The Impact of Extended Reproductive Time Horizons: Evidence from Israel's Expansion of Access to IVF

Naomi Gershoni (TAU)

31 December 2015, 11:15-12:30, bld. 72, room 465

Abstract: Women who delay childbearing to make time-costly career and educational investments face a lower probability of having a child, since women's fertility significantly declines with age. In addition, they may be penalized on the marriage market for their lower "reproductive capital" and end up with a lower-quality spouse. Israel's 1994 policy change to make *in vitro* fertil- ization and other assisted reproduction technologies free created an exogenous shock to later life fecundity, providing women with a form of insurance against age-related infertility. This natural experiment can be used to study the impact of expected fertility decline on women's educational choices and marriage outcomes. We find that following the policy change, women are more likely to marry later, complete college education, and achieve post-college education. Moreover, after the change, the observed decrease in spousal quality for women who get married in their thirties rather than their twenties dissipates. This suggests that both men and women's decisions were affected by their updated perception of women's fertility prospects. More gener- ally, our findings indicate that the asymmetry in later life fertility between men and women is an important force in explaining women's educational, career, and marriage outcomes, and thus policies that protect against later life infertility can have far-reaching impacts.

The Impact of Extended Reproductive Time Horizons: Evidence from Israel's Expansion of Access to IVF

Naomi Gershoni* and Corinne Low†

November 8, 2015

Abstract

Women who delay childbearing to make time-costly career and educational investments face a lower probability of having a child, since women's fertility significantly declines with age. In addition, they may be penalized on the marriage market for their lower "reproductive capital" and end up with a lower-quality spouse. Israel's 1994 policy change to make *in vitro* fertilization and other assisted reproduction technologies free created an exogenous shock to later life fecundity, providing women with a form of insurance against age-related infertility. This natural experiment can be used to study the impact of expected fertility decline on women's educational choices and marriage outcomes. We find that following the policy change, women are more likely to marry later, complete college education, and achieve post-college education. Moreover, after the change, the observed decrease in spousal quality for women who get married in their thirties rather than their twenties dissipates. This suggests that both men and women's decisions were affected by their updated perception of women's fertility prospects. More generally, our findings indicate that the asymmetry in later life fertility between men and women is an important force in explaining women's educational, career, and marriage outcomes, and thus policies that protect against later life infertility can have far-reaching impacts.

1 Introduction

The introduction of technology that allowed women to delay fertility—"the pill"—has been tied to greater educational investments and improved labor market outcomes for women (Goldin and Katz, 2002; Bailey, 2006, 2010). This suggests that there is a career-family tradeoff. Raising children interferes with women's career investments, and has recently been shown to be tied to substantial wage declines (Adda et al., 2011; Kleven et al., 2015). However, women choosing to delay fertility

*Tel Aviv University, The Eitan Berglas School of Economics, naomiger@post.tau.ac.il.

†University of Pennsylvania, The Wharton School, corlow@wharton.upenn.edu. We are grateful for the advice of Analia Schlosser, Pierre-Andre Chiappori, Cristian Pop-Eleches, and Yoram Weiss. We thank Tal Gross, Alma Cohen, David Weiss, and Maya Rossin-Slater for helpful comments, and Ting Wang and Jennie Huang for excellent research assistance. We thank the staff of Tel Aviv University and the Israeli Central Bureau of Statistics for facilitating data access. Funding from the Wharton Pension Research Council, the Wharton Dean's Research Fund, and the University of Pennsylvania Population Studies Center is gratefully acknowledged.

in order to make career investments encounter a second biological constraint: delaying fertility significantly lowers the probability of conception and successful birth.

Unlike men, whose fertility deteriorates gradually with age, women experience a sharp decline in fertility starting in their mid thirties until a complete loss of fertility at menopause. This gender-asymmetry in later life fertility may be a major source of inequality in outcomes and achievements between men and women, especially as career investments become longer and more demanding.[1] Young women may be discouraged from making time-costly career and educational investments that delay marriage and childbearing. At the same time, women who make such investments may be penalized on the marriage market for their lower "reproductive capital," as shown in Low (2014). Similarly to any other economic asset, the depreciation rate of "reproductive capital" is expected to affect initial investment decisions. In light of the limited sources for exogenous shifts in women's later-life fertility, the role of this biological constraint in explaining women's career decisions and outcomes is not well understood.

Ideally, to study the impact of expected fertility on education decisions and marriage outcomes, we would randomly assign women to have longer or shorter periods of fecundity, and measure differences in educational investments, age at marriage, and quality of marriage match. In lieu of perfect random assignment, Israel's unprecedented decision to provide full health insurance coverage for *in vitro* fertilization (IVF) and other assisted reproductive technologies (ART) through the 1994 National Health Insurance Law provides a natural experiment: following the introduction of the new policy, women, and their prospective partners, could expect a longer period of fecundity.[2] In the ensuing years, Israeli families made wide use of these services. In 2002, for instance, 1,657 IVF treatment cycles per million people have been performed, compared to 126 in the United States (Collins, 2002).[3]

We hypothesize that public access to IVF, which made it easier for older women to conceive, changed younger women's perceived cost of career investment, resulting in increased education and potential earnings. Moreover, potential partners' estimation of women's fecundity horizons gradually changed, improving older women's marriage market outcomes. Our theory of impact does not rely on the affected women actually using the technology themselves. Rather, we propose that women, and their potential partners, view access to the technology as a form of insurance against age-related infertility. Moreover, even without specific knowledge of the policy change, the large amount of media attention to older women having children, and first-hand experience of

[1] One example of such "longer" investments would be attaining graduate education.

[2] In addition to the effect of the policy change itself, there was also a concurrent improvement in technology and widespread media attention to assisted reproduction surrounding the time of the policy change.

[3] We focus on IVF funding rather than other ARTs since this was the technology that most significantly affected chances of conception for older women. Moreover, costs of IVF are much higher than those of other infertility treatments, making funding a crucial determinant of usage. In the US for example, while costs of ovarian stimulatory drugs are a few hundred dollars, a single IVF treatment cycle costs around 10,000 dollars and the overall cost of IVF treatments per delivery is estimated to be higher than 50,000 dollars (Collins, 2001).

observing motherhood at an older age, have facilitated a *general* shift in beliefs regarding the time horizon of fertility. Thus, women's decisions would be impacted whether they eventually use the technology or not.

Using data from the 2008 Israeli population census, we study educational outcomes by birth cohort, identifying cohorts that were affected by the policy as those at a relevant age to make educational decisions at the post period. We also use retrospective marriage data to examine shifts in women's age at first marriage by year of marriage. Finally, we match data on women and their spouses to evaluate the "quality" of "older" brides' spouses. Spousal quality is measured by the husband's wages controlling for a variety of factors. Our identification strategy relies on looking for gender differential changes in education rates and age-at-first-marriage, presuming that men's marriage timing and educational decisions should not be impacted by the policy change (or at least impacted drastically less than women's). We also use Arab women, who were less likely to use the technology and less likely to be on the margin of large career investments, as an alternate control group, and find consistent effects. For marriage market outcomes, we compare women who marry over thirty and whose reproductive capital significantly increased due to the policy change, against women who marry younger and whose reproductive capital remained essentially unchanged. Additionally, we look across genders in a triple difference specification.

For each of these outcomes, we examine changes over time starting in the year of the policy change. Despite the discontinuous nature of the change in IVF funding, we expect a gradual shift in outcomes, as awareness builds up over time based on people's experience. The gradual information dispersion may be especially salient for outcomes that involve equilibrium on the marriage market, which crucially relies on men's perceptions of potential wives' reproductive fitness and may take longer to update.

