037)	0.081 (0.035)	2.831 (1.201)	0.106 (0.038)	0.126 (0.035)	3.885 (1.253)
<i>B. Free preschool provided for</i>						
one year	0.070 (0.033)	0.081 (0.036)	1.865 (0.912)	0.043 (0.050)	0.053 (0.050)	1.677 (1.351)
two years	0.038 (0.046)	0.039 (0.044)	1.189 (1.588)	0.076 (0.058)	0.074 (0.056)	2.287 (1.865)
three years	0.058 (0.051)	0.066 (0.050)	3.897 (1.922)	0.156 (0.049)	0.179 (0.050)	6.511 (1.795)
four years	0.104 (0.063)	0.127 (0.057)	4.181 (1.944)	0.152 (0.062)	0.197 (0.056)	5.245 (1.998)
Number of observations	3,331	3,331	3,331	1,905	1,905	1,905
Number of towns	24	24	24	24	24	24

Notes: The table present difference-in-differences estimates for alternative measures of female labor supply. The regression estimates are from models that include town and time fixed effects and control for mother's characteristics specified in Table 3. Data is weighted using sampling weights. Standard errors are reported in parenthesis and are adjusted for clustering at the town level.

Table 6. Difference-in-Differences Estimates of the Effect of Free Preschool Provision on Female Labor Supply Mothers with no children Aged 2-4

	labor force participation			studies or works	worked last week	hours worked
	(1)	(2)	(3)	(4)	(5)	(6)
<i>A. Mean effect in post-law period</i>	0.031 (0.029)	0.007 (0.037)	0.005 (0.037)	0.000 (0.038)	0.001 (0.038)	0.729 (1.130)
<i>B. Free preschool provided for</i>						
one year	0.041 (0.033)	0.032 (0.039)	0.014 (0.032)	-0.007 (0.033)	0.004 (0.032)	0.201 (1.040)
two years	0.017 (0.036)	0.003 (0.046)	-0.016 (0.040)	-0.005 (0.038)	-0.009 (0.035)	1.057 (1.495)
three years	0.035 (0.039)	0.006 (0.050)	0.008 (0.047)	0.009 (0.054)	0.005 (0.050)	1.318 (1.768)
four years	0.032 (0.063)	-0.014 (0.069)	0.011 (0.074)	0.004 (0.070)	0.004 (0.075)	0.508 (2.131)
Control variables	✓	×	✓	✓	✓	✓
Town fixed effects	×	✓	✓	✓	✓	✓
Time fixed effects	✓	✓	✓	✓	✓	✓

Notes: The table reports difference-in-differences estimates on alternative measures of female labor supply for a sample of 3,949 mothers residing in treated and comparison towns with no children aged 2-4. The regression estimates are from models that include town and time fixed effects and control for mother's characteristics specified in Table 3. Data is weighted using sampling weights. Standard errors are reported in parenthesis and are adjusted for clustering at the town level.

Table 7. Triple Difference Estimates of the Effects of Free Pre-School Provision on Female Labor Supply

