The Long-Term Impact of Extended Military Service on Women's Employment, Education, and Fertility

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Abstract

Military service has been found to have significant consequences for conscripts' employment, education, and other life outcomes, as it affects young people in a crucial phase of their lives. Previous research has extensively examined the impact of military service on men. However, we know little about how it may affect women. Exploiting an exogenous change in the length of women's mandatory service in Israel as a natural experiment, I estimate the long-term effect of extended military service on women's education, fertility, and labor market outcomes. At the time of the policy change, women were excluded from combat duty and fulfilled roles that resembled employment in the civilian labor market. In addition, the service required women to temporarily delay education and motherhood, focusing solely on their professional duties while serving. Thus, studying the implications of mandatory service for women offers insights into a policy that provides a full-time employment-like experience at a young age. Based on data from the 1962 and 1971 censuses, I find that each additional year of service leads to a persistent and positive impact on women's labor force participation, increasing it by 18% at age 27 and 12% at age 38. Furthermore, serving longer increases women's probability of attending higher education in the short run, but this effect vanishes over time. Finally, longer service induces women to postpone motherhood without altering their total fertility.

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1 Introduction

Many countries, such as Austria, Denmark, Greece, and Switzerland, rely on mandatory military service to ensure the nation's ability to defend itself against external threats. Mandatory service usually affects individuals early in life, typically between 18 and 25, and has long-lasting consequences on education and labor force outcomes. These consequences may be positive or negative. On the one hand, mandatory service disrupts the human capital accumulation process at a stage of life in which people usually invest in their skills and education. On the other hand, conscripts may benefit from their service by gaining new skills through training or by building networks. Existing research in this area often points towards a negative impact on earnings and employment in the short run (e.g., Angrist, 1990; Imbens and Klaauw, 1995; Bingley et al., 2020). However, these studies primarily focus on male conscripts, and little is known about how mandatory military service affects women.

There are several reasons to believe that the effect of mandatory service would be different for women. First, men and women often perform different duties in the military, with women frequently assigned to roles that resemble jobs in the civilian workforce, such as nurses and secretaries. Second, women face different job-family and education-family trade-offs. Mandatory military service temporarily delays women's motherhood and education while they serve, which impacts these trade-offs.[1] As a result, the policy change creates an exogenous shock that compels women to focus on professional duties while postponing motherhood and education, effectively simulating an additional year of full-time civilian employment.

This study provides new evidence on the effect of the length of mandatory military service on women's labor force participation, education, and fertility. I exploit a policy change that extended the length of mandatory military service for women in Israel as a natural experiment. In 1952, the Israeli Defense Service Law was amended to require women to serve 24 months instead of 12 months. Consequently, every Israeli woman not yet drafted by September 1952 was obligated to serve the longer term. Combined with Israel's mandatory female service, this policy change enables me to identify the causal effect of an additional year of service by comparing the cohort affected by the new policy with the preceding cohort, using a regression discontinuity design. Furthermore, using data from two censuses, I examine both the short-term and the long-term consequences of extended service duration. In contrast to the short-run adverse effects of military service for men, which are well established in the literature, I show that the combination of experiences associated with a longer military service positively affects women's education, employment, and labor force participation.

Empirical studies exploring military service's effect on men's labor market outcomes primarily focus on veterans' income. In general, these studies find either a negative effect or no effect at all. In the US, the earnings of white veterans from the Vietnam era draft were 15% lower than those of non-veterans, while the earnings of non-white veterans were not affected (Angrist, 1990). In the long run, the earnings of veterans and non-veterans converged, and the service penalty disappeared by the 1990s (Angrist et al., 2011). In the Netherlands, the cost of a year of

[1] In Israel, it is possible to marry during military service. However, women who marry while serving are released from their service obligations. Despite this, sociological research indicates that women who serve tend to complete their military service before marrying, as the Israeli society views military service as an integral part of the transition to adulthood (see Stier, 1995).

service is 5% of men's annual income (Imbens and Klaauw, 1995). In Denmark, high-ability men face a lifetime earnings penalty of $23,000 for military service, with no penalty for low-ability men (Bingley et al., 2020). In Germany, there is no evidence of military service's effect on men's income and employment (Puhani and Sterrenberg, 2021; Bauer et al., 2012). For minorities and men with low education, however, some studies show a positive impact of military service on incomes (Hirsch and Mehay, 2003; Card and Cardoso, 2012; Greenberg et al., 2022).

By contrast, evidence regarding the effect of military service on employment and labor force participation remains scarce, and the findings regarding the direction of the impact are mixed. For instance, Siminski (2013) shows that Vietnam War veterans in Australia were substantially less likely to be employed. In Germany, no effect was observed on men's employment when accounting for selection bias (Bauer et al., 2009). Bingley et al. (2020) find that the Danish draft had a modest positive effect on low-ability recruits' likelihood to be employed but no effect on high-ability men.

Previous literature suggests that the effect of military service on education is context-specific and can be negative or positive. According to Cipollone and Rosolia (2007), mandatory service reduces boys' high-school graduation rates by two percentage points in Italy. In the Netherlands, compulsory military service decreased the university graduation rate by 1.5 percentage points (Hubers and Webbink, 2015). Keller et al. (2009) employ a macro approach, using a theoretical model and empirical analysis, and show that conscription has been negatively associated with post-secondary education for 22 OECD countries since the 1960s. Conversely, some studies find a positive effect of mandatory service on education. After the abolition of mandatory military service in France, the education achievements of men fell relative to that of women (Maurin and Xenogiani, 2007). In the US, the Vietnam War draft increased the college enrollment rate of young men (Card and Lemieux, 2001). However, these papers conclude that the positive impact is driven by using education as a service-avoidance tactic. Thus, the effect of mandatory service on men's educational attainment depends on the legal and social context.

This study's contribution to the literature is threefold. First, to the best of my knowledge, it is the first study to provide causal evidence on the relationship between mandatory military service and women's post-service outcomes. While the effects of military service have been extensively studied, the previous literature focuses on the impact of service on male conscripts. Second, while most studies in the military service literature primarily analyze the extensive margin, examining the presence or absence of service, this paper investigates the intensive margin by estimating the effect of the duration of service. Third, this paper provides unique insights into how early-career employment experiences shape women's long-term outcomes. The characteristics of women's military service during this period—which combined civilian-like employment with temporary delays in fertility and education—provides a setting to examine this question while reducing the identification challenges typically arising from the endogenous nature of employment decisions. This contributes to a broader literature examining how various policies affect women's labor market outcomes, including IVF treatment policies (e.g., Kroeger and La Mattina, 2017; Gershoni and Low, 2021), public sector employment policies (e.g., Gornick and Jacobs, 1998; Gomes and Kuehn, 2019), and family policies (e.g., Jones and Wilcher, 2024; Han et al., 2009).

I find that an additional year of service increased women's probability of participating in

the labor market by 6.2 percentage points at age 27 and 4.6 percentage points at age 38. This corresponds to an increase of 18% and 12% in labor force participation, respectively. Hence, the positive effect persisted over time, although its magnitude diminished. Additionally, longer service increased the probability of attaining higher education in 1962, but this effect had disappeared by 1971. Women who served longer had fewer children in 1961, but by 1972, the fertility gap had closed, indicating that women who served longer postponed motherhood beyond the additional year of service without changing their completed fertility. These results suggest that longer military service prompts women to delay family formation and to invest in their human capital directly after service by entering the labor market and attaining higher education early. This career-family choice provided them with a labor market advantage in both the short and long term.

2 Mandatory Military Service in Israel

The Israeli Defence Force (IDF) has relied on conscription since its establishment in 1948. According to the Defence Service Law of 1949, every Israeli citizen, regardless of sex, religion, or ethnicity, must enlist in the military upon reaching age 18 (The Knesset, 1949). However, some populations are exempt from service. Under Article 11 of the law, women who are either mothers, pregnant, married, or very devout (such that their religious practices forbid them from serving) are exempted. In addition, some populations are not legally exempt from service but are not called to serve under the IDF's discretion. In the 1950s, the period relevant to this study, these populations were the Jewish Ultra-Orthodox and non-Jewish citizens.

Article 6 of the 1949 Defence Service Law states that men must serve 24 months, and women must serve 12 months. The length of the service was extended as part of a 1952 law amendment, requiring men to serve 30 months and women to serve 24 months (The Knesset, 1952a).[2] In a speech delivered at the Israeli Knesset, Prime Minister Ben Gurion stated that the uncertainty caused by the 1952 Egyptian Revolution was the main reason for the extension. This concern stemmed from the fact that just three years earlier, Israeli and Egyptian armed forces were engaged in the 1948 war. Mohamed Naguib, one of the leaders of the Egyptian revolution, expressed strong anti-Israeli opinions, which increased concerns that war could break out again. However, the next military conflict between Israel and Egypt was the Sinai War in 1956, two years after the women of the cohort studied in this paper had completed their service. In addition, demographers had raised concerns about smaller birth cohorts, which would reduce military size if the duration of service remained unchanged (The Knesset, 1952b).

The duration of military service for both men and women in Israel has changed several times since 1952, although most of these adjustments were relatively minor. The next meaningful alteration occurred in 1968, following the 1967 war, when men's service was extended to 36 months (The Knesset, 1968). In 2015, there was a reduction in the length of men's service, and as of 2024, women are obligated to serve 24 months, and men are obligated to serve 32 months (The Knesset, 2015).

[2]This paper does not consider the extension of men's service because it impacted those already serving, complicating the identification of the affected cohort. Moreover, these cohorts of men may have been affected by the 1948 war, making it challenging to disentangle the impact of the policy change and the effects of the war.

Although men and women were both called to serve in the 1950s, they performed distinct duties during their service. While most men were sent to combat units if found fit, women fulfilled more civilian roles. Only 25 possible jobs were open to women in the 1950s-1960s, and they were not allowed to fulfill any combat role (Efron, 2000). As of August 1960, 46.4% of all female soldiers were administrative workers; 12% were teachers, instructors, or social workers; and 4.5% were nurses (IDF, 1961).[3] Hence, women fulfilled duties that are equivalent to occupations that are available in the labor market.