We find statistically significant evidence that after the policy change women completed more education and delayed marriage. Following the change, women's age at first marriage increased by 4 months on average. For education we find an increase of 3 percentage points in college completion and approximately the same for graduate education completion. In fact, this implies that the rate of change was larger for graduate education, since the initial level was lower.[4] This last finding strongly supports our hypothesis, as later-life fertility is expected to have a larger impact for decisions that involve longer investments and hence longer delays. Furthermore, we find a large change in marriage outcomes for women who marry when older, with the "older marriage" penalty, in terms of spousal income, dissipating. These findings are confirmed by a number of robustness checks, including an event study analysis that pinpoints the time of change in outcomes to be exactly as predicted in accordance with the policy time-line. Specifically, for college education, the

[4]These estimates come from Table 2, column 1, Table 3, column 3, and Table 4, column 1. We use the simple difference-in-difference specification for these estimates, as the coefficients are easier to interpret. For college education, we use the specification with group-specific time trends included, since there is a differential time trend evident in the pre-period.

gender-differential change occurs exactly for the cohort which was most likely to enter school the year of the change and for age-at-first-marriage, the shift starts precisely with the introduction of the policy.[5]

Our findings indicate that mitigating women's concerns for age related infertility, alters women's educational and marriage decisions. Furthermore, the marriage market responds to the change, in a way that measurably impacts matching along the dimension of spousal income, showing the dollars-and-cents value of reproductive capital. These results bolster the theory that the time-cost of education in terms of lost fertile years may be an important factor in women's educational decisions, and that fertility may be a valuable "asset" in attracting a more high-powered spouse on the marriage market. This research has implications for the economic understanding of women's career investment decisions, of the costs of aging to women, and of income inequality between genders. It can also inform an analysis of the welfare implications of a policy such as universal access to IVF, which appears to not only impact the women actually using IVF, but rather, affect all women by insuring against future infertility. Together, these findings point to the importance of biological differences in divergence of economic outcomes, and the potential role of policies in blunting this effect.

The remainder of the paper proceeds as follows: Section 2 discusses prior literature and the theoretical predictions; Section 3 describes the empirical setting for our project and the data we use; Section 4 presents results and tests their robustness, and Section 5 concludes.

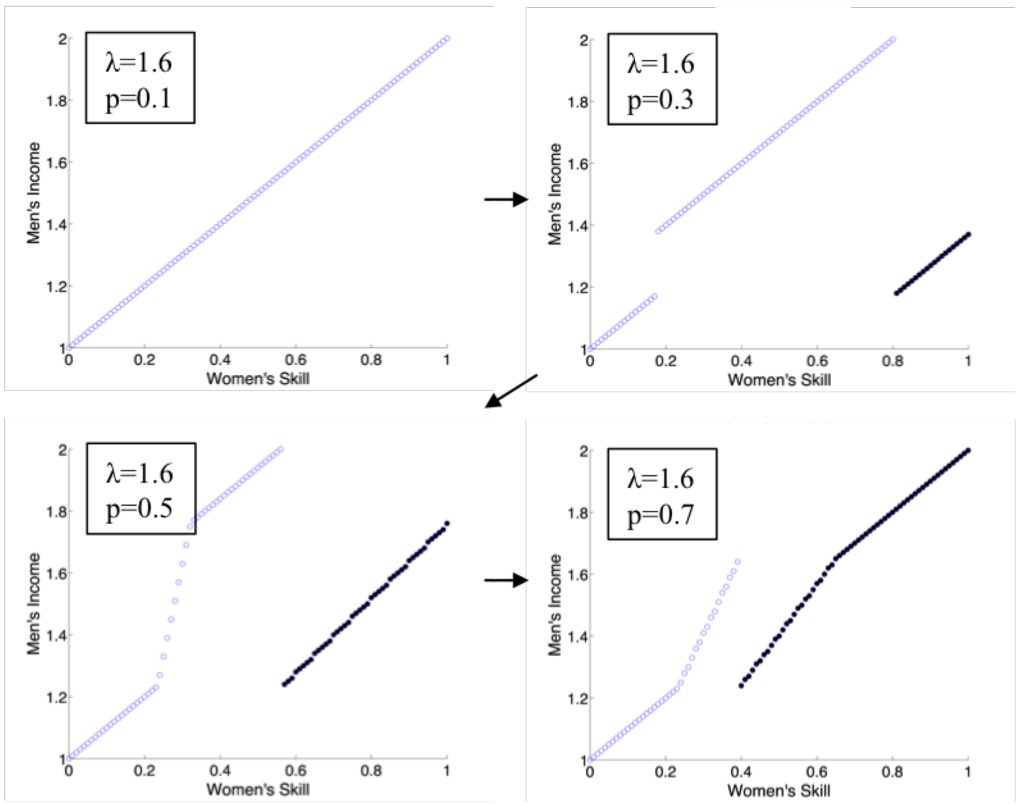
2 Literature and Predictions

This research is related to previous literature on the impact of fertility on the timing of marriage and childbearing, and on educational, career, and marriage outcomes. The link between fertility and career and education has been up to now explored in the literature as a case of too much, rather than too little, fertility. This literature establishes that there *is* a career-family tradeoff, and therefore a payoff to delayed fertility in labor market outcomes (Loughran and Zissimopoulos, 2009; Buckles, 2008; Blackburn et al., 1993; Taniguchi, 1999; Gustafsson, 2003; Miller, 2011; Avellar and Smock, 2003; Wilde et al., 2010; Goldin and Katz, 2002; Adda et al., 2011).

On the other hand, delaying fertility in favor of career investments could lead to problems on the marriage market, as suggested by Siow (1998); Dessy and Djebbari (2010); Bronson and Mazzocco (2013). Low (2014) shows that in the US during the '70s and '80s, women who earned graduate degrees married poorer spouses than women with only college degrees, potentially due to their lower fertility, and thus lower suitability as spouses. Moreover, Low's online dating experiment demonstrated that men valued fertility in potential dating partners—each year a woman aged was

[5] We also implement a Quandt Likelihood Ratio test, which tests every possible breakpoint from our sample, and takes the sup of the F stats—the actual year of the policy change has the highest F-stat, and it is significant even after controlling for the multiple hypothesis testing.

Figure 1: Theoretical predictions from Low (2014): As later-life fertility, p, increases, education and matching outcomes among high-skilled women improve, for constant return to education (λ)



Notes: x-axis represents women's skill, while y-axis represents men's income (proxy for quality). Shaded dots represent women who have invested in education. The placement of the dots represents the matching between men and women.

worth $7,000 in annual income to potential partners. Interestingly, this effect was driven only by men who had no children already and had accurate knowledge of the age-fertility gradient. Low's theoretical work suggests, though, that if the decline in later-life fertility is abated, these marriage market penalties should also lessen. Better marriage outcomes for women who delay marriage, would in turn lead to more women choosing to do so. Figure 1 shows the theoretical predictions for human capital investments and marriage matching when the probability of successful conception later in life, p, increases. First, no women invest in time-costly education, because the costs, in terms of forgone fertility, are too high (top left). In this case higher quality women are matched with higher income men. As later-life fertility, p increases, some high-quality women become educated, but they are penalized on the marriage market with lower-quality spouses. As p increases more, more women invest in education, and the marriage-market penalty dissipates. Finally, when expected fertility is high enough, assortative matching returns, and high skill women match with higher income men, despite their lower fertility (bottom left).

This work suggests that a technology that insures against infertility following fertility delay could increase women's educational investments and marriage outcomes (just as Goldin and Katz (2002) showed that a technology that allowed this delay in the first place led to more educational investments). However, the research on assisted reproduction technology has, to date, mainly focused on outcomes of women who actually use the technology, rather than younger women who perceive it as offering insurance.

A series of papers uses the variation in the mandated insurance coverage of assisted reproductive technology (ART) across US states and over time to determine how more coverage affects IVF usage and outcomes (Velez et al., 2014; Hamilton and McManus, 2012; Bitler and Schmidt, 2012, 2006; Bundorf, Henne, and Baker, Bundorf et al.; Buckles, 2013; Schmidt, 2007, 2005), offering suggestive evidence that when coverage goes up, more women use IVF, fertility rates for older mothers go up, and multiple births rise.

A much more limited literature explores the impact of such mandates on the timing of marriage and childbearing, which allows for the possibility that infertility treatments may impact women beyond those who actually use them to conceive. Ohinata (2011) finds that infertility insurance mandates resulted in 1-2 year delays in first birth among highly educated white women, and Abramowitz (2012, 2014) shows that increased access is associated with marriage delays for white women. The only evidence on education and career outcomes comes from Buckles (2007), which finds suggestive evidence that infertility insurance mandates led to increased labor force participation for women.