	All mothers				Mothers whose youngest child is aged 2-4 and all mothers not having children aged 2-4			
	labor force participation (1)	studies or works (2)	worked last week (3)	hours worked (4)	labor force participation (5)	studies or works (6)	worked last week (7)	hours worked (8)
A. Mean effect in Post-Law Period								
treated x post-law x (child aged 2-4)	0.082 (0.034)	0.100 (0.035)	0.097 (0.036)	2.525 (1.091)	0.118 (0.034)	0.124 (0.035)	0.129 (0.036)	3.449 (1.138)
treated x post-law	0.003 (0.031)	-0.011 (0.031)	-0.003 (0.032)	0.745 (0.914)	0.009 (0.032)	-0.003 (0.032)	0.004 (0.033)	0.851 (0.962)
B. Free preschool provided for								
one year x (child aged 2-4)	0.069 (0.042)	0.099 (0.041)	0.085 (0.043)	1.763 (1.274)	0.040 (0.056)	0.065 (0.056)	0.049 (0.055)	1.544 (1.609)
two years x (child aged 2-4)	0.048 (0.056)	0.070 (0.053)	0.063 (0.052)	0.442 (1.947)	0.065 (0.066)	0.091 (0.062)	0.080 (0.062)	1.377 (2.225)
three years x (child aged 2-4)	0.059 (0.052)	0.088 (0.058)	0.085 (0.055)	3.145 (2.030)	0.154 (0.050)	0.166 (0.053)	0.177 (0.051)	5.433 (1.906)
four years x (child aged 2-4)	0.131 (0.074)	0.131 (0.073)	0.138 (0.073)	3.980 (2.268)	0.181 (0.076)	0.151 (0.075)	0.183 (0.076)	4.800 (2.399)
one year	0.016 (0.030)	-0.013 (0.032)	0.004 (0.030)	0.344 (0.958)	0.022 (0.031)	-0.006 (0.032)	0.011 (0.030)	0.411 (0.971)
two years	-0.011 (0.036)	-0.011 (0.033)	-0.009 (0.030)	1.310 (1.343)	-0.007 (0.037)	-0.005 (0.034)	-0.003 (0.031)	1.322 (1.377)
three years	0.003 (0.040)	-0.006 (0.047)	-0.002 (0.042)	1.279 (1.542)	0.011 (0.041)	0.005 (0.048)	0.008 (0.044)	1.435 (1.585)
four years	0.006 (0.067)	-0.010 (0.063)	-0.003 (0.067)	0.462 (1.932)	0.015 (0.068)	0.002 (0.064)	0.008 (0.068)	0.637 (1.967)

Notes: The table reports triple difference-in-differences estimates for female labor supply. The regression estimates are from models that include town and time fixed effects and control for mother's characteristics specified in Table 3. In addition, the regressions control for a differential effect of having a child aged 2-4 in treated and comparison towns and allow for a specific time trend for mothers with children aged 2-4. Estimates in cols. 1-4 are based on a sample that includes all mothers. Estimates in cols. 5-8 are based on a sample that includes mothers whose youngest child is 2-4 years old and mothers who do not have children of that age. Data is weighted using sampling weights. Standard errors are reported in parenthesis and are adjusted for clustering at the child's age-town level.

Table 8. Difference-in-Differences Estimates of the Effect of Free Preschool Provision on Female Labor Force Participation: Treated Towns Only

	treatment effects for mothers of children aged 2-4		treatment effects for mothers whose youngest child is aged 2-4	
	all mothers	all women	all mothers	all women
	(1)	(2)	(3)	(4)
<i>A. Mean effect in post-law period</i>				
post-law x (child aged 2-4)	0.076 (0.027)	0.078 (0.023)	0.093 (0.029)	0.091 (0.024)
child aged 2-4	-0.072 (0.018)	-0.075 (0.019)	-0.069 (0.019)	-0.076 (0.020)
no children	-	0.057 (0.028)	-	0.050 (0.028)
<i>B. Free preschool provided for</i>				
one year x (child aged 2-4)	0.055 (0.028)	0.050 (0.026)	0.042 (0.039)	0.037 (0.038)
two years x (child aged 2-4)	0.077 (0.037)	0.070 (0.032)	0.078 (0.043)	0.064 (0.038)
three years x (child aged 2-4)	0.059 (0.043)	0.051 (0.038)	0.114 (0.045)	0.098 (0.040)
four years x (child aged 2-4)	0.104 (0.053)	0.123 (0.041)	0.127 (0.057)	0.146 (0.043)
child aged 2-4	-0.072 (0.018)	-0.075 (0.019)	-0.069 (0.019)	-0.076 (0.020)
no children	-	0.056 (0.028)	-	0.051 (0.028)
N	4,256	6,373	3,232	5,349