3 Methodology

To estimate the effect of a longer service, I use a weighted regression-discontinuity design (RDD) model, exploiting the amendment to the Defense Service law passed on August 31, 1952. Every woman who turned 18 after this date was obligated to serve 24 months instead of 12 months. As the amendment to the law was unexpected and exogenous to the characteristics of the individuals, we can treat the assignment to different lengths of service for women who turned 18 around September 1952 as random. Hence, under the assumption that the cohort who turned 18 in the year before September 1952 is no different from the cohort who turned 18 in the year after 1952, I can estimate the effect of the amendment of the law by comparing the two cohorts, using the following regression:

$$y_i = \alpha + \tau LONG_i + \gamma' X_i + \epsilon_i \tag{1}$$

Where $LONG_i$ is a binary variable that equals 1 if a woman turned 18 after the cutoff (and was required to serve for 24 months instead of 12) and 0 otherwise. In this context, women who turned 18 after the cutoff are defined as the treated group and are otherwise part of the control group. y_i is the outcome of interest (e.g., education, employment and fertility), and X_i is a vector of pretreatment controls (ethnicity, immigration status, and immigration age).[4]

In this framework, τ can be interpreted as the effect of an additional year of service on the outcomes of interest.[5] Note, however, that information on the actual service status of individuals is unavailable as it is classified. Moreover, the birthday information does not provide information regarding the date of enlisting, as service might be delayed for several reasons (e.g., health issues). In the analysis, I treat every individual as if they served the full service as defined by the law unless they are part of a specific group known to be exempt (for example, non-Jewish women). Hence, the effect estimated in Equation 1 is the intention to treat effect (ITT). In other words, I estimate the systematic effect of being a part of a cohort obligated to serve longer on education, fertility, and labor market outcomes.

The bandwidth used in the analysis (denoted h and coded in months) is constrained by data limitations, which restrict the flexibility of bandwidth selection. Specifically, while detailed

[3] As women did not take part in any combat duties, female military casualties were generally low, with only 51 casualties between January 1, 1950, and December 31, 1960 (Baumel-Schwartz, 2009). This suggests that the increased risk of loss of life does not drive the results of this paper.

[4] The outcome and control variables are described in detail in Section 4.

[5] This causal interpretation relies on the assumption that pretreatment variables are not correlated with the treatment assignment and are continuous around the cutoff. I test this assumption using several methods and find that the assumption is likely to hold. This is further discussed in Section 7.

birth month data is available in the 1972 census, it is banded in groups of 6 months in the 1962 census. This restricts the possible bandwidths in 1961 to intervals of 6 months. In addition, the data regarding birth month and year is discrete in both censuses, making the use of different data-driven optimal bandwidth-choice methods infeasible. Considering these limitations, I use a bandwidth of h=6 for the primary analysis, which ensures that the sample used is as close as possible to the cutoff. To verify that the results are robust to the choice of bandwidth, I also estimate the model in Equation 1 using different bandwidths. This robustness check is further discussed in Section 7.

The model described in Equation 1 does not control for the local polynomial or linear trend. The running variable (the month of birth) is discrete, and it takes only six values below and over and cutoff. This might cause bias because the running variable is rounded, and the functional form of the relationship between the outcome and the running variable is unknown (see Dong, 2015; Kolesár and Rothe, 2018). Instead, I use a locally weighted average approach, using a triangular kernel in the main analysis. Assuming that the pretreatment variables are balanced between the treatment and control groups around the cutoff, the choice of kernel should not bias the results (Mattei and Mealli, 2017). As a robustness check, I also use a parabolic and a rectangular kernel and find that the results are robust for the choice of kernel. The results of this test are discussed in Section 7. Note that the fact that the birth month is banded in the census of 1961 implies that weighting around the cutoff is impossible. To ensure that this does not bias the results, I use a triangular kernel to analyze the results of 1961 using a longer bandwidth in Section 7. All results are robust to the use of different bandwidths and kernels.

To show that the RDD model is suitable in this context, I plot the global relationship between the month of turning 18 and labor market outcomes (labor force participation and employment). Appendix Figure B.1 presents the average outcome for each birth month two years before and after the policy change (h=24), as well as the linear and polynomial fit for the census of 1972. The figure shows a discontinuity in the outcome variables around the policy change for both fits. As data regarding the birth month is banded in groups of six months in the 1961 census, there are only four data points before and after the policy change. This implies that the same exercise cannot be done for the 1961 census, as a polynomial fit is impossible. Nonetheless, the results for the linear fit in the 1961 census are presented in Appendix Figure B.2 and show discontinuity of the outcomes as well.

4 Data

To analyze the effect of an additional year of service on women's post-service outcomes, I use the 1961 and the 1972 censuses of Israel collected by the Israeli Central Bureau of Statistics (CBS). Each census covers 20% of the Israeli population at the time of the survey and contains rich demographic and socioeconomic data. This data allows me to explore the effect of extension of service on women's outcomes 7 and 18 years after their service completion.

In line with the empirical strategy of this study, the sample was restricted to women who turned 18 a year before and after the policy change of August 1952 (a bandwidth of h=12, which is the longest bandwidth used in the analyses of this paper). Since the census of 1972 contains precise information on the month and year of birth, I classify individuals who turned 18 in

August 1952 or after as the treated group (who served for 24 months), while those who turned 18 before August 1952 are classified as the non-treated group (who served for 12 months).

The census of 1961, however, only provides information regarding the month of birth in a grouped form. It only allows me to observe whether the individual was born between January 1st and May 31st or after June 1st. Consequently, for the 1961 census, I define individuals who turned 18 between June 1952 and May 1953 as the treated group and those who turned 18 between June 1951 and May 1952 as the non-treated group.[6] As a result, some women who are part of the control cohort are included in the treated group. Because the treated group is diluted by untreated individuals, this approach only introduces a downward bias to the magnitude of the effect and will provide a conservative estimate. To further test whether the results of the 1961 census represent a lower bound for the true effect size, I use the same cutoff of June 1952 in the census of 1972. By comparing the results obtained using the actual cutoff of August 1952 with those obtained using the June cutoff, I can observe whether the effect size is smaller when diluting the treatment group. The results for this test are presented in Appendix Table A.1. Indeed, the diluted results are smaller in magnitude than the results of the actual cutoff. Although the results for the June cutoff are not significantly different from zero, they are of the same sign as the results of the August cutoff.[7]

Individuals who immigrated to Israel after the age of 18 are excluded from the sample since their service status is unclear. As discussed in Section 2, pregnant and married women do not serve in the IDF. Hence, women who married before the age of 17 are also excluded from the sample.[8] Women who married at the age of service are not excluded to avoid possible selection bias. The 1952 policy change might have incentivized women to marry at a younger age to avoid the longer service. To ensure this will not bias the results, I test whether the service extension induced women to marry before 19 to get an earlier release. I find no evidence for the hypothesis that women required to serve longer used marriage to avoid service. This result is discussed further in Section 7.

I focus on four primary outcome variables to assess the effect of prolonged military service. I use two dummy variables for the labor market outcomes: the first takes the value of 1 if a woman participated in the labor market in the past week, and the second takes the value of 1 if a woman was employed in the past week, and 0 otherwise. For education, I use a variable that equals 1 if a woman ever attended a university or college and 0 otherwise. Lastly, for fertility, I use the woman's number of children.

In addition, I assess the effect of the service extension on occupational choice to test whether women who served longer were more likely to be employed in an occupation requiring high skills. To do so, I construct a dummy variable that equals 1 if a woman is employed in a high-skilled occupation after service and 0 otherwise. High-skilled occupations are defined according to the International Classification of Occupations (ISCO), corresponding to skill levels 3 and 4 (ILO, 2012). The list of occupations defined as high-skilled, as well as their Israeli census and ISCO

[6] Appendix Figure B.3 provides a visualization of the time frame and the service lengths before and after the policy change in each census.

[7] The only exception is higher education, which is of similar magnitudes and opposite sign. However, it is very small and not significantly different from zero.

[8] Although the law in Israel does not permit marriage before the age of 17 (The Knesset, 1950), some women immigrated to Israel having married at a younger age.

codes, is presented in Appendix Table A.6.

As controls, I include three pretreatment variables: immigration status, immigration age, and ethnicity. Immigration status is a binary variable that takes the value of 1 if the individual was born outside of Israel and 0 otherwise. Immigration age is coded in years, where the immigration age of Israeli-born individuals is set to 0. Ethnicity is a dummy variable, with a value of 1 indicating Mizrahi ethnicity and 0 indicating Ashkenazi ethnicity.[9]

Before excluding the populations exempt from service, the cohort includes 5,795 women in the census of 1961 and 6,793 women in the census of 1972. After limiting the sample to those required to serve, the sample consists of 3,434 observations from the census of 1961 and 3,473 from the census of 1971. Summary statistics of demographic variables of the sample are presented in Table 1. The table shows that the two samples are similar in terms of pretreatment variables.

Table 1: Descriptive statistics

Panel A: Census of 1961

	N	Mean	SD	Min	Max
Age	3382	26.98	0.69	26	28
Immigration status	3382	0.62	0.48	0	1
Mizrahi	3237	0.39	0.49	0	1
Married	3382	0.88	0.32	0	1
Age at immigration	2056	13.94	4.06	1	17
Number of children	2950	1.79	1.18	0	9
Observations	3382				

Panel B: Census of 1972

	N	Mean	SD	Min	Max
Age	3473	37.78	0.65	37	39
Immigration status	3473	0.63	0.48	0	1
Mizrahi	3314	0.40	0.49	0	1
Married	3473	0.92	0.28	0	1
Age at immigration	2140	13.93	3.82	1	17
Number of children	3257	3.10	1.75	0	16
Observations	3473				

Notes: The table reports the demographic characteristics of the samples used in this study. The samples are restricted to Jewish women who turned 18 a year before or after the change in service length of August 1952 (h=12), as explained in Section 3. The sample excludes individuals who were exempt from service, as detailed in Section 4. Note that the data is taken from the 1961 and 1972 censuses collected by the Israeli CBS, which are cross-sectional surveys.

[9]Mizrahi refers to Jewish individuals with origins in Arab and Muslim countries, while Ashkenazi refers to Jewish individuals with origins in European countries. The ethnicity variable is determined based on the paternal continent of origin. The maternal continent of origin is used if paternal information is unavailable.