However, the approach of using state-year mandates to explore these perception-based outcomes has some significant limitations, especially when discussing general equilibrium shifts in perceptions of both men and women. Since these are small and localized policy changes, awareness is expected to develop very gradually, especially with young women who may not even be managing their own insurance yet. More importantly, there is mixed evidence on how state health insurance mandates influence insurance and labor market equilibrium. At least some suggest that mandates increase insurance premiums more significantly for the most affected workers and therefore negatively affect their wages and employment.[6]

The Israeli policy, to the contrary, applies equally to all and insurance is publicly funded. Therefore, there are no concerns that the observed changes in women's career investment are driven by a shift in employers' costs and preferences for employing older women.[7] Thus, our paper offers the first opportunity to study a large-scale policy change that may have changed not just the actual chance of getting pregnant when older, but, crucially, the beliefs about this chance by both young women considering career investments and men considering marrying them later on. Moreover,

[6]In some cases workers would be willing to accept a wage cut that embodies the insurance benefit. In the case of infertility mandates the most affected workers are older women. Lahey (2012) presents evidence on infertility mandates suggesting that wage shifts will not fully offset the increased premium costs for women in affected age groups, hence labor force participation for this group decreases.

[7]Hence observed changes in women's career investment cannot be a result of the changed terms in labor markets

this is the first paper to empirically study the impacts of a shift in later-life fertility potential on outcomes resulting from the decision to delay childbearing, including educational investments and the marriage match quality.

3 Setting and Empirical Approach

3.1 IVF in Israel

Since the emergence of IVF technology in the early 80s, Israel has been in the forefront of research and actual usage of IVF.[8] However, usage of the technology was still relatively low, and technological advances were slow in coming. In 1994 a significant change in funding policy of assisted reproduction in Israel, accompanied by technology improvements and greater public knowledge of IVF availability, caused a sharp increase in IVF usage. The 1994 law provided *full funding* of all assisted reproduction services for up to two "take-home babies", the most generous IVF coverage anywhere in the world. Figure 2 shows that the number of IVF treatment cycles more than doubled in the 6 years following the approval of the new policy.[9] Although the benefits of the law came into effect in 1995, the increase in the number of IVF treatment cycles began already in 1994, with the large amount of press coverage and increased knowledge on IVF availability.[10] In the year after usage increased, 1995, there was a sharp increase in live deliveries using IVF.

Figure 3 goes further to describe changes in Jewish Israeli women's fertility rates for different age groups over the last decades. Clearly, fertility rates for women in their thirties increase more rapidly after 1994. A more moderate but similar change is apparent for women in their forties.[11] At the same time a decrease in fertility rates is displayed for younger women. As our paper will demonstrate, this may indicate that the policy change not only affected women or couples experiencing infertility, but also influenced other women to postpone childbearing, due to this new form of insurance against later-life infertility.

The 1994 law ushered in an era of expanding usage and technological improvement worldwide, providing access to IVF that is unmatched anywhere in the world. This funding policy is a part of an entire set of "overtly pronatalist" reproductive policies designed to support and endorse Israel's tradition of familism.[12] A significant support of these policies was offered by the Israeli Supreme

[8] The first Israeli "test tube baby" was born in 1982. She was the fifth worldwide.

[9] The Israeli parliament "Kneset" issued a report in 2012 that attributes this dramatic change to the regularization and expansion of IVF funding under the NHI law.

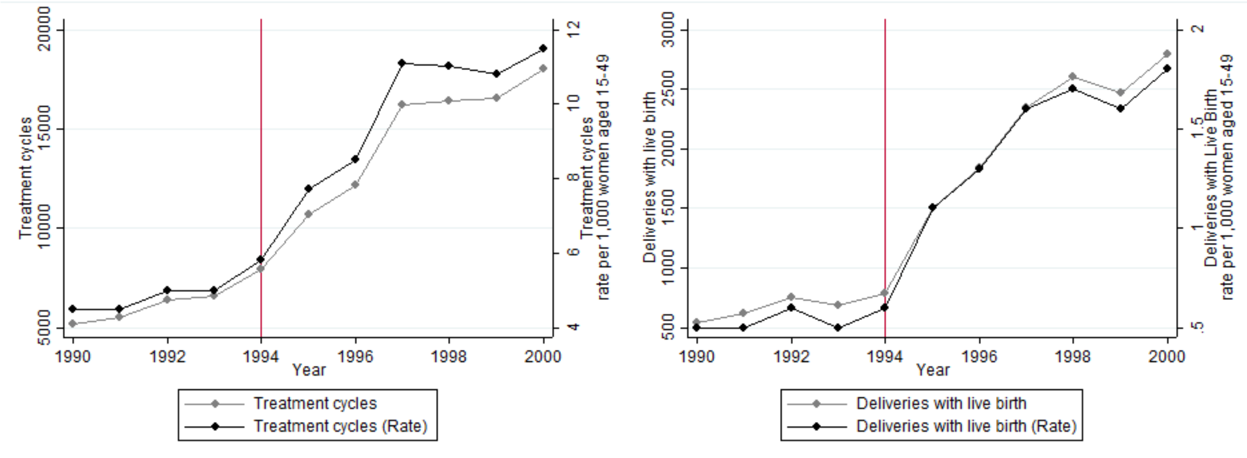
[10] The common measure of usage is the number of IVF treatment cycles relative to the size of fertile women population. Since there is no documentation of the number of women treated each year, it is impossible to assess whether the sharp increase in usage stems from an increase in the number of women undergoing IVF treatments, or from an increase in the number of attempts each IVF patient makes. It is reasonable to assume that it is a result of a combination of these two, especially given the large increase in IVF-assisted births.

[11] Note that the increase in fertility for women over 45 should be attributed solely to increased IVF usage, since naturally conceiving is practically impossible.

[12] Other examples of such policies are governmental child allowances and maternity grants, broad legal protection

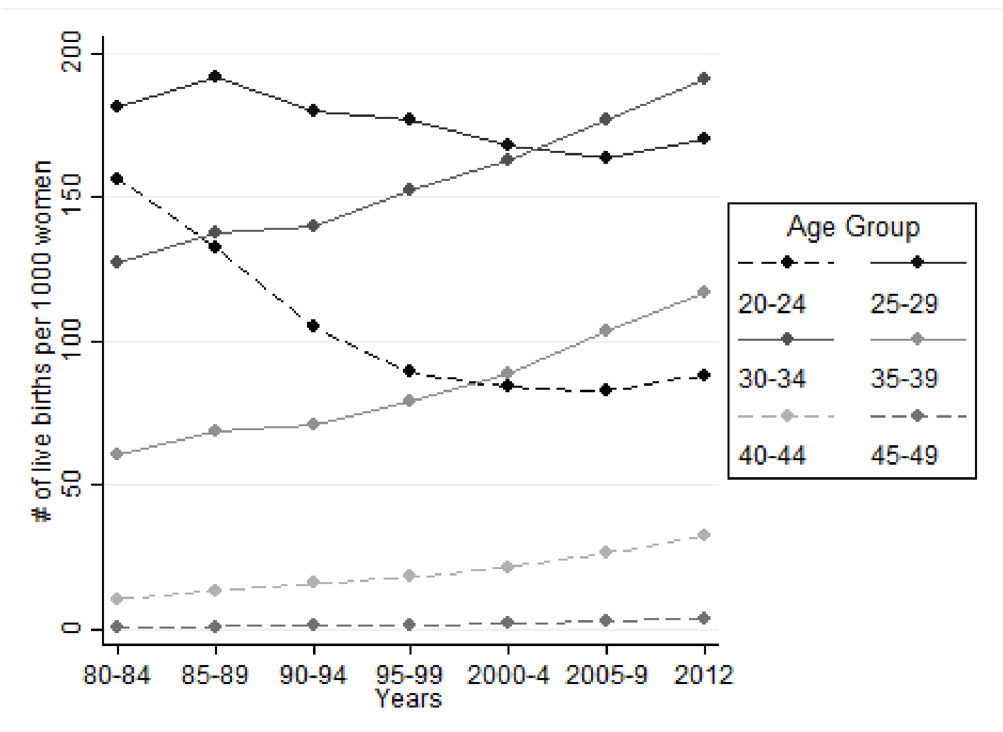
Figure 2: Direct Impacts of IVF Access

(a) IVF Treatment Cycles (b) IVF Deliveries with Live Births



Notes: Data addresses all Israeli women. Data source: Israeli Ministry of Health.

Figure 3: Fertility Rates by Age Group



Notes: The figure presents the number of live births during one year period, per 1000 women in a specified age group, for Jewish Israeli women. Data for five year periods are arithmetic averages. Although fertility is not the main focus of our analysis, we present some additional results on the fertility impact using longitudinal data in the appendix. Figure A1 depicts fertility by age group, while Figure A2 shows the discontinuous increase in births to mothers 38-42 following the policy change.