Notes: The table reports difference-in-differences estimates for female labor force participation using a sample that includes only treated towns. The regression estimates are from models that include town and time fixed effects and control for mother's characteristics specified in Table 3. The comparison group used in the estimations reported in columns 1 and 3 includes all mothers with no children aged 2-4. The comparison group used in the estimations reported in columns 2 and 4 includes all women with no children aged 2-4 (including also childless women). The treated group for columns 1 and 2 includes all mothers of children aged 2-4. The treated group for columns 3 and 4 includes all mothers whose youngest child is aged 2-4. Standard errors are reported in parenthesis and are adjusted for clustering at the child's age-town level.

Table 9. Heterogeneous Effects of Free Preschool Provision

	Mothers of children aged 2-4				Mothers with no children aged 2-4			
	labor force participation (1)	studies or works (2)	worked last week (3)	hours worked (4)	labor force participation (5)	studies or works (6)	worked last week (7)	hours worked (8)
A. Results by women's education								
Years of schooling < 12	0.022 (0.023)	0.036 (0.022)	0.033 (0.020)	0.874 (0.640)	0.025 (0.026)	0.029 (0.024)	0.027 (0.025)	1.250 (0.850)
<i>Outcome's mean in the post-law period</i>	<i>0.042</i>	<i>0.043</i>	<i>0.041</i>	<i>1.011</i>	<i>0.064</i>	<i>0.056</i>	<i>0.056</i>	<i>1.992</i>
Years of schooling ≥ 12	0.209 (0.061)	0.202 (0.065)	0.221 (0.064)	7.081 (2.309)	0.013 (0.083)	-0.010 (0.098)	-0.003 (0.090)	1.363 (2.943)
<i>Outcome's mean in the post-law period</i>	<i>0.379</i>	<i>0.381</i>	<i>0.368</i>	<i>10.079</i>	<i>0.394</i>	<i>0.400</i>	<i>0.358</i>	<i>10.580</i>
B. Results by women's education and family size								
Main treatment effect	0.018 (0.034)	0.025 (0.034)	0.034 (0.033)	1.354 (1.222)	0.026 (0.039)	0.024 (0.039)	0.029 (0.039)	1.226 (1.374)
<i>Additive effect for</i> years of schooling ≥ 12	0.147 (0.041)	0.141 (0.040)	0.151 (0.044)	4.271 (1.451)	0.008 (0.062)	-0.002 (0.079)	-0.003 (0.072)	0.734 (2.377)
number of children ≤ 3	0.052 (0.048)	0.042 (0.045)	0.039 (0.044)	1.158 (1.479)	-0.023 (0.030)	-0.025 (0.029)	-0.029 (0.029)	-0.642 (1.088)
C. Results by wife's and husband's education (sample of married women)								
Main treatment effect	0.027 (0.027)	0.029 (0.028)	0.035 (0.028)	1.309 (0.908)	0.033 (0.027)	0.033 (0.027)	0.032 (0.027)	1.575 (0.948)
<i>Additive effect for</i> wife's years of schooling ≥ 12	0.175 (0.050)	0.165 (0.049)	0.172 (0.053)	4.765 (1.659)	0.039 (0.050)	0.026 (0.067)	0.017 (0.059)	1.713 (2.196)
husband's years of schooling ≥ 12	-0.009 (0.037)	-0.006 (0.035)	-0.004 (0.036)	-0.493 (0.883)	-0.008 (0.045)	-0.016 (0.044)	0.005 (0.050)	0.103 (1.646)

Notes: The table reports differential treatment effects according to mother's education. The regression estimates are from models that include town and time fixed effects and control for mother's characteristics specified in Table 3. Data is weighted using sampling weights. Standard errors are reported in parenthesis and are adjusted for clustering at the town level.