5 Results

5.1 Labor Market Outcomes

I first examine the effect of an additional year of service on labor market outcomes. The results are presented in the first two columns of Table 2, with column 1 showing the effect on labor force participation and column 2 the effect on employment. Panel A presents the results for the 1961 census, and Panel B presents the results for the 1972 census. Starting with the census of 1961, the table reveals a positive and statistically significant effect of longer service on both labor force participation and employment. I find that an additional year of service increased women's probability of participating in the labor market and being employed by 6.2 and 5.9 percentage points in 1961, respectively. Given that the labor force participation rate is 34.9% and the employment rate is 33.2% among women in the control group, these results translate to increases of 17.9% and 17.7% in labor force participation and employment, respectively.

The results of the 1972 census are presented in the first two columns of panel B in Table 2. Consistent with the census of 1961, the results indicate a positive and statistically significant effect of prolonged service on women's labor force participation and employment. The effect size is 4.6 and 4.9 percentage points for the probability of participating in the labor force and being employed, respectively. To put this in context, given the labor force participation and employment rates in the control group, the effect of longer service translates to a 12.2% increase in labor force participation and a 13.3% increase in employment.[10]

Considering the findings from both censuses, we observe that a longer service positively affected women's labor market outcomes. The persistence of this effect over time and its consistency across the two variables strengthen this conclusion. However, the effect in 1972 is smaller in magnitude than that of 1962. This suggests that although longer service affects labor market participation both in the short and the long run, the effect is diminishing over time. Note that the labor force participation rate of the treated group remains steady at 42.8% across both censuses, while the control group's participation rate rises from 34.9% to 37.8%. This suggests that the diminishing effect is more likely due to the control group catching up rather than the treated group exiting the labor market.

5.2 Education

The impact of an additional year of service on the probability of ever attending a college or university is presented in Table 2 column 3. Panel A displays the result from the 1961 census, while Panel B presents the result from the 1972 census. As seen in the table, the point estimate for the effect of service is positive and statistically significant at the 1% level in the census of 1961: women who served longer are 3.2 percentage points more likely to attend university or college. Given the control group's low university/college attendance, this translates to an increase of

[10] Women's labor force participation appears to be higher in the census sample compared to that of the general female Jewish population, which is recorded at 29.4% in 1961 (Gronau, 1978) and at 32% in 1972 (Fishelson, 1972). These differences are reasonable since the sample for this study excludes women who immigrated after the age of 18 or married at a very young age; both characteristics are likely to be negatively correlated with employment status (for example, see Hermansen, 2017; Gustafsson et al., 2017; Schaafsma and Sweetman, 2001).

180.1%.[11] In addition, as column 4 shows, women who served longer were more likely to work in a high-skilled occupation. This implies that higher education attainment translated to better jobs.

However, unlike the positive effect observed for employment, the impact of a longer service on education is not persistent over time. As shown in panel B of Table 2, although the estimated coefficient is positive in the 1972 census, it is not statistically significant and smaller in magnitude. Therefore, I cannot reject the hypothesis that an additional year of service did not affect education in the long run and that the positive effect found in 1961 vanishes over time. This conclusion is also true for the probability of working in a high-skilled occupation, as the effect found in column 4 of Table 2 is very small and statistically insignificant.

The women in the sample were, on average, 27 years old in 1961 and 38 years old in 1972. The results indicate that some women in this cohort attended university or college after the age of 27. While this may initially seem late, it reflects the expansion in higher education attainment during that period. This trend of late educational attainment is observed in the censuses. Appendix Figure B.5 illustrates this trend by showing the percentage of students over age 27 among adult Jewish women who reported being current students. As the figure shows, the rate of female students over 27 was 35% in the 1961 census and 39% in the 1972 census, demonstrating that pursuing education later in life was relatively common during this period. This trend is supported by data from the Council for Higher Education in Israel, which shows that between 1964 and 1974, the percentage of individuals aged 25-29 enrolled in a university more than doubled, indicating a trend towards later university enrollment (Herskovic, 1995).

Hence, women who served an additional year were more likely to attend higher education in the short run, but this advantage disappeared in the long run. This indicates that women with longer service entered education earlier, which led them to occupations requiring more skills. However, their peers eventually caught up with them in terms of education and skills.

5.3 Fertility

We now turn to the effect of an additional year of service on fertility, defined as the number of children. The results are presented in column 5 of Table 2. Panel A of the table shows that the service extension induced women to have fewer children in 1961. As this census was conducted seven years after service, this lower fertility goes beyond a mechanical one-year delay caused by the extension of service.

At that time of the 1961 census, the sample women were, on average, 27 years old and still in their reproductive years. To study the effect on completed fertility, we move to the census of 1972 presented in Panel B of Table 2, in which women were, on average, 38 years old. In that census, the longer service has no statistically significant effect on the number of children. The fact that women who served longer had fewer children seven years after service but the same number of children eighteen years after service implies that the longer service did not affect completed fertility. However, it caused women to postpone their fertility beyond the temporary delay caused by the additional year of service.

[11] The low attendance rate of the sample reflects the generally low higher education attendance in Israel in the 1960s (Kirsch, 2014).

Table 2: The effect of one additional year of service on marriage, labor market outcomes, education and fertility

	(1) LFP	(2) Employment	(3) Higher edu.	(4) High skilled	(5) #Children
Panel A: Census of 1961					
Long	0.062***	0.059**	0.032***	0.032*	-0.245***
	(0.023)	(0.023)	(0.009)	(0.017)	(0.059)
Mean	*0.349*	*0.332*	*0.018*	*0.122*	*2.001*
Effect as % of mean	17.9%	17.7%	180.1%	26.3%	-12.3%
Observations	1,698	1,698	1,692	1,673	1,484
Panel B: Census of 1972					
Long	0.046*	0.049*	0.014	-0.006	-0.083
	(0.025)	(0.025)	(0.013)	(0.018)	(0.091)
Mean	*0.378*	*0.370*	*0.059*	*0.151*	*3.158*
Effect as % of mean	12.2%	13.3%	23.0%	-3.8%	-2.6%
Observations	1,736	1,736	1,686	1,736	1,622

Notes: This table shows the impacts of an additional year of service on women's outcomes based on Eq.(1). Sample: women who turned 18 six months before or after June 1952 (bandwidth h=6), excluding non-Jewish, those who immigrated to Israel after 18 or married before 17. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6 Impressionable Years, Networking, and Training as Possible Explanations

There are several potential explanations for the positive effects of military service on women's education and labor market outcomes. Due to data limitations and the complex nature of military service, which combines the experience of early employment and a temporary delay of family formation and education, it is impossible to pinpoint the exact mechanism driving the results. However, we can consider three possible explanations.

The first is related to the age at which women serve. As explained in Section 2, Israeli women are required to serve in the military at age 18. This age falls into the age group known in social psychology as *impressionable years*. According to the impressionable years hypothesis, the events occurring between 18 and 25 shape most of an individual's behaviors and views (Krosnick and Alwin, 1989). Social psychology literature confirms that the economic situation during the impressionable years affects political views, entrepreneurship, and labor force participation (see, Alon et al., 2001; Emmenegger et al., 2017; Sotirakopoulos et al., 2021). In particular, studies have shown that women's employment at this stage in life is associated with a shift in their gender attitudes towards more egalitarian views (Fan and Marini, 2000; Cunningham, 2008). In contrast, parenthood and marriage were found to shift women's attitudes toward a more traditional direction (Morgan and Waite, 1987; Fan and Marini, 2000). As such, the longer employment-like experience the military provides, coupled with the temporary delay in fertility until the completion of service, might shift women's attitudes in a more gender-equal direction. Consequently, this shift in attitudes can encourage these women to seek employment, invest in education, and delay motherhood.

The second possible explanation is networking. Previous studies highlight the significant role of connections in securing employment and shortening unemployment duration (for example, see Montgomery, 1991; Topa, 2001; Bramoullé and Saint-Paul, 2010). Women who serve longer in the military may have more opportunities to expand their social networks and build connections with peers and superiors. Then, as they enter the civilian labor market, these connections can be crucial in finding suitable employment. This will give them an advantage in the labor market over those with shorter service who may not have had the same networking opportunities. Thus, networking may be a key mechanism explaining the results of this study. If women expanded their social network when serving longer, they likely accessed better opportunities in education and employment after service. In turn, these better opportunities could have incentivized them to postpone fertility to benefit from their advantages.

An additional explanation to consider is that women may receive longer and better training with a longer service. To test this, I examine whether women with longer service are more likely to work in occupations related to their military training. I create three binary variables for post-service occupation. The first variable equals 1 if a woman is employed in an occupation equivalent to those available during service (for example, a teacher).[12] The second equals 1 if a woman is doing administrative jobs (the most common military role), and the third variable equals 1 if a woman is in the medical field (the military role that requires the most training). These variables take the value of 0 if a woman is employed in an unrelated occupation or is unemployed. Then, I use these variables as outcomes in the model described by Equation 1. If the channel through which the service affects education and employment is training, we would expect women to be more likely to work in the types of occupations they were trained in during service. The results of this exercise are presented in Appendix Table A.2. All of the estimated coefficients for the effect of service are very small and statistically insignificant. Thus, I find no evidence that training is a likely channel.

Since better training does not seem to be a plausible factor, the impressionable years hypothesis and networking are likely explanations. Military service occurs during a critical period when women are forming their worldviews. The employment-like experience and the enforced temporary delay in starting a family may influence women to adopt a more egalitarian gender-role attitude. In addition, military service may expand women's social network, providing them with better labor market and education opportunities after service. This shift in perspective and better options may lead women to prioritize employment, pursue early education, and further delay motherhood.

7 Robustness Checks

The previous analyses point to an overall positive effect of an additional year of service on women's labor force participation, employment and education. I use several methods and tests to assess the robustness of these results.

First, as explained in Section 3, the RDD analysis relies on the assumption that the treated and control groups are not systematically different. Thus, I examine the balance and continuity

[12] The list of occupations used to construct this variable is based on information from the archives of the IDF (IDF, 1961).

of pretreatment variables. Second, I address the potential bias arising from non-compliance among the treated group, which could pose a concern if women required to serve longer were more likely to avoid service.

Next, I use two methods based on placebo treatment for unaffected groups: members of the same cohort that were not subject to policy change (exempt women and men) and members of cohorts enlisted after the policy change (who did not experience a change in service length). These two methods share a common theoretical basis: By creating placebo treatments and estimating their effect, I can test the distinctiveness of the actual treatment effect found in this study.