Court's ruling that a person's right to have a family and be a parent is one of the constitutional human rights.[13]

The emphasis Israelis put on family is also demonstrated by the unusually high marriage and birth rate in comparison to other developed countries.[14] Israel also stands out with an unusually young average marriage age for both genders.[15] This early marriage and high birth rate means that even early educational investments, such as completing college, may infringe on a woman's planned reproductive years, and potentially limit family size. Moreover, Israelis tend to complete education later than in other OECD countries, due to mandatory military service following high school.[16] Thus, in Israel more than in other countries, the decline in probability of successful childbearing that comes with delaying motherhood may have been a salient and significant cost to women considering further educational or career investments.

Following a widely covered, public debate, the 1994 National Health Insurance Law (NHI), enacted by the Kneset, included IVF tests and treatments in the list of free health services that health plans must provide. Like other services specified by the new law, IVF services are provided by the health plans and subsidized by the government. Until 1994, five percent of Israel's residents had no health insurance at all. The other 95 percent were members of four health plans and received funding according to their different policies. There was practically no competition among the different funds since transferability was highly limited. Membership was often the result of political agenda. IVF treatments were covered at least to some extent by the four health plans. The coverage varied both by the number and frequency of covered treatment cycles, and by the terms of eligibility (such as age, marital status and qualification period). The most generous coverage was offered by the largest health plan ("Clalit") which placed almost no limitations on usage.[17] The other health plans offered a limited number of treatment cycles and placed age restrictions and other barriers for eligibility.[18] Therefore, the new law practically applied the Clalit's generous policy on all Israeli residents.

Nevertheless, even the members of Clalit experienced an improvement in access to IVF, simply

of working mothers' rights, extended funding of prenatal care and various tax benefits for parents. For an elaborate discussion of those policies and their evolvement over time see Birenbaum-Carmeli (2003).

[13]See for example High Court 7052/03 Adalla vs. Ministry of Interior.

[14]For example, according to UN data, between 2000-2005 Israel's total fertility rate was 2.91, the highest among all 35 OECD current member countries, and way above the OECD average which was 1.65. According to OECD data, at 2009 the marriage rate in Israel was 6.3 per 1000 residents compared to an OECD average of 5.0.

[15]According to the Israeli Central Bureau of Statistics report "Women and men in Israel 1990-2009" the median age of first marriage in Israel is 3-4 years lower than in other western countries.

[16]According to OECD data, in 2011 Israel's median age at first graduation was slightly above 27 whereas the OECD average was slightly below 25.

[17]Interestingly, this generosity was a result of "faulty computer infrastructure (that could not trace women's treatment and entitlement efficiently) rather than from professional or social conviction" (Birenbaum-Carmeli, 2004)

[18]For example in the "Leumit" health plan the number of treatment cycles was limited to six and the maximal age was 40 (an exceptions committee was authorized to approve up to 3 cycles for women aged 40 to 42, 2 cycles to women aged 42-44 and 1 cycle to women aged 45). In all health plans egg donations required an exceptions committee approval.

because for the first time, the scope of treatment entitlement was clearly and formally defined:

> IVF treatments for the purpose of the birth of two children for couples who do not have children from their present marriage, as well as for childless women who wish to establish a single-parent family.[19]

The law, as originally written, did not place any restrictions on the age of women, or the number of attempts that could be made. This is in stark contrast to most IVF coverage policies, which usually entitles beneficiaries to a certain number of *treatments*, rather than a certain number of children. Only in 1998, following the report of a professional committee, the funding was limited to women until the age of 45 for women using their own rather than donor eggs. When using donor eggs the age limit was placed at 51.[20] The same committee also suggested restricting the number of treatment cycles each woman can get public funding for, however this suggestion was not adopted. Any attempts made in the following years to abridge or condition coverage were instantly blocked by vigorous public protest, backed by the aforementioned court rulings (Shalev and Gooldin, 2006).[21]

The Israeli policy change was introduced at a time of rapid improvement in assisted reproduction technology which led to greater use, success, and media coverage worldwide.[22] Starting in the early nineties the media was flooded with IVF success stories, the most eye-catching being extreme advanced maternal age cases.[23] In Israeli press, local success stories were celebrated as "national accomplishments and symbols of local scientific excellence" (Birenbaum-Carmeli, 2004). This was topped off by the considerable amount of attention devoted by the press to the introduction of the NHI law, and within it the highly debated IVF funding issue.[24]

Personal experience with relatives or friends having successful older-age births coupled with media coverage of the new fertility possibilities and of extreme cases of older parenting appear to have updated Israelis' beliefs regarding women's fertility horizons, even yielding on average an over estimation of later life fertility success rates. A study examining Israeli students' knowledge regarding age-dependent fertility decline, found a significant overestimation of the likelihood of pregnancy, especially for women over 40, with or without the aid of IVF (Hashiloni-Dolev et al.,

[19] In practice, public funding covers approximately 85% of total treatment costs. Private and complementary health insurance programs of the health plans offer additional coverage, and also cover treatments for third and fourth child.

[20] In 2010 the age limit was raised to 54, however this is beyond the time scope of our research.

[21] In 2014 a limitation on number of treatment cycles was placed for the first time, but it applies only to women over 42 and when 3 consecutive cycles using the woman's own eggs did not reach the embryo transfer stage and only if a committee of experts suggests that it will be useless to continue.

[22] The most eminent improvement was the ICSI technology (intra-cytoplasmic sperm injection) first introduced in 1992.

[23] For example, "World record: woman aged 60 gave birth to girl, Yedioth Aharonoth 22.2.94"; After 44 failed test-tube fertilizations, a 60-year-old woman gave birth to a baby girl in 1994.

[24] The Ministry of Health expressed its intent to limit coverage to seven treatment cycles and provoked public protest. The press covered this conflict using personal stories of women over 40 that had children only following dozens of IVF treatment cycles and others who are still trying after a number of failures (Birenbaum-Carmeli, 2004)

2011).[25] In a different study, one third of the students that participated marked as "correct" statements declaring that healthy women over 45 have good chances of naturally conceiving and that the birth of a first child could be delayed till a woman turns 43 (Haimov-Kochman R and A, 2012).

The new and unique Israeli funding policy facilitated fast adoption and increased usage of the new fertility possibilities. The three forces of improved access, publicity, and technology improvement reinforced each other, leading to an IVF boom in Israel. The large amount of press coverage led to greater awareness and trust of the new technology which due to the generous funding policy translated into more actual usage. In turn, the increased demand for fertility treatments promoted the establishing of new fertility clinics and raised incentives to invest in training, research and development. These led to more frequent positive outcomes of assisted reproduction, which were also covered in the press. This course of events drove a rapid and ongoing change in Israelis' attitudes and perceptions regarding IVF success rates, causing the aforementioned sharp increase in IVF usage starting at 1994, the year the NHI law was approved.

In the following years additional rules and regulations were enacted, standardizing practices surrounding IVF and their funding. This legal progress, considered innovative and liberal in a global prospective, supported the ongoing expansion of IVF usage.[26] Nowadays, there are 26 IVF clinics spread throughout Israel. Most public hospitals have an IVF unit, making treatment very easily accessible for most residents of Israel. Israel is the world leader in the rate of IVF treatment cycles and in the percentage of babies born following IVF treatments: approximately 4% of all babies born in Israel are conceived using IVF (Hashiloni-Dolev, 2013).[27] This makes Israel an ideal setting for the study of the impact of extended reproductive time horizons offered by IVF technology on women's decisions and outcomes.

3.2 Data and Empirical Approach

Our data comes from the 2008 Israeli population census. The census is an approximately 20% sample of Israeli households. It is based on data from administrative sources integrated with sample data gathered in surveys.[28]

We present most of the analysis that follow for Israeli-born Jews, given that other popula-

[25] The study compared the students' estimations to medical data. The survey was taken by participants at 2009. Of course, since we don't have similar studies conducted prior to 1994, we can't conclude how perceptions changed. Similar studies in other countries also found overestimation of conception probability but not necessarily for the over 40 age group. In addition, those studies did not specifically target IVF success rates (see for example Bretherick et al. (2010)).

[26] The most distinct example is the 1996 Embryonic Carrying Agreement Law, officially legalizing and regulating surrogacy for the first time in the world (Simonstein, 2010).

[27] Compared to approximately 1-2% of the children are born in this way in other countries wealthy enough for women to afford IVF treatment.