Table 10. Difference-in-Differences Estimates of the Effect of Free Preschool Provision on the Probability of Childbearing

	All mothers		Unskilled Mothers (< 12 years of schooling)		Skilled Mothers (≥ 12 years of schooling)	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Mean effect in post-law period	-0.001 (0.058)	-0.005 (0.067)	-0.008 (0.067)	-0.008 (0.065)	0.000 (0.096)	-0.005 (0.112)
B. Free preschool provided for						
one year	-0.057 (0.059)	-0.068 (0.067)	-0.002 (0.098)	0.003 (0.096)	-0.095 (0.097)	-0.125 (0.105)
two years	0.019 (0.066)	0.017 (0.075)	-0.071 (0.090)	-0.075 (0.089)	0.045 (0.107)	0.056 (0.126)
three years	-0.001 (0.078)	-0.005 (0.090)	0.042 (0.086)	0.042 (0.085)	0.008 (0.120)	0.000 (0.137)
four years	0.038 (0.063)	0.042 (0.074)	0.002 (0.088)	0.000 (0.085)	0.057 (0.117)	0.060 (0.137)
Model	<i>Linear probability</i>	<i>Probit</i>	<i>Linear probability</i>	<i>Probit</i>	<i>Linear probability</i>	<i>Probit</i>
Outcome's mean	0.425	0.425	0.412	0.412	0.439	0.439
Number of observations	3,331	3,331	1,969	1,969	1,362	1,362
Number of towns	24	24	24	24	24	24

Notes: The table reports difference-in-differences estimates for the probability of having a child aged 0-1. The sample of columns 1 and 2 includes all mothers of children aged 2-4. The sample of columns 3 and 4 includes mothers of children aged 2-4 with less than 12 years of schooling. The sample of columns 5 and 6 includes mothers of children aged 2-4 with 12 or more years of schooling. Two estimates are reported for each sample: one for a linear probability model and one for the marginal effects of a probit model evaluated at the sample means. Regression estimates are from models that include town and time fixed effects and control for mother's characteristics specified in Table 3. Data is weighted using sampling weights. Standard errors are reported in parenthesis and are adjusted for clustering at the town level.

Table A1. Probit Difference-in-differences Estimates of the Effects of Free Preschool Provision on Female Labor Supply

	Mothers of children aged 2-4			Mothers whose youngest child is 2-4 years old		
	labor force participation (1)	studies or works (2)	worked last week (3)	labor force participation (5)	studies or works (6)	worked last week (7)
<i>A. Mean effect in Post-Law Period</i>	0.083 (0.036)	0.080 (0.038)	0.087 (0.035)	0.129 (0.050)	0.125 (0.060)	0.144 (0.056)
<i>B. Free preschool provided for</i>						
one year	0.084 (0.049)	0.070 (0.049)	0.083 (0.052)	0.051 (0.056)	0.038 (0.059)	0.050 (0.057)
two years	0.052 (0.050)	0.080 (0.064)	0.077 (0.057)	0.078 (0.069)	0.136 (0.103)	0.131 (0.090)
three years	0.086 (0.058)	0.104 (0.065)	0.110 (0.064)	0.221 (0.098)	0.229 (0.097)	0.276 (0.108)
four years	0.208 (0.098)	0.167 (0.098)	0.208 (0.097)	0.332 (0.119)	0.259 (0.130)	0.351 (0.130)
Number of observations	3,331	3,331	3,331	1,905	1,905	1,905
Number of towns	24	24	24	24	24	24

Notes: The table reports difference-in-differences estimates for female labor supply based on a probit model. The estimates shown are marginal effects evaluated at the sample means. The regression estimates are from models that include town and time fixed effects and control for mother's characteristics specified in Table 3. Data is weighted using sampling weights. Standard errors are reported in parenthesis and are adjusted for clustering at the town level.