Additionally, I test the sensitivity of the results to the choice of bandwidth and kernel by employing the model described in Equation 1 with different kernels and bandwidths. Finally, I use the Romano-Wolf multiple hypothesis correction to account for the multiple tests in this study.

7.1 Balance and Continuity of Pretreatment Variables

As explained in Section 3, the coefficient τ of Equation 1 can be interpreted as casual under the assumption that the cohorts are not significantly different from each other on any observable or unobservable dimensions. As such, the RD identification of this study relies on the assumption that the pretreatment variables are continuous around the cutoff. To test for this, I use a local linear fit and a quartic polynomial fit of these variables (ethnicity, immigration status, and immigration age) around the cutoff. The results for the 1972 census are presented in Appendix Figure B.4. The graph indicates no significant differences between the control and treatment groups, as all three variables are continuous around the cutoff.

Since the birth month information is grouped in the census of 1961, performing the same exercise is impossible for this cohort, as I cannot control for the linear trend near the cutoff. Instead, I use two tests to verify the balance of the pretreatment variables around the cutoff in the 1962 sample. First, I calculate the absolute standardized difference (ASD) in means of pretreatment variables across treatment groups using the following equation:

$$ASD = (\bar{X}_{control} - \bar{X}_{treated}) \Big/ \sqrt{\frac{s^2_{control} + s^2_{treated}}{2}} \tag{2}$$

Where s^2_{group} is the variance and $\bar{X}_{group}$ is the mean of covariate X in the control or treated group. A perfect balance will result with $ASD = 0$. Following Austin (2011), a covariate is considered balanced if the ASD is smaller than 0.1 in absolute value. Second, I test for balance by using the following regression model:

$$X_i = \theta + \delta Long_i + u_i \tag{3}$$

Where $Long_i$ is a dummy variable that equals 1 if individual i was subject to an extended service (and thus, treated), and 0 otherwise, X_i is the pretreatment variable of individual i. A coefficient δ that is not significantly different from 0 would indicate that the controls are balanced between the groups. As a threshold for this test, I use a p-value of 0.05. Since I run multiple tests, I apply the Bonferroni adjustment of p-values. For comparison, I also employ the

same test for the 1972 census.

The results of the ASD and the regression tests are presented in Appendix Table A.3. For all variables in both censuses, the ASD is lower than the threshold of 0.1, and the p-value of the regression tests is larger than 0.05. These results suggest that the pretreatment variables are well-balanced in the two censuses. Nonetheless, I use the three pretreatment variables as controls in the main analysis to increase efficiency.

7.2 Service Avoidence

A potential factor that could bias the results is non-compliance of the treated group driven by possible avoidance of service. The service extension might have incentivized some women to refrain from service to avoid the additional year. If this were the case, the positive effects observed might be driven by the fact that women who were required to serve longer were more likely to avoid service and enter the labor market or attain education instead. Since the service status is unknown for the sample, this hypothesis cannot be tested directly. However, data from the IDF archives reveals that 60.4% of the 1932-1933 cohorts of women required to serve were recruited, compared to 68.8% of the 1934-1935 cohorts (IDF, 1956). The first group turned 18 before the extension, while the latter group turned 18 afterward. This implies that women overall were less likely to avoid service after the policy change.

Furthermore, as an additional support that avoidance does not bias the results, I test whether the policy change influenced women to marry at a younger age. As discussed in Section 2, married women are exempt from service. Hence, the policy change might have encouraged women to get married at service age to avoid a longer service. To test for this, I use the regression model described in Equation 1 with a binary variable which equals 1 if the individual was married at age 19 or younger, and 0 otherwise, as the outcome variable. The results are presented in Appendix Table A.7. In both censuses, the point estimate for the effect of service on the probability of marrying at age 19 or younger is not statistically significant. Furthermore, the coefficients are negative, indicating that if the longer service affected marriage age, it induced later rather than earlier marriage. This result indicates that there is no evidence that women who were obligated to serve longer systematically used marriage as an avoidance tactic as a response to the policy change.

7.3 Placebo Treatment for Unaffected Cohorts

Another method employed as a robustness check involves placebo treatments for different cohorts. In essence, this method assesses the distinctiveness of the observed results by creating placebo treatments for cohorts that were not directly affected by the policy change. If the observed effects are unique compared to the placebo effects, this would provide evidence that the results observed in this study were not created merely by chance.

In practice, to conduct the test, I choose a time when there was no change in the duration of service and treat it as a placebo policy change. Subsequently, I create a sample of individuals who turned 18 half a year before and half a year after this placebo date, excluding the same populations as the study sample (i.e., men, non-Jewish, women who immigrated after the age of 18 or were married before the age of 17). The analysis is then carried out following the same

procedures described in Section 3. For example, one such point in time used as a placebo policy change is August 1955. For this placebo treatment, the sample would consist of individuals who turned 18 between March 1955 and February 1956. Following the methodology described in Section 3, I create the variable $LONG_i$, which equals 0 for women who turned 18 before the placebo policy change (i.e., August 1955), and 1 for women who turned 18 after the placebo policy change. I then estimate the effect of the placebo treatment on that cohort using the model described in Equation 1. This procedure is repeated for every possible placebo cohort in the data, creating several placebo treatments and cohorts for each census. Then, I test how many placebo treatment effects are greater in absolute value than the actual treatment effect. The number of greater placebo effects divided by the number of placebo treatments will serve as the p-value of the test.[13]

The results of this method are presented in Appendix Figures B.6 and B.7, and in Appendix Table A.8. For all outcomes and both censuses, the number of placebo treatments greater than the observed effect found in Section 5 amount to less than 5% of the placebo treatment. The only exception is the probability of being employed in a high-skilled occupation in the census of 1961, where 5 of the 40 placebo cohorts produce a higher estimated effect. Hence, the main results of this study, the positive effect on labor market outcomes and education, are considered unique.

7.4 Placebo Treatment for Unaffected Cohort Members

To further assess the robustness of the findings, I employ a placebo test with individuals of the cohort used in the main analysis who were unaffected by the policy change. I use two samples for the placebo test: Jewish women exempted from service (e.g., women who immigrated to Israel after service age or were married before age 18) and all of the Jewish men of the cohort. For these unaffected samples, I create the treatment variable $LONG_i$ as explained in Section 3, and employ the model described by Equation 1.[14] As the policy change did not affect these cohort members, a zero placebo effect will strengthen the notion that the observed effects were indeed a result of the policy change and not a result of a different event that affected the entire cohort.

The results are presented in Figures B.8 and B.9. The figures show that the placebo effects are statistically insignificant for all outcomes in the two censuses. This serves as evidence for the robustness of the main results. It indicates that it is unlikely that the effects on labor market outcomes, education, and fertility are created by an overall cohort effect but rather by the change in the length of service.

7.5 Different Bandwidths and Kernels

In the main analysis of this paper, I use a bandwidth of six months and a triangular kernel. To ensure that the choice of bandwidth or kernel does not drive the results, I employ the same

[13] Note that fewer cohorts were used as placebos in 1961 than in 1972. This is because the data regarding birth month is banded in 1961 and only available in groups of 6 months. Hence, I can create only two placebo treatments a year in the 1961 census, compared to 12 placebo treatments in the 1972 census. Overall, there are 40 placebo cohorts in 1961 and 252 in 1972.

[14] Note, however, that the results for the number of children cannot be tested for men, as the number of children is only available for women in both censuses.

model described in Equation 1 for different bandwidths and kernels. The results for different bandwidths are presented in Appendix Tables A.9 and A.10. As the birth month is only available in semesters in the 1961 census, I use h=12 as a robustness test. For the 1972 census, where information regarding birth month is in months, I use $3 \leq h \leq 12$. Most statistically significant treatment effects in the main analysis remain significant when the bandwidth is varied. In addition, the direction of the effects is consistent across bandwidths. The only two exceptions are the labor market outcomes in 1972, which are not statistically significant for h=3. The small sample size may drive these insignificant results. They are, however, of the same sign as in the main analysis.

To test whether the choice of kernel biases the results, I used two different kernels: parabolic and rectangular. The tables in the Appendix show that both kernels give results consistent with the main analysis. Hence, the results of this paper are robust to kernel choice.

7.6 Testing of Multiple Hypothesis

As the number of tests increases, so does the probability of a false rejection of a true null hypothesis. To control for this, I use the Romano-Wolf multiple hypothesis correction to adjust the p-values to multiple testing. The corrected p-values are presented in Appendix Table A.12. As revealed by the table, all of the results on the 1961 census are still significant at the 5% level, even after the correction. As for the 1972 census, the effect of the longer service on labor force participation and employment is no longer statistically significant after the correction. It is important to highlight, however, that the results of the 1972 census are robust to all of the other robustness tests conducted in this paper.

8 Conclusions

This study explores the effect of extended military service on women's labor market outcomes, education, and fertility by exploiting a policy change that exogenously extended the length of service for women in Israel. I analyze the effect of the policy change by comparing the outcomes of two consecutive cohorts: one who turned 18 before the policy change and had a shorter service and one who turned 18 later and had a longer service. The policy change, combined with the unique female conscription in Israel, allows me to estimate the effect of a longer service on women, a population rarely studied in this context. In addition, the service extension enables me to provide evidence for the effects of the duration of service rather than the effect of the presence or absence of service. As service in the 1950s resembled the labor market in terms of the roles women could fill, this paper sheds light on the consequences of a policy that provides women with an employment-like experience at a young age while temporarily postponing their fertility and education until after they complete service.

In this study, I use data from two points in time: 1962 and 1971, 7 and 18 years after service. This allows me to investigate the effect of extended military service over time and study whether it is persistent in the long run. In addition, using two data sets allows me to verify the results I find in one data set by comparing the two.

I find that an additional year of service positively affected women's labor market participation and employment in 1961 and 1972. Serving an additional year increased the probability of

participating in the labor market by 18% at age 27 and by 12% at age 28. Thus, the effect is persistent over time but decreases in magnitude. In addition, longer service positively affected women's education attainment in 1961, but no effect was found in 1972. Regarding fertility, I find that women who had a longer service had fewer children than their peers in the short run, seven years after service. However, the longer service did not affect the number of children 18 years after service.