[28] The survey began at the end of 2008 and was concluded in July, 2009.

tion groups may have responded differently to the changes in IVF policy (Remennick, 2010).[29] In addition, there are significant differences between native born and immigrants in culture and norms surrounding family and gender issues (see for example Danziger and Neuman (1999)). It is important to note that intermarriages between Jews and other religions are extremely rare in Israel. Moreover, in our sample and relevant period, percentage of marriages between native born and immigrants is very low[30]. Nevertheless, our results are robust to using an expanded sample, including all population groups (shown in Tables A11 and A12).

Because Arab-Israelis are less likely to use IVF (and also less likely to be on the margin of time-costly educational investments, given baseline education rates), we also use this population to perform placebo tests of male-female differences and as a second control group, comparing Jewish women to Arab women.

Table 1 shows summary statistics for the women in our sample. Note that we limit most of our analysis to specific cohorts so these figures are presented only to provide a general description of the complete sample and of how our matched sample characteristics compare to the regular sample.

We start by testing the impact of the 1994 policy change on women's decisions including marriage age, college attainment, and graduate degree attainment. We then further examine the impact on the marriage market equilibrium, by looking at the effect on spousal quality for women who marry when older. Our empirical approach for education outcomes and age at first marriage is to compare women's outcomes before and after the policy change using men as a comparison group. Panel A of each analysis shows a "naive" difference-in-differences specification, that measures the average difference between the "before" and "after" time period, according to the following equation:

$$Education = \beta_0 + \beta_1 fem + \beta_2 post + \beta_3 fem \times post + \beta_4 time + u$$

Panel B adds group-specific time trends to the difference-in-differences specification in Panel A. Note, however, that in some cases this may be a mis-specification, as for some outcomes the time trends are *not* differential pre-"treatment," but are differential post treatment.

Thus, Panel C tests both for a change in levels at the time of the policy change and a change in the time-trend of the outcome variable, allowing us to examine the evolution of the effect over

[29]This difference is partially attributed to religious beliefs, as Muslim religion does not support all ART practices (especially surrogacy and ova donations) and the Roman Catholic church bans all types of ART(Birenbaum-Carmeli, 2003). However, at least some of the documented difference should be attributed to other sources since country of origin also affects behavior in this context.

[30]This is partially due to a large incoming flow of immigrants from the Former Soviet Union during the nineties which allowed this population to form segregated communities, with institutions designed to retain distinct cultural characteristics of this community. This in turn induced family formation within those communities and prevent marriages to native born Jews or to immigrants from other origins.

Table 1: Summary Statistics

	All Women		
	Mean	SD	N
Age	37.80	14.40	172,762
College Plus	0.29	0.45	172,762
Highly Ed	0.09	0.28	172,762
	Women with spousal matches		
	Mean	SD	N
Age	41.3	12.3	107,136
College Plus	0.36	0.48	107,136
Highly Ed	0.11	0.32	107,136
Income 2008	84,375	85,930	89,060
Spouse's Age	44.2	12.9	107,136
Spouse's Income	166,934	160,846	91,983
Age at 1st Marriage	23.60	4.11	101,592
Married Older	0.08	0.27	101,931
Ever Gave Birth	0.89	0.31	107,023

2008 Israeli population census (20% sample), restricted to Jewish, Israeli-born women.

time:

$$Education = \beta_0 + \beta_1 fem + \beta_2 post + \beta_3 fem \times post + \beta_4 time \\ + \beta_5 post \times time + \beta_6 fem \times time + \beta_7 fem \times post \times time + u$$

For educational outcomes, we also add year-of-birth fixed effects in addition to controlling for time linearly. For age at first marriage, the specification is similar, but "time" represents year of marriage, and we add corresponding fixed effects.

Although men's and women's outcomes may follow different trends prior to the policy change, our results are robust to the inclusion of gender-specific time trends, either in Panel B for outcomes where there is a significant pre-trend present, or in Panel C, where the differential trend appears only post-policy change.[31]

To examine the impact on marriage market matching, we compare husbands' income for marriages of women over age 30 that took place before and after the policy change to those of women who got married under age 30, over the same time period. First without differential and period-

[31]In addition we use event studies to make sure that there is actually a change in trend for women and that the results we see are not only due to different overall gender-specific time trends.

specific time trends in Panel A:

$$Spouse\_inc = \beta_0 + \beta_1 older + \beta_2 post + \beta_3 older \times post + \beta_4 time + u$$

And then with them in panel C:

$$\begin{aligned} Spouse\_inc = \beta_0 + \beta_1 older + \beta_2 post + \beta_3 older \times post + \beta_4 time \\ + \beta_5 post \times time + \beta_6 older \times time + \beta_7 older \times post \times time + u \end{aligned}$$

In this specification as well, time represents year of marriage, which is also added as fixed effects.

Finally, we use men as a second control group in a triple-differences specification:

$$\begin{aligned} Spouse\_inc = \beta_0 + \beta_1 older + \beta_2 post + \beta_3 older \times post \\ + \beta_4 fem + \beta_5 older \times fem + \beta_6 post \times fem + \beta_7 older \times post \times fem + u \end{aligned}$$

Because there may have been other long-term societal trends that could have divergent effects for men and women, we perform a few different types of analysis to provide further evidence that the 1994 policy change drives our results. First, we use event study graphs, charting the impact over time around the time of the policy change, to show that a pre-trend is not driving our results, but rather that the observed effects only become significant after the policy change. Secondly, we use a Quandt Likelihood Ratio (QLR) test[32] to search over all possible break dates, and show that our "treatment year" is indeed identified as the break among candidate dates. Finally, we rely on the specific combination of outcome variables to bolster the evidence that IVF access is the driver behind the changes. While there are a few other mechanisms that may have an effect on one of the outcomes we study, none of those can be expected to impact *both* women's educational and marriage decisions *and* marriage outcomes for older women. We review the most prominent alternative explanations in more detail in section 4.3.

As one final note on methodology, we use 1994 as the first year of the treatment period. As mentioned before, we expect changes in our outcome variables to occur regardless of individuals' actual usage of IVF but rather as a consequence of their updated expectations. Therefore, our "treatment" kicks in as soon as women became aware of the insurance coverage rather than when coverage was actually implemented. Due to the large amount of press surrounding the approval of the law in 1994, we find it to be the turning point in the gradual process of perceptual change regarding female later life fertility, although the law only came into effect in the beginning of 1995. We use 1994 as our cutoff for outcomes that are measured over years. For outcomes that are measured by cohort, the first treated cohort is the one that was at the relevant age for educational

[32]See Andrews (1993).

decisions, at the time they learned about increased access to IVF *later in life*. Accordingly, we set a 30 year study period, from 1978 to 2008 for marriage decisions and outcomes, and birth cohorts from 1950 to 1980 for education decisions. These periods were chosen so that the before and after treatment periods are as similar as possible and so our individuals are mature enough at the census year 2008 to make education and marriage decisions.[33] Nevertheless, our results are robust to using longer or shorter time frames.

4 Results

4.1 Women's Decisions

We first examine the impact of the policy on women's decisions regarding education and marriage timing. As noted, above, we do this using a difference-in-differences framework, where women's outcomes before and after the policy change are compared to men's outcomes before and after. We present three specifications, each with and without year fixed effects: Panel A shows a basic difference-in-differences specification, Panel B adds group-specific time trends, and Panel C allows for the time trend to vary across the threshold.

Age at First Marriage The first outcome we examine is women's marriage timing. If women indeed feel more confident about their reproductive prospects later in life, they may be more willing to delay marriage, which could be a mediating mechanisms allowing greater rates of college and graduate education. Here, time represents the year the marriages are taking place, and each data point is a single marriage with an attached age for the woman or man. Thus, the treatment year is 1994, exactly the year of the policy change. This decision can be shown by year of marriage, using retrospective analysis from the 2008 census. Figure 4 shows that women's marriage age was practically constant relative to men's until 1994, implying parallel pre-trends for men and women. Starting 1994, women's marriage age begins to rise sharply, compared to men's. This is confirmed by A3 that presents the average age ate first marriage for men and women separately.

Since the new policy mainly affected expectations for women's fertility over the age of 35 (when female fertility starts to decline steeply), we expect it not only to change the average age at first marriage, but also to shift the distribution of first marriage age in a specific way. Figure 6 indicates that the percentage of women getting married at their early twenties decreased significantly during the eighties and early nineties, prior to the policy change, whereas the percentage of women getting married in their late twenties and early thirties decreased rapidly in the decade following the policy change. Since the percentage of women ever married in their forties remains practically unchanged, we can conclude that following the policy, women delayed marriage, but did not forego it. This

[33] As we explain when discussing education outcomes, we also limit our youngest cohort to be old enough to complete college, in order to minimize censoring.