Table A2. Differential Effect of Free Preschool Provision on Female Labor Supply by Mother's Age

Mother's Age	Mothers of children aged 2-4				Mothers with no children aged 2-4			
	labor force participation (1)	studies or works (2)	worked last week (3)	hours worked (4)	labor force participation (5)	studies or works (6)	worked last week (7)	hours worked (8)
Age ≤ 30	0.069 (0.038)	0.065 (0.042)	0.079 (0.039)	2.824 (1.430)	-0.019 (0.057)	-0.054 (0.067)	-0.036 (0.059)	-0.991 (1.981)
Age > 30	0.065 (0.048)	0.074 (0.050)	0.078 (0.050)	2.447 (1.876)	0.018 (0.033)	0.026 (0.034)	0.020 (0.033)	1.724 (0.893)

Notes: The table reports differential treatment effects according to two age groups: those aged 30 or less and those aged more than 30. The regression estimates are from models that include town and time fixed effects and control for mother's characteristics specified in Table 3. In addition, the models include a main effect for each age group in treated and comparison towns, and allow the two age groups to have different time trends. Data is weighted using sampling weights. Standard errors are reported in parenthesis and are adjusted for clustering at the age group-town level.

Table A3. Robustness Checks

	labor force participation (1)	studies or works (2)	worked last week (3)	hours worked (4)
1 Effects on Mothers of children aged 0-1 or aged 5-9 (with no children aged 2-4)	0.021 (0.051)	0.015 (0.050)	0.014 (0.051)	0.913 (1.494)
2 Treatment in 1999: mothers with children aged 2-4	0.041 (0.050)	0.049 (0.048)	0.046 (0.050)	1.684 (1.847)
3 Treatment in 1999: mothers with no children aged 2-4	-0.044 (0.039)	-0.042 (0.037)	-0.047 (0.037)	-0.944 (1.265)
4 Differential treatment effect by town SES ranking for mothers with children aged 2-4				
Main treatment effect	0.105 (0.037)	0.104 (0.040)	0.114 (0.038)	3.647 (1.298)
Main treatment effect x town SES ranking	0.060 (0.027)	0.061 (0.028)	0.058 (0.026)	1.434 (0.739)
p-value of joint test for significance of coeff.	0.025	0.032	0.017	0.029
5 Differential treatment effect by town SES ranking for mothers with no children aged 2-4				
Main treatment effect	0.006 (0.051)	0.002 (0.050)	0.003 (0.051)	0.931 (1.518)
Main treatment effect x town SES ranking	0.001 (0.047)	0.002 (0.042)	0.004 (0.044)	0.428 (1.427)
p-value of joint test for significance of coeff.	0.989	0.998	0.995	0.807
6 Effects on mothers of children aged 2-4 controlling also for interaction between town SES and time trend	0.092 (0.034)	0.105 (0.035)	0.106 (0.033)	3.026 (1.108)
7 Effects on mothers with no children aged 2-4 controlling also for interaction between town SES and time trend	-0.008 (0.046)	-0.015 (0.049)	-0.007 (0.047)	0.321 (1.433)

Notes: The table reports results for various robustness checks. See Section V.B for a detailed explanation of each test.

**Table A4. Treatment Effects on the Likelihood of Employment
in the Education Sector**

A. Main Economic Branches of Arab Mothers' Employment before the Implementation of the Preschool Law	
Education	39.8
Health services	20.7
Retail sale	9.1
Welfare and social work	6.7

B. Impact of preschool provision on the likelihood of employment in the education sector	
Mothers of children aged 2-4	0.042 (0.026)
Mothers with no children aged 2-4	-0.017 (0.027)

Notes: Panel A reports the major sectors of employment of Arab mothers during the years 1998-1999. Panel B reports difference-in-differences estimates for the probability of employment in the education sector. The regression estimates are from models that include town and time fixed effects and control for mother's characteristics specified in Table 3. Data is weighted using sampling weights. Standard errors are reported in parenthesis and are adjusted for clustering at the age group-town level.