The findings of this study suggest that women who experience a longer service change the timing of education and fertility. The fact that women who served longer tended to have fewer children in 1961 but the same number of children in 1972 indicates that a longer service did not alter the final number of children but rather the timing of motherhood. In the same way, women who served longer attended higher education earlier, but their peers caught up with them in the longer run. These two results imply that women with a longer service postponed fertility to invest in human capital accumulation. In contrast, women who experienced a shorter service invested in their human capital later and reached their peers' education level in the long run. Nevertheless, despite this "catching up", the labor force participation gap did not close. This might indicate that the advantage gained from delaying family formation and investing in education persists over time.

Although this paper exploited a policy change related to military service, the positive effect on labor market outcomes may translate to other contexts and outcomes. The finding of this paper suggests that the combination of an extended employment-like period and a forced temporary delay in family formation and education increases labor force participation. This finding may apply to other policies, such as civil service or different financial incentives that can encourage women to enter the labor market early and focus on gaining work experience before becoming mothers.

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A Tables

Table A.1: The effect of one additional year of service in the 1972 Census using June 1952 as a cutoff

	(1) LFP	(2) Employment	(3) Higher edu.	(4) High skilled	(5) #Children
Long	0.020	0.020	0.005	-0.023	-0.004
	(0.025)	(0.025)	(0.014)	(0.019)	(0.092)
Mean	*0.402*	*0.396*	*0.064*	*0.166*	*3.139*
Effect as % of mean	5.09%	5.05%	7.50%	-13.70%	-0.12%
Observations	1,700	1,700	1,652	1,700	1,596

Notes: This table shows the impacts of an additional year of service on women's outcomes based on Eq.(1), using the data restriction of the census of 1961. Sample: women who turned 18 six months before or after June 1952 (bandwidth h=6), excluding non-Jewish, those who immigrated to Israel after 18 or married before 17. Robust tandard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.2: The effect of one additional year of service on occupational choise

	(1) Service equ. occupation	(2) Medical occupation	(3) Administrative roles
Panel A: Census of 1961			
Longer service	0.019	-0.001	0.004
	(0.017)	(0.008)	(0.008)
Mean	*0.135*	*0.030*	*0.025*
Effect as % of mean	14.3%	-4.2%	17.0%
Observations	1,673	1,673	1,673
Panel B: Census of 1972			
Longer service	0.001	-0.003	0.009
	(0.019)	(0.009)	(0.011)
Mean	*0.163*	*0.029*	*0.035*
Effect as % of mean	0.4%	-11.7%	24.8%
Observations	1,736	1,736	1,736

Notes: This table shows the impacts of an additional year of service on women's occupational choise based on Eq.(1). Sample: women who turned 18 six months before or after June 1952 (bandwidth h=6), excluding non-Jewish, those who immigrated to Israel after 18 or married before 17. Robust tandard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Covariate Balance

Panel A: Census of 1961

	Obs	Treated mean	Control mean	Diff:T-C	Separate tests	Joint test
Mizrahi	3237	0.394	0.382	0.025	1.960	0.334
Immigration status	3382	0.608	0.639	-0.064	0.274	0.334
Age at immigration	2056	13.891	13.998	-0.026	2.209	0.334

Panel B: Census of 1972

	Obs	Treated mean	Control mean	Diff:T-C	Separate tests	Joint test
Mizrahi	3314	0.397	0.398	-0.002	0.284	0.469
Immigration status	3473	0.610	0.652	-0.087	3.586	0.469
Age at immigration	2140	13.785	14.087	-0.079	0.181	0.469

Notes: This table presents the results of testing covariate balance across treatment groups. N is the number of observations, Mean is the mean, and Var is the sample variance of the covariate within each group. ASD is the absolute standardized difference as defined by Eq.(2). The separate and joint tests are based on Eq.(3) The analysis uses a bandwidth of h=12, which covers one year before and after the policy change. P-values are adjusted for multiple testing using the Bonferroni method.

Table A.6: High skill occupations in the Israeli censuses of 1961 and 1972

	1961 Census		1972 Census	
ISCO-08	**CBS**	**Occupational Group**	**CBS**	**Occupational Group**
1. Managers				
	10	Managers ingovernmental, municipal and public institutions	21	Public service manager
	11	Managers in industrial plants, mines, minerals, electricity, gas, water and sanitation services	22	Unit managers in the field of natural sciences
	12	Managers in construction factories	23	Unit managers in the field of humanities and social sciences
	13	Managers in retail, managers of banks and financial institutions and managers of transport and storage	24	Other managers
	14	Managers in personal and business services		
	15	Other managers		
2. Professionals				
	00	Architects, engineers, surveyors and technical workers	00	Academic workers in the life sciences
	01	Chemists, physicists, geologists, biologists, veterinarians, other workers in the natural sciences	01	Academic workers in natural sciences
	02	Medical doctors, surgeons and dentists	02	Engineers and architects
	05	Teachers	03	Medical doctors and dentists
	07	Legal professionals	04	pharmacists and veterinarians
	73	Opticians	05	Legal professionals
			06	Academic workers in the social sciences
			07	Academic workers in the humanities
			08	Teachers in higher education
			09	High school teachers
			10	School teachers
			18	Systems analysts and programmers
3. Technicians and associate professionals				
	03	Nurses and midwives	15	Nurses and other paramedical professions
	04	Medical and health technicians and professional workers in medicine	11	Accountants
	09	Workers in the humanities and social sciences, accountants, social workers	14	Social workers and probation officers
			16	Natural science technicians
			17	Engineering technicians

Notes: Based on ILO .

Table A.7: The effect of one additional year of service on early marriage

	(1) Census of 1961	(2) Census of 1972
Longer service	-0.036	-0.015
	(0.023)	(0.024)
Mean	*0.354*	*0.287*
Effect as % of mean	-10.2%	-5.4%
Observations	1,698	1,656

Notes: This table shows the impacts of an additional year of service on women's probability to marry before 19 based on Eq.(1). Sample: women who turned 18 six months before or after June 1952 (bandwidth h=6), excluding non-Jewish, those who immigrated to Israel after 18 or married before 17. Robust tandard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Placebo treatment for unaffected cohorts

Panel A: Census of 1961

	c	n	p=c/n
Labor force participation	2	40	0.05
Employment	2	40	0.05
High skilled	5	40	0.13
Higher education	2	40	0.05
#Children	1	40	0.03

Panel B: Census of 1972

	c	n	p=c/n
Labor force participation	11	252	0.04
Employment	7	252	0.03

Notes: This table presents the results of the untreated-cohorts placebo test, conducted by estimating the model described by Eq.(1) using cohorts unaffected by the policy change. c is the number of placebo effects larger in absolute value from the observed effects, and n is the number of cohorts used in each test.

Table A.9: The effect of one additional year of service using h=12 in the 1961 census

	(1) LFP	(2) Employment	(3) Higher edu.	(4) High skilled	(5) #Children
Longer service	0.048***	0.046***	0.028***	0.031**	-0.255***
	(0.018)	(0.017)	(0.007)	(0.013)	(0.045)
Mean	*0.379*	*0.365*	*0.026*	*0.132*	*1.929*
Effect as % of mean	12.7%	12.5%	104.6%	23.8%	-13.2%
Observations	3,237	3,237	3,229	3,196	2,818

Notes: This table shows the impacts of an additional year of service on women's outcomes based on Eq.(1). Sample: women who turned 18 a year before or after June 1952 (bandwidth h=12), excluding non-Jewish, those who immigrated to Israel after 18 or married before 17. Robust tandard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: The effect of one additional year of service using different bandwidths in the 1972 census

	(1) LFP	(2) Employment	(3) Higher edu.	(4) High skilled	(5) #Children
h=3					
Longer service	0.036	0.041	0.026	-0.003	-0.088
	(0.035)	(0.034)	(0.019)	(0.024)	(0.127)
Mean	*0.365*	*0.358*	*0.056*	*0.133*	*3.259*
Effect as % of mean	9.8%	11.4%	46.1%	-2.3%	-2.7%
Observations	867	867	844	867	807
h=4					
Longer service	0.049	0.052*	0.019	-0.003	-0.106
	(0.030)	(0.030)	(0.016)	(0.021)	(0.111)
Mean	*0.357*	*0.350*	*0.060*	*0.144*	*3.220*
Effect as % of mean	13.6%	14.9%	32.5%	-2.0%	-3.3%
Observations	1,171	1,171	1,141	1,171	1,092
h=5					
Longer service	0.049*	0.052*	0.015	-0.003	-0.099
	(0.027)	(0.027)	(0.015)	(0.019)	(0.099)
Mean	*0.367*	*0.358*	*0.057*	*0.142*	*3.214*
Effect as % of mean	13.4%	14.5%	26.6%	-2.4%	-3.1%
Observations	1,482	1,482	1,444	1,482	1,389
h=6					
Longer service	0.046*	0.049*	0.014	-0.006	-0.083
	(0.025)	(0.025)	(0.013)	(0.018)	(0.091)
Mean	*0.378*	*0.370*	*0.059*	*0.151*	*3.158*
Effect as % of mean	12.2%	13.3%	23.0%	-3.8%	-2.6%
Observations	1,736	1,736	1,686	1,736	1,622
h=7					
Longer service	0.042*	0.044*	0.011	-0.008	-0.083
	(0.024)	(0.023)	(0.012)	(0.017)	(0.085)
Mean	*0.389*	*0.382*	*0.059*	*0.153*	*3.168*
Effect as % of mean	10.7%	11.6%	19.5%	-5.3%	-2.6%
Observations	2,024	2,024	1,968	2,024	1,896
h=8					
Longer service	0.040*	0.042*	0.011	-0.008	-0.091
	(0.022)	(0.022)	(0.012)	(0.016)	(0.080)
Mean	*0.379*	*0.373*	*0.053*	*0.144*	*3.186*
Effect as % of mean	10.6%	11.2%	20.5%	-5.6%	-2.9%
Observations	2,306	2,306	2,242	2,306	2,160
h=9					
Longer service	0.037*	0.038*	0.010	-0.008	-0.099
	(0.021)	(0.021)	(0.011)	(0.015)	(0.075)
Mean	*0.383*	*0.375*	*0.055*	*0.142*	*3.181*
Effect as % of mean	9.7%	10.3%	17.7%	-5.7%	-3.1%
Observations	2,570	2,570	2,493	2,570	2,402