Figure 4: Difference Female vs. Male Age at First Marriage

Notes: The figure presents the difference in average age-at-first-marriage between women and men, as well as fitted lines for the pre and post periods. The 'F-stat' is for the Panel C specification (table 2), on the null-hypothesis that both coefficients on the interaction terms of interest (fem×post and fem×post×time) are equal to zero, i.e. the policy has no gender differential effect. The value of the F-stat confirms that the change is statistically significant.

course of change fits our hypothesis that the new fertility horizons allowed women to delay marriages to their thirties, which delays childbearing even further (to late thirties and early forties).

Table 2 analyzes this change using a regression, in both a simple difference-in-differences framework, and an analysis demonstrating the change in the time trend. The latter indicates a gradual change in the outcome, which correlates with the gradual change in perceptions, rather than a one-time jump. We find that women marry about a third of a year older on average, relative to men, after the policy change. This change appears to be principally driven by a slope shift beginning at the time of the policy change. Note, although we include the difference-in-difference specification with group-specific time trends in Panel B, in this case we believe this to be a mis-specification, since the time trends are largely parallel before the change, and thus, without allowing the time trend to break at 1994, the group-specific time trend picks up the differential time trends after the change. This is demonstrated in panel C, where the group-specific pre-trends are small and not significant.

Table 2: Age at First Marriage

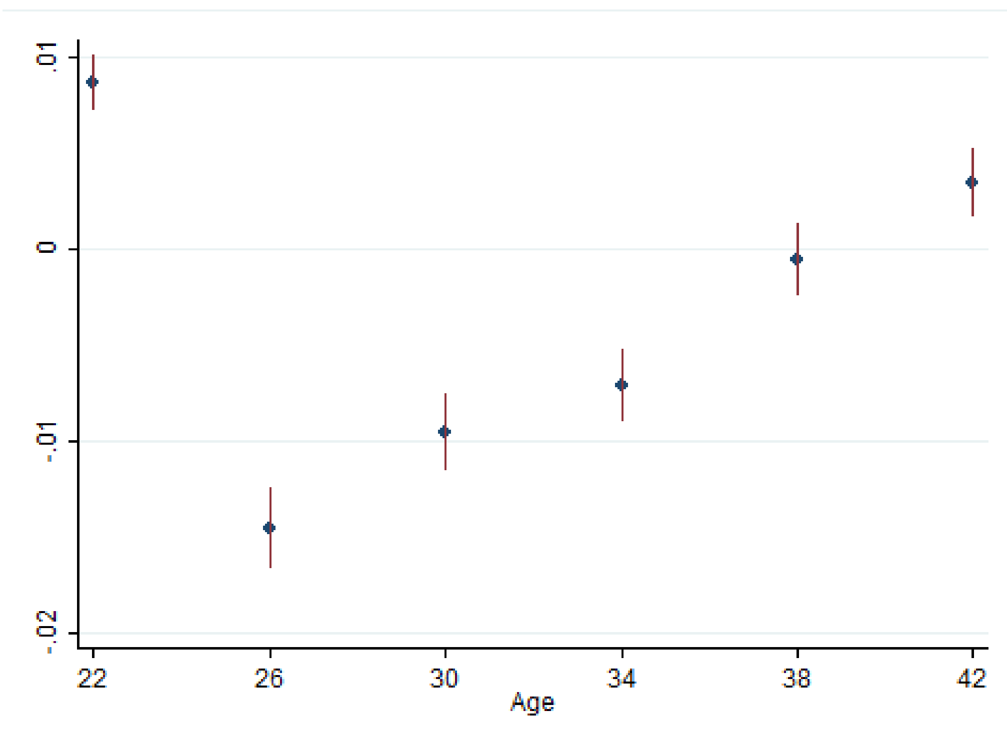
	Dependent Variable: Age at First Marriage					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
fem × post × time					0.0301**	0.0308**
					(0.00979)	(0.00980)
fem × post	0.342***	0.345***	0.115	0.111	0.0967	0.0976
	(0.0494)	(0.0496)	(0.0983)	(0.0990)	(0.0857)	(0.0860)
fem × time			0.0152*	0.0156*	0.000773	0.000376
			(0.00593)	(0.00602)	(0.00784)	(0.00784)
post × time					0.00811	0.00655
					(0.0130)	(0.00696)
post	-0.385**		-0.268*		-0.270*	
	(0.121)		(0.126)		(0.122)	
female	-2.686***	-2.687***	-2.577***	-2.576***	-2.682***	-2.685***
	(0.0283)	(0.0284)	(0.0547)	(0.0551)	(0.0730)	(0.0734)
time	0.137***	0.131***	0.130***	0.128***	0.126***	0.126***
	(0.00570)	(0.000899)	(0.00654)	(0.00168)	(0.0107)	(0.00412)
Constant	26.83***	26.62***	26.77***	26.63***	26.74***	26.54***
	(0.0740)	(0.0129)	(0.0732)	(0.0114)	(0.0965)	(0.0382)
Fixed Effects		YES		YES		YES
Observation	173750	173750	173750	173750	173750	173750
R-Squared	0.136	0.137	0.136	0.137	0.136	0.137

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

To understand how the distribution of marriage age was affected, rather than just the average, we run a series of regressions using the Panel C specification, but replacing the outcome variable with an indicator for being married by a certain age. Figure 5 shows the point estimates and confidence intervals for the coefficients on the interaction term fem×post×time, in each separate regression. The values of these coefficients suggest that the policy had no effect on the likelihood of marrying by age 22. This provides a useful falsification test, since we would not expect women inclined to marry and begin childbearing by age 22 to be concerned about fertility in their late thirties and hence affected by access to IVF. We see the largest reduction in marriage by age 26, and from there a steadily decreasing coefficient, until the effect reaches zero at age 38. The lack of reduction in marriage by age 38 suggests that women are delaying marriage, but not forgoing it entirely. Overall, this analysis suggests that the decrease in average marriage age after the policy change, is mostly driven women delaying marriages from their mid-twenties into their thirties and even late thirties.

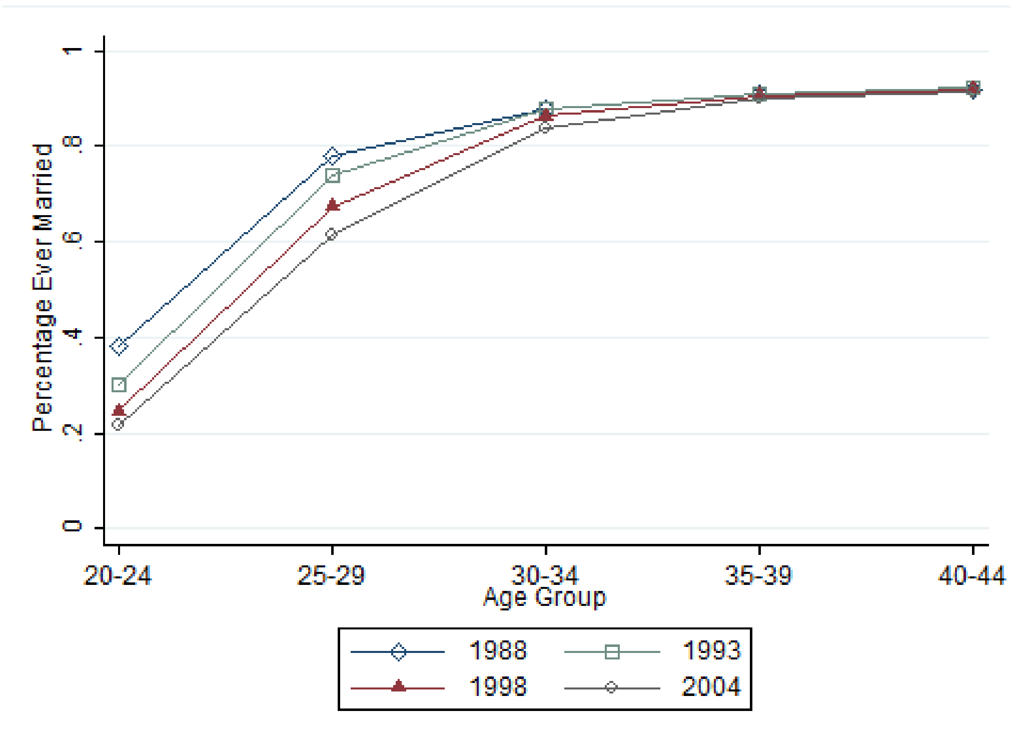
Figure 5: Married by Age Coefficients



Notes: The figure presents the point estimates and confidence intervals of the coefficient on the interaction term fem×post×time, for regressions where the outcome is a binary variable indicating whether or not the individual got married at or before a certain age, and the specification is as in column (5) in table 2. The ages we use for the outcome variable are 22 to 42 with 4 year intervals.