**Table A5. Descriptive Statistics of Covariates and Outcomes of Treated and Comparison Groups
Mothers of Children Aged 2-4 - Excluding Bedouin Towns**

	Pre-law period			Post-law period		
	treated (1)	comparison (2)	difference (3)	treated (4)	comparison (5)	difference (6)
Age	30.91	30.76	0.151 (0.671)	30.12	31.11	-0.989 (0.506)
Years of schooling	9.663	10.393	-0.730 (0.406)	10.582	10.955	-0.373 (0.334)
Completed high school	0.367	0.481	-0.113 (0.056)	0.476	0.573	-0.097 (0.056)
Married	0.965	0.977	-0.013 (0.018)	0.985	0.987	-0.002 (0.008)
Number of children	3.643	3.202	0.441 (0.182)	3.350	3.318	0.032 (0.181)
Druze head of household	0.000	0.019	-0.019 (0.017)	0.000	0.036	-0.036 (0.033)
Christian head of household	0.058	0.056	0.002 (0.045)	0.045	0.057	-0.012 (0.033)
Muslim head of household	0.942	0.925	0.017 (0.059)	0.954	0.904	0.049 (0.061)
Labor force participation	0.090	0.161	-0.070 (0.050)	0.220	0.183	0.037 (0.027)
Studies or works	0.090	0.161	-0.071 (0.050)	0.225	0.189	0.036 (0.028)
Worked last week	0.086	0.158	-0.072 (0.050)	0.216	0.173	0.043 (0.027)
Hours worked	2.193	4.810	-2.616 (1.595)	5.884	4.454	1.430 (0.725)
Number of towns	8	13	-	8	13	-
Number of observations	285	396		729	1,040	

Notes: The table reports descriptive statistics and balancing tests for a sample of mothers of children aged 2-4 residing in treated and comparison towns excluding Bedouin towns. The pre-law period is from January 1998 to September 1999. The post-law period is from October 1999 to December 2003. Column 3 presents the difference between column 1 and column 2. Column 6 presents the difference between column 4 and column 5. Standard errors for the differences corrected for clustering at the town level are reported in parenthesis. Data is weighted using sampling weights.

**Table A6. Difference-in-Differences Estimates of the Effect of Free Preschool Provision on Female Labor Supply
Full Sample Excluding Bedouin Towns**

	Mothers of children aged 2-4				Mothers with no children aged 2-4			
	labor force participation (1)	studies or works (2)	worked last week (3)	hours worked (4)	labor force participation (5)	studies or works (6)	worked last week (7)	hours worked (8)
<i>A. Mean effect in post-law period</i>	0.097 (0.034)	0.098 (0.037)	0.109 (0.034)	3.675 (1.197)	0.010 (0.044)	0.005 (0.046)	0.005 (0.046)	1.117 (1.260)
<i>B. Free preschool provided for</i>								
one year	0.075 (0.039)	0.066 (0.040)	0.079 (0.042)	1.608 (1.127)	0.017 (0.036)	-0.013 (0.038)	0.002 (0.036)	0.272 (1.130)
two years	0.044 (0.055)	0.058 (0.056)	0.060 (0.052)	1.948 (1.876)	-0.016 (0.050)	-0.003 (0.044)	-0.011 (0.041)	1.530 (1.764)
three years	0.085 (0.054)	0.105 (0.055)	0.109 (0.055)	5.512 (2.240)	0.013 (0.055)	0.018 (0.060)	0.010 (0.059)	1.771 (1.971)
four years	0.173 (0.059)	0.158 (0.064)	0.178 (0.056)	5.769 (1.897)	0.019 (0.093)	0.015 (0.088)	0.016 (0.093)	1.072 (2.592)
Number of observations	2,450	2,450	2,450	2,450	3,317	3,317	3,317	3,317
Number of towns	21	21	21	21	21	21	21	21

Notes: The table reports difference-in-differences estimates for alternative outcomes of female labor supply on a sample that excludes Bedouin towns. The regression estimates are from models that include town and time fixed effects and control for mother's characteristics specified in Table 3. Standard errors are reported in parenthesis and are adjusted for clustering at the town level.