Continued on next page

Table A.10: The effect of one additional year of service using different bandwidths in the 1972 census (continued)

	(1) LFP	(2) Employment	(3) Higher edu.	(4) High skilled	(5) #Children
h=10					
Longer service	0.035*	0.036*	0.009	-0.005	-0.106
	(0.020)	(0.020)	(0.010)	(0.014)	(0.072)
Mean	*0.387*	*0.380*	*0.056*	*0.139*	*3.175*
Effect as % of mean	9.0%	9.5%	15.8%	-3.9%	-3.3%
Observations	2,804	2,804	2,720	2,804	2,624
h=11					
Longer service	0.034*	0.035*	0.009	-0.002	-0.106
	(0.019)	(0.019)	(0.010)	(0.014)	(0.069)
Mean	*0.386*	*0.378*	*0.055*	*0.137*	*3.156*
Effect as % of mean	8.9%	9.3%	16.9%	-1.4%	-3.4%
Observations	3,078	3,078	2,987	3,078	2,883
h=12					
Longer service	0.032*	0.033*	0.010	0.001	-0.104
	(0.019)	(0.018)	(0.009)	(0.013)	(0.066)
Mean	*0.394*	*0.386*	*0.056*	*0.140*	*3.148*
Effect as % of mean	8.2%	8.5%	17.3%	0.6%	-3.3%
Observations	3,314	3,314	3,221	3,314	3,105

Notes: This table shows the impacts of an additional year of service on women's outcomes based on Eq.(1). Sample: women who turned 18 around June 1952, excluding non-Jewish, those who immigrated to Israel after 18 or married before 17. Robust tandard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: The effect of one additional year of service using different kernelss in the 1972 census

	(1) LFP	(2) Employment	(3) Higher edu.	(4) High skilled	(5) #Children
Panel A: Parabolic kernel					
Longer service	0.048*	0.050**	0.011	-0.005	-0.084
	(0.025)	(0.025)	(0.013)	(0.018)	(0.089)
Mean	*0.378*	*0.370*	*0.059*	*0.151*	*3.158*
Effect as % of mean	12.7%	13.6%	19.1%	-3.5%	-2.6%
Observations	1,736	1,736	1,686	1,736	1,622
Panel B: Rectangular kernel					
Longer service	0.039*	0.042*	0.009	-0.012	-0.043
	(0.023)	(0.023)	(0.012)	(0.016)	(0.082)
Mean	*0.378*	*0.370*	*0.059*	*0.151*	*3.158*
Effect as % of mean	10.3%	11.3%	15.3%	-7.6%	-1.4%
Observations	1,736	1,736	1,686	1,736	1,622

Notes: This table shows the impacts of an additional year of service on women's outcomes based on Eq.(1). Sample: women who turned 18 six months before or after June 1952 (bandwidth h=6), excluding non-Jewish, those who immigrated to Israel after 18 or married before 17. Robust tandard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: The Romano-Wolf multiple hypothesis correction

Panel A: Census of 1961

Variable	Model p-value	Resample p-value	Romano-Wolf p-value
Labor force participation	0.01	0.01	0.01
Employment	0.01	0.01	0.02
Higher education	0.00	0.00	0.00
High skilled	0.00	0.00	0.00
#Children	0.06	0.04	0.04

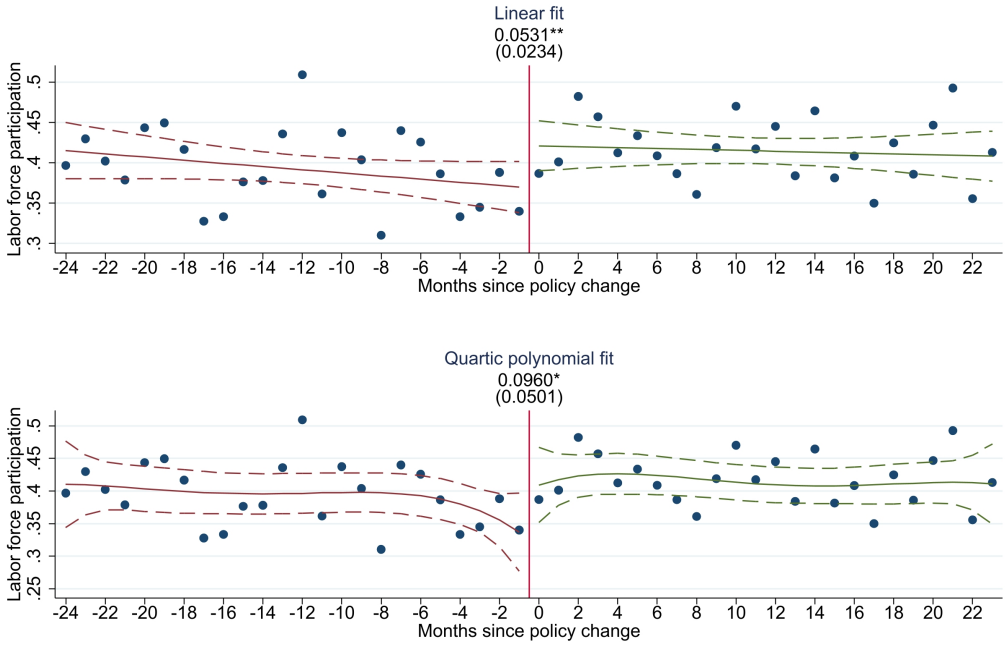
Panel B: Census of 1972

Variable	Model p-value	Resample p-value	Romano-Wolf p-value
Labor force participation	0.07	0.07	0.23
Employment	0.05	0.06	0.18
Higher education	0.31	0.29	0.67
High skilled	0.36	0.36	0.67
#Children	0.74	0.76	0.76

Notes: The table reports the results for the Romano-Wolf multiple hypothesis correction, using 1500 replications.

B Figures

(a) Labor Force Participation



(b) Employment

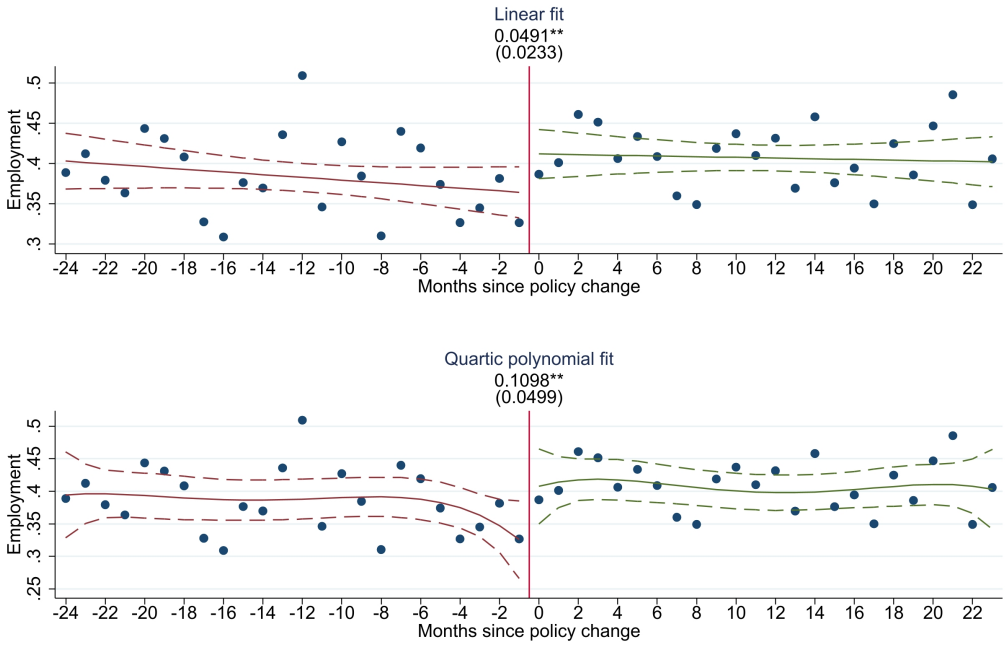


Figure B.1: Labor market outcomes as a function of the month of turning 18, Census of 1972

Notes: This figure presents the global linear and quartic polynomial regressions of the labor market variables around the cutoff of August 1952. The red vertical line represents the cutoff. Dashed lines correspond to the 95% confidence interval. Blue markers represent the average outcome. Sample: women who turned 18 two years before or after the law change (bandwidth h=24), excluding non-Jewish, those who immigrated to Israel after 18 or married before 17. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

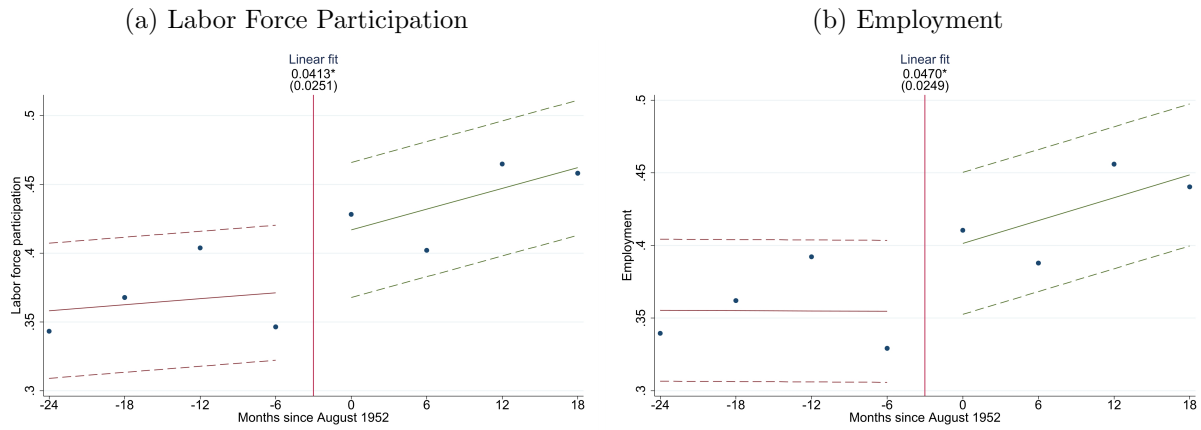


Figure B.2: Labor market outcomes as a function of the month of turning 18, Census of 1961