To show these results another way, we look at 4 different points in time, before and after the change, and divide women into age groups according to their age at that time. Then, we measure the percentage of women in each age group who have already been married at least once (note that we don't look at their marital status but rather if they ever got married). Figure 6 shows that while the percentage married before young ages has been steadily falling over time, for the 20 to 24 age group the largest decline occurs before the policy change. However, for the 25 to 29 age group the reduction in the fraction of women that were ever married is clearly larger after 1994, i.e. between 1993 and 1998, and on to 2003. For the same years, a small but apparent change is displayed for women in their early thirties, but no change to marriage rates by the late thirties/early forties.

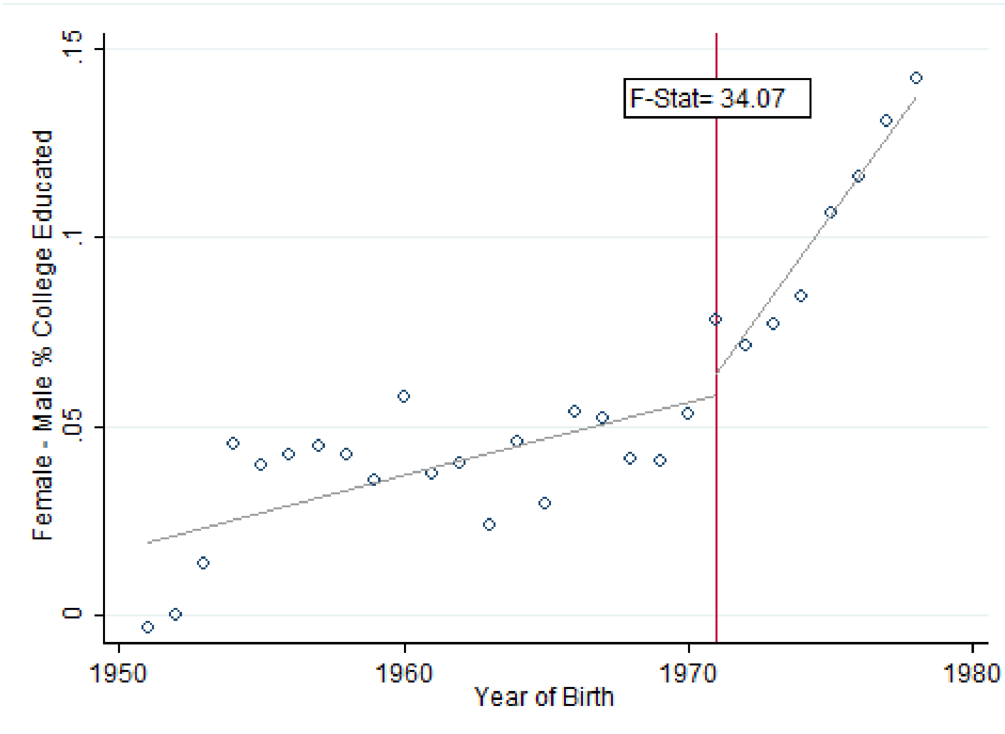
Figure 6: Percent Married by Age and Year



Notes: The figure presents the fraction of women ever married in each specific age group, at different points in time. These fractions are calculated by looking at the age of the woman at a chosen year and whether the year she first got married in precedes the chosen year or not. We use 5 year intervals, with 1993 as an 'anchor', being the last year pre-change.

College Education We then turn to women's educational investments. Figure 7 shows the raw data used in this analysis, charting women's college completion compared to men, by year of birth. Because the median age for college entry for women in Israel is 22.5, we use the cohort born in 1971 as the first treatment year, as they would have been 23 and thus still able to be influenced in completing college by the change to their reproductive time horizon. Most women born in earlier cohorts would have already been past the age of making decisions about college completion at the time of the policy change.[34]

Figure 7: Difference in Percentage of College Educated Female and Male



Notes: The figure presents the difference in average college completion rates between women and men, as well as fitted lines for the pre and post periods. The 'F-stat' is for the Panel C specification (table 3), on the null-hypothesis that both coefficients on the interaction terms of interest (fem×post and fem×post×time) are equal to zero, i.e. the policy has no gender differential effect. The value of the F-stat confirms that the change is statistically significant.

As Figure 7 clearly shows, there is a sharp increase in women's college completion rates relative to men beginning with the first treated cohort. However, women's relative college attainment appears to also increase for cohorts that are mostly unaffected by the change. This implies that the pre trend for each group is different, as confirmed by figure A3 which presents the rate of college completion by cohort, separately for men and women. Therefore, it is crucial to confirm that controlling for group specific time trends does not eliminate the effect (Panel B and C specifications).

These results are presented formally in a regression in table 3. Panel A shows a difference-in-differences specification, with men as the control group. The coefficient on the interaction with being female and of college-entering-age after the year of the policy change is positive and significant. This effect remains stable when year of birth fixed effects are introduced. Panel B demonstrates that this effect is robust to including gender-specific time trends, although the magnitude of the effect is smaller. Because in this case pre-trends are not parallel, Panel A is misspecified and the

[34]Men's median age for college entry is 24. Because men enter college slightly later, we experiment with shifting the treatment year for men as one of our robustness checks, discussed below.

coefficient appears to be positively biased. Nevertheless, the increase in women's college education is marginally significant and is on average three percentage points. When discontinuous time trends are allowed for in Panel C, we see that the effect is driven by an increased slope for the rate of women completing college. Rates of college completion have been increasing over time, but they begin to increase much more steeply following the policy change, at a time when men's entry rates appear to slightly decrease.[35]

Because the outcome is a binary variable for whether the respondent has completed college, we also run these same regressions using logit and probit specifications, which appear in the appendix, in Table A1. We also perform a placebo test, to demonstrate that we are picking up a real effect of the policy, by demonstrating that there is no impact on high school education in either the birth cohort most affected for college education, 1971, or the birth cohort finishing high school in 1994, 1978. These results are shown in Figure A4.

Graduate Education We next examine whether more women completed graduate education following the policy change. For this outcome measure, we again use the median age of students entering that educational level to guide us, treating the 1966 cohort as the first treated year.[36] The raw data is shown in Figure 8, showing again a clear increase in women's completion relative to men starting in the younger birth cohorts, who have not completed their educational decisions before they learn of expanded access to IVF. For the cohorts that were not affected by the policy the gap between men and women bounces around a lot but eventually remains almost constant. Figure A3 presents rates of graduate education for women and men separately.

Table 4 tests formally whether women are more likely to complete graduate degrees following the expansion. We again find, in a differences-in-differences framework, presented in Panel A, that women are significantly more likely to complete graduate degrees following the expansion than before. Panel B allows for a group-specific time trend, but not for this time trend to evolve over time. Because there is no differential pre-trend in the data, as shown in Figure 8, Panel C is a more appropriate specification. When allowing for a discontinuous slope change, in Panel C, the main effect diminishes, and we find that it is driven by an increase in the rate of women completing advanced degrees. Once again, this shows that the impact evolves over time, but the increase starts in 1994, rather than being driven by a female-specific pre-existing trend.

We also look at whether rates of women gaining graduate education *conditional* on obtaining college education have increased, in table 5. Doing this seeks to understand whether graduate education has increased as a natural consequence of the increase in college education, or whether there has been an increase in graduate education over and above the mechanical impact of increasing

[35]This apparent decrease is probably the result of censoring for the youngest cohorts, as some men (and women) may have not completed college by the time of the survey. We conduct several additional tests and analyses to verify that censoring is not what drives our results, as elaborated in the following sections.

[36]The median age for second degree applicants in Israel is 28.2 for women and 29.7 for men.