Notes: This figure presents the global linear regressions of the labor market variables around the cutoff of August 1952. The red vertical line represents the cutoff. Dashed lines correspond to the 95% confidence interval. Blue markers represent the average outcome for each birth month. Sample: women who turned 18 two years before or after the law change (bandwidth h=24), excluding non-Jewish, those who immigrated to Israel after 18 or married before 17. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

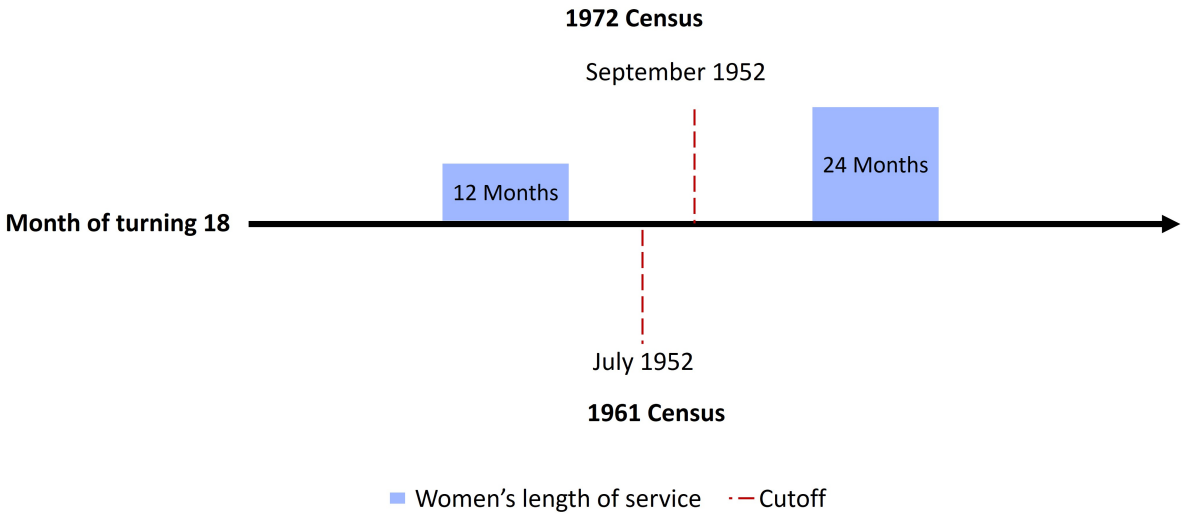


Figure B.3: Cutoff Definition and Length of Service

Notes: This figure describes the time frame used in creating the sample of each census. The red dashed lines describe the timing of treatment defined in each census, and the black dashed lines describe the beginning and end of the time frame (a year before and a year after the policy change).

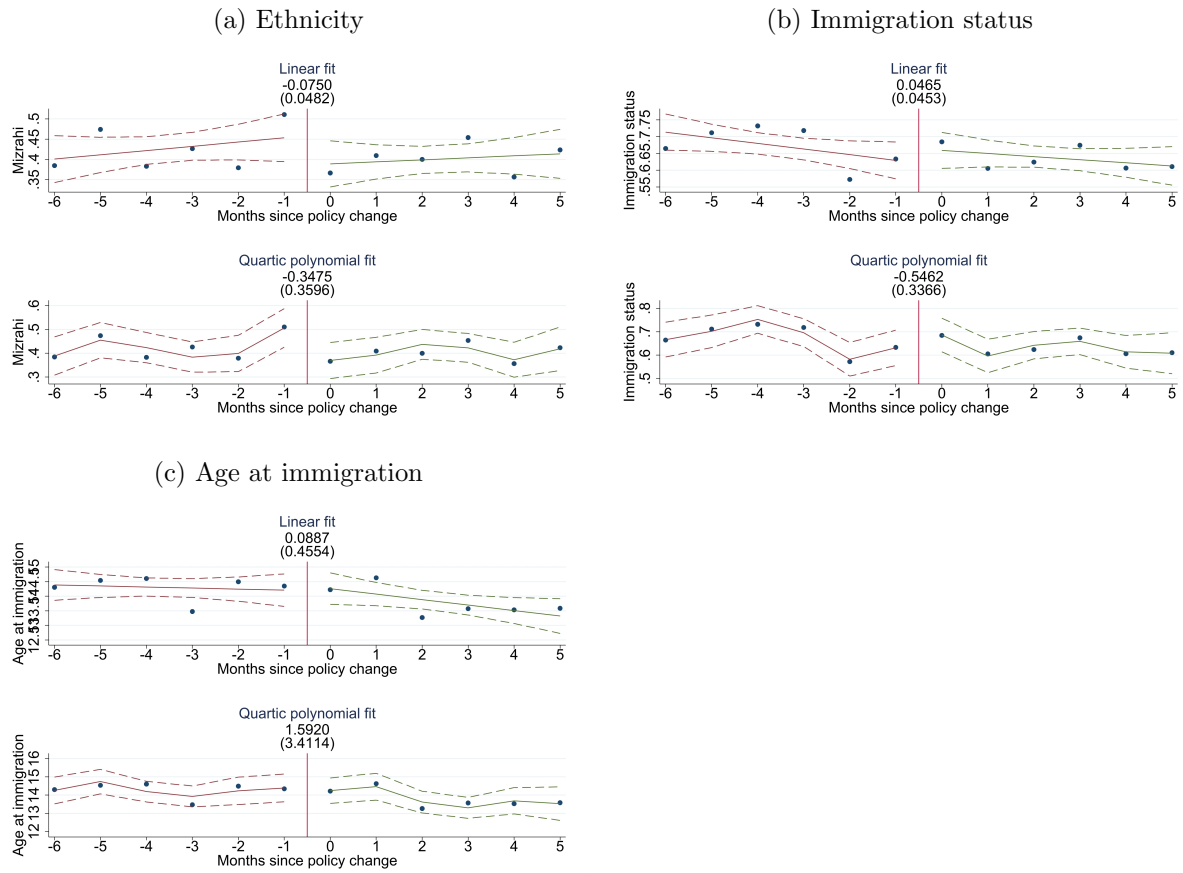


Figure B.4: Continuity test of pre-treatmnt variables

Notes: This figure presents the a local linear and a quartic polynomial regressions of the pretreatment variables around the policy change of August 1952. The red vertical line represents the policy change. Dashed lines correspond to the 95% confidence interval. Blue markers represent the average for each birth month. Sample: women who turned 18 two years before or after the law change (bandwidth h=24), excluding non-Jewish, those who immigrated to Israel after 18 or married before 17. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

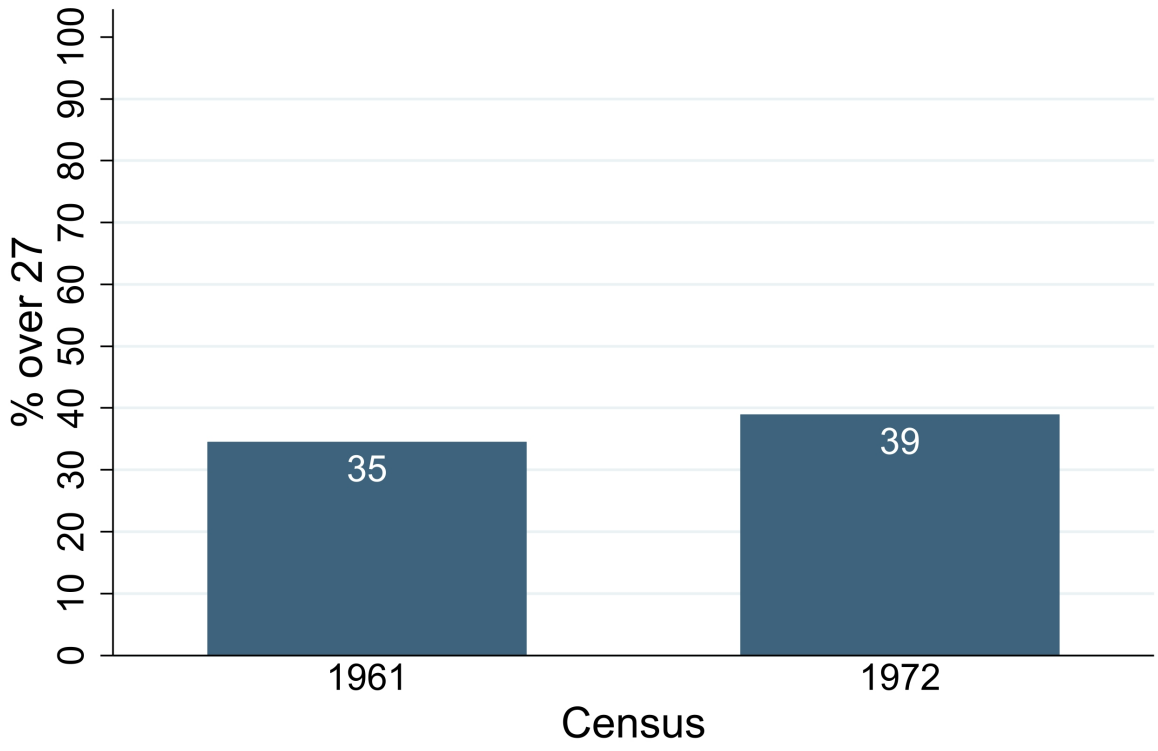
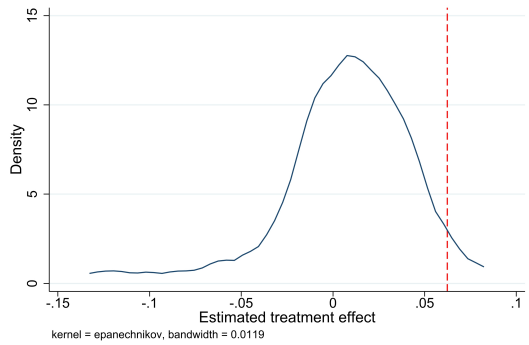
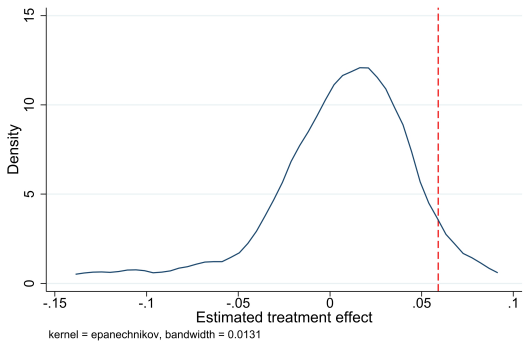


Figure B.5: Age composition of female students

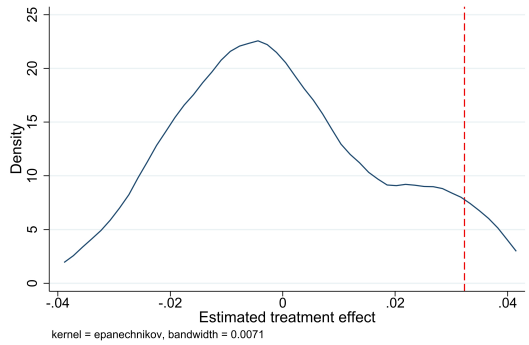
Notes: This figure presents the age composition of Jewish women who reported being current students in the 1961 and 1972 censuses. The data represents a subsample of Jewish women aged 20 and above who identified themselves as students at the time of each census.



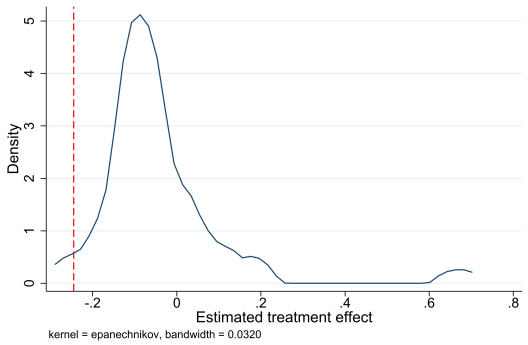
(a) Labor force participation



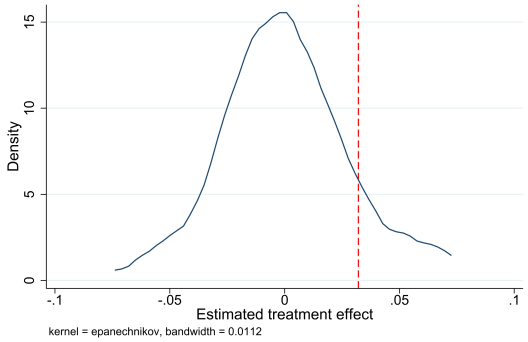
(b) Employment



(c) Higher education



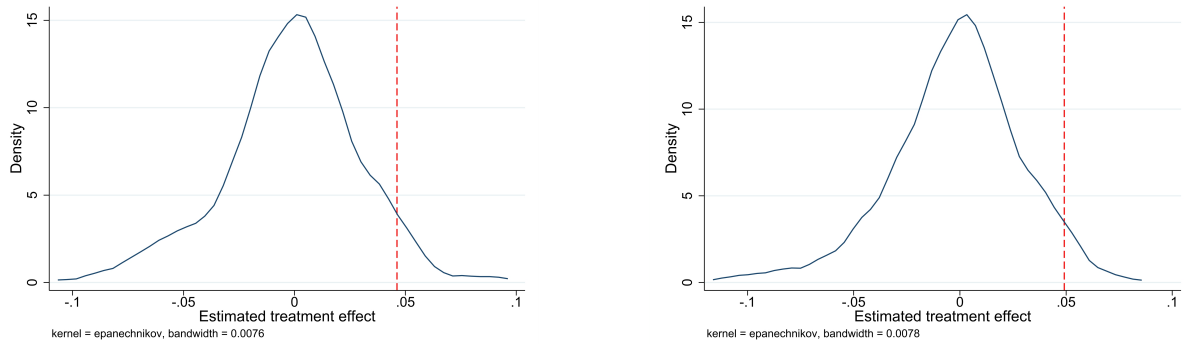
(d) Number of children



(e) High skilled

Figure B.6: The distribution of placebo treatments using unaffected cohorts in the census of 1961

Notes: This figure presents the distribution of placebo treatment effect, resulted from using cohorts which were not affected by the policy change to estimate the model described by Eq.(1). The red dashed line represents the observed effects, as presented in table 2 Panel B.



(a) Labor force participation (b) Employment

Figure B.7: The distribution of placebo treatments using unaffected cohorts in the census of 1972

Notes: This figure presents the distribution of placebo treatment effect, resulted from using cohorts which were not affected by the policy change to estimate the model described by Eq.(1). The red dashed line represents the observed effects, as presented in Table 2, Panel B.

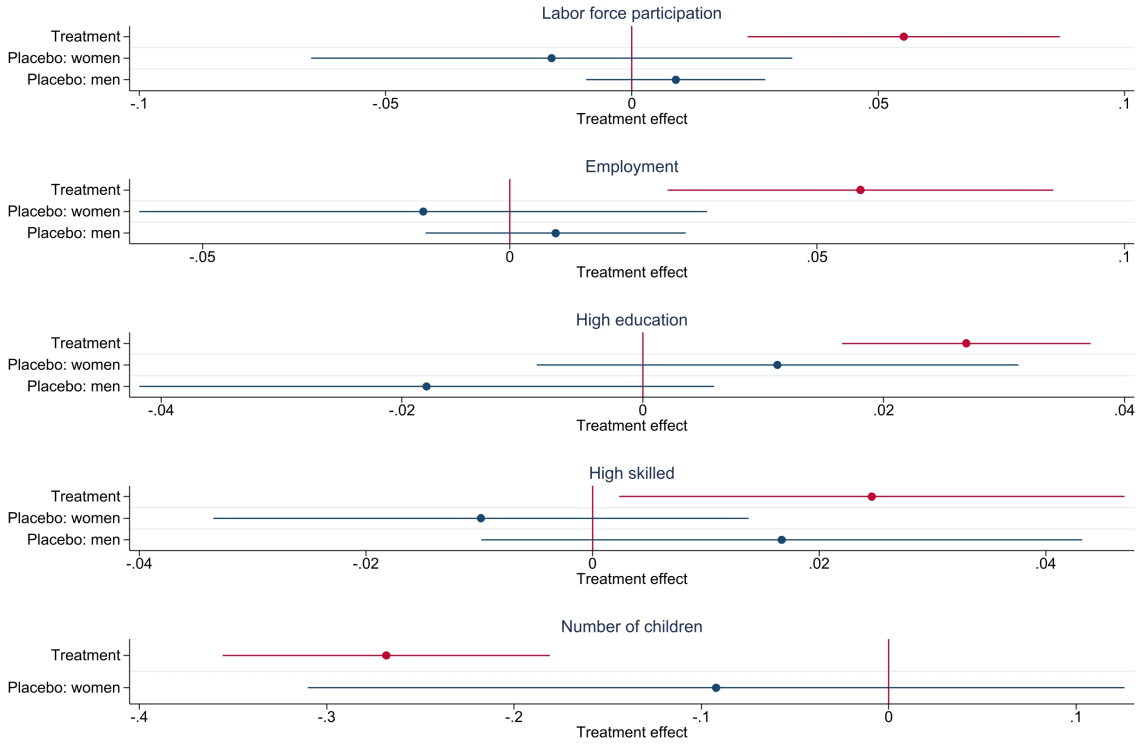


Figure B.8: Placebo treatment for unaffected cohort members- 1961 census

Notes: This figure presents the results of the placebo test using a sample of Jewish men and women who are members of the same treated cohort, but were unaffected by the policy change. The markers corresponds to the estimated treatment effect, and the spikes correspond to the 90% confidence interval.

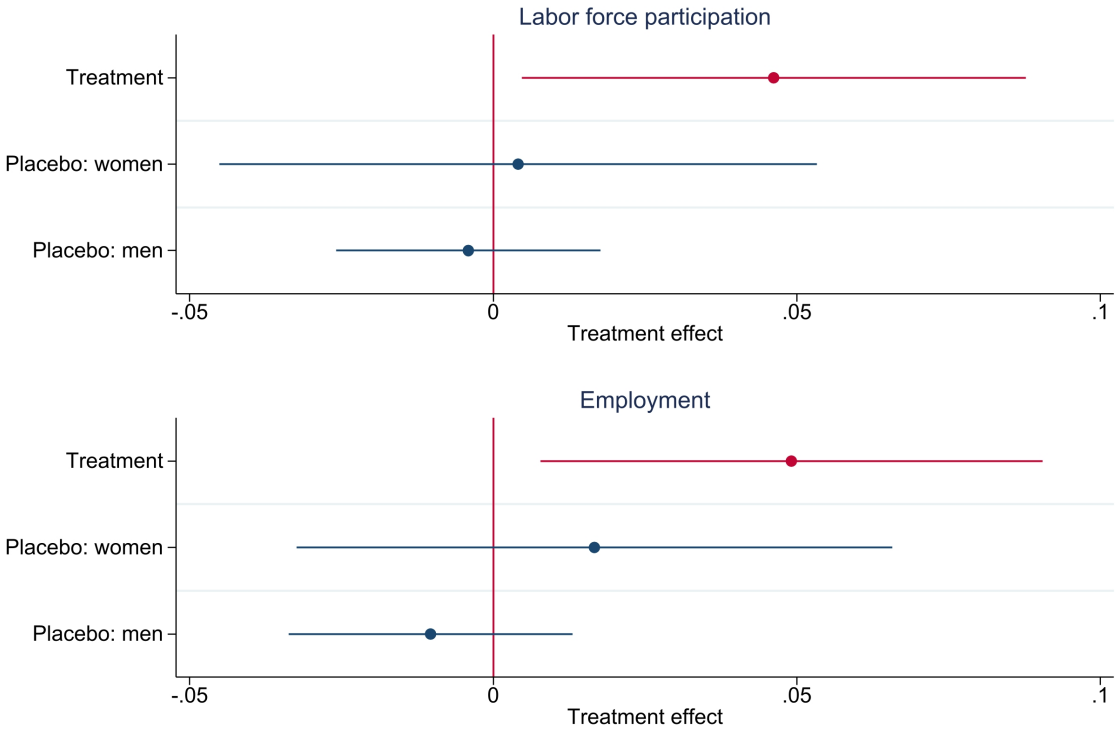


Figure B.9: Placebo treatment for unaffected cohort members- 1972 census

Notes: This figure presents the results of the placebo test using a sample of Jewish men and women who are members of the same treated cohort, but were unaffected by the policy change. The markers corresponds to the estimated treatment effect, and the spikes correspond to the 90% confidence interval.