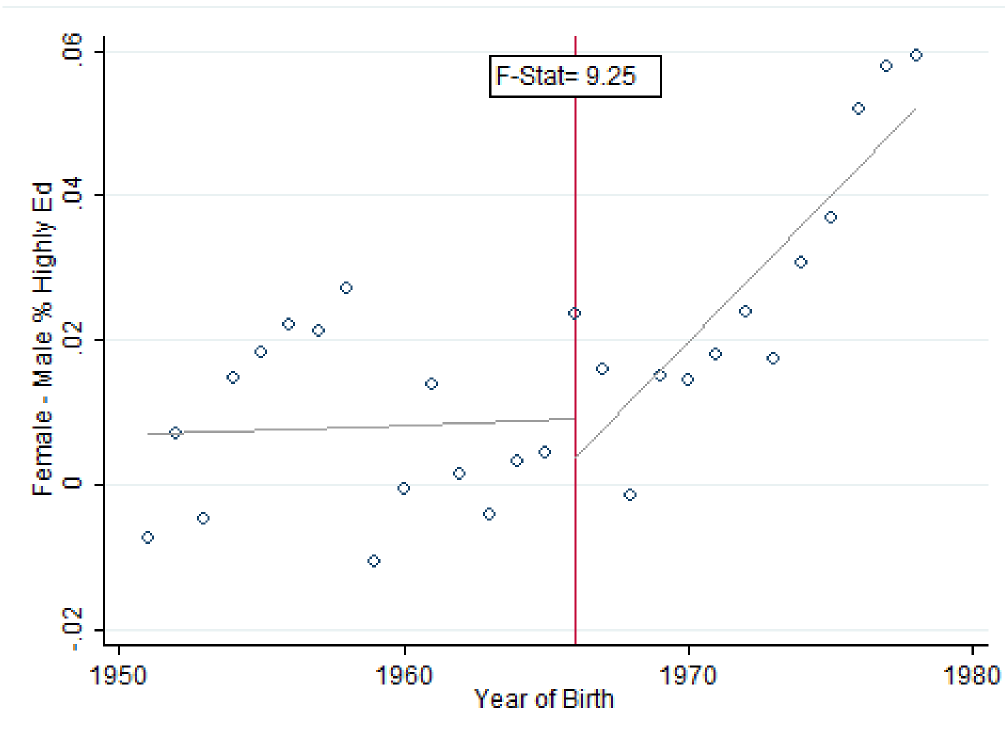
Table 3: College Graduation Rates

	Dependent Variable: College Education					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
fem × post × time					0.00836***	0.00838***
					(0.00148)	(0.00147)
fem × post	0.0767***	0.0768***	0.0291*	0.0285*	0.00889	0.00853
	(0.0105)	(0.0106)	(0.0111)	(0.0113)	(0.00959)	(0.00956)
fem × time			0.00331***	0.00335***	0.00202***	0.00205***
			(0.000655)	(0.000659)	(0.000469)	(0.000458)
post × time					-0.00937**	-0.00985***
					(0.00278)	(0.000558)
post	0.00449		0.0282		0.0508**	
	(0.0128)		(0.0159)		(0.0148)	
female	0.0326***	0.0330***	0.0650***	0.0659***	0.0526***	0.0532***
	(0.00394)	(0.00390)	(0.00756)	(0.00768)	(0.00485)	(0.00483)
time	0.00429***	0.00240***	0.00264*	0.00155***	0.00408***	0.00429***
	(0.000775)	(0.000186)	(0.00104)	(0.000233)	(0.000781)	(0.000229)
Constant	0.324***	0.295***	0.308***	0.295***	0.322***	0.343***
	(0.00928)	(0.00351)	(0.0127)	(0.00252)	(0.00879)	(0.00238)
Fixed Effects		YES		YES		YES
Observation	206921	206921	206921	206921	206921	206921
R-Squared	0.0202	0.0214	0.0204	0.0217	0.0210	0.0220

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 8: Difference in Percentage of Highly Educated Female and Male



Notes: The figure presents the difference in average graduate education completion rates between women and men, as well as fitted lines for the pre and post periods. The 'F-stat' is for the Panel C specification (table 4), on the null-hypothesis that both coefficients on the interaction terms of interest (fem×post and fem×post×time) are equal to zero, i.e. the policy has no gender differential effect. The value of the F-stat confirms that the change is statistically significant.

Table 4: Rates of Graduate Education

	Dependent Variable: Graduate Education					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
fem × post × time					0.00379***	0.00390***
					(0.00100)	(0.00101)
fem × post	0.0265***	0.0267***	-0.000391	-0.000508	0.00565	0.00512
	(0.00571)	(0.00573)	(0.0109)	(0.0111)	(0.00751)	(0.00779)
fem × time			0.00176**	0.00178**	-0.000511	-0.000511
			(0.000608)	(0.000608)	(0.000565)	(0.000564)
post × time					-0.00730***	-0.00763***
					(0.00137)	(0.000651)
post	0.0267		0.0401*		0.0292*	
	(0.0150)		(0.0176)		(0.0115)	
female	0.00408	0.00422	0.0177*	0.0180*	0.000247	0.000277
	(0.00315)	(0.00310)	(0.00693)	(0.00694)	(0.00458)	(0.00458)
time	-0.00238*	-0.00248***	-0.00326**	-0.00290***	0.00106*	0.000889**
	(0.00106)	(0.000100)	(0.00115)	(0.000168)	(0.000487)	(0.000287)
Constant	0.0890***	0.0690***	0.0822***	0.0692***	0.115***	0.118***
	(0.0100)	(0.00147)	(0.0114)	(0.00135)	(0.00401)	(0.00319)
Fixed Effects		YES		YES		YES
Observation	206921	206921	206921	206921	206921	206921
R-Squared	0.00250	0.00567	0.00264	0.00581	0.00409	0.00598

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

the pool of college graduates. Once again, the difference-in-difference specification finds positive and significant effects. But, more surprisingly, in Panel C, we find that rates of graduate education conditional on college education had been declining for women before the policy change, but are boosted following the policy change. Moreover, the magnitude of the effect is larger, even in percentage points, than the effect we see on college graduation. This supports our main hypothesis that these changes are driven by the extended later-life fertility for women, as decisions on graduate education are made at an older age when expected fertility plays a much more important role.

Table 5: Conditional Rates of Graduate Education

	Dependent Variable: Graduate Education | College					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
fem × post × time					0.0111***	0.0119***
					(0.00222)	(0.00224)
fem × post	0.0354*	0.0399*	0.00832	-0.00333	0.0264	0.0222
	(0.0152)	(0.0153)	(0.0296)	(0.0299)	(0.0204)	(0.0212)
fem × time			0.00177	0.00283	-0.00484**	-0.00488**
			(0.00164)	(0.00156)	(0.00144)	(0.00145)
post × time					-0.0224***	-0.0209***
					(0.00266)	(0.00160)
post	0.0563		0.0709		0.0266	
	(0.0386)		(0.0471)		(0.0234)	
female	-0.0219	-0.0230	-0.00851	-0.00160	-0.0596***	-0.0598***
	(0.0117)	(0.0116)	(0.0192)	(0.0194)	(0.0145)	(0.0146)
time	-0.0114***	-0.0103***	-0.0124***	-0.0110***	0.00176	-0.000558
	(0.00240)	(0.000293)	(0.00290)	(0.000481)	(0.00129)	(0.000754)
Constant	0.317***	0.268***	0.310***	0.268***	0.417***	0.396***
	(0.0268)	(0.00426)	(0.0309)	(0.00423)	(0.0120)	(0.00849)
Fixed Effects		YES		YES		YES
Observation	71858	71858	71858	71858	71858	71858
R-Squared	0.0203	0.0278	0.0204	0.0279	0.0258	0.0285

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Because college and graduate education are binary outcome variables, we also present logit and profit specifications in Table A1. These alternative specifications confirm our main results.

College education by percent of cohort affected Because not all students enter college at the same age, it may be artificial to pinpoint one birth cohort as being “treated” while the others are not. For this reason, in Table 6, we present a regression that classifies a portion of each cohort as treated based on which percentage of individuals would have not yet entered college, according to data from the 1995 Census on college entry ages. These percentages are allowed to be different for men and women, which accounts for the fact that men may enter college later. These results again show a significant impact of being in the "treated" cohorts.

Table 6: College Education by Percent of Cohort Treated

	Dependent Variable: College Education					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
fem × treated × time					0.00535*	0.0105***
					(0.00206)	(0.000966)
fem × treated	0.0919***	0.0970***	0.00998	0.0413*	-0.0104	-0.0300*
	(0.00978)	(0.0107)	(0.0227)	(0.0181)	(0.0204)	(0.0121)
fem × time			0.00441***	0.00299**	0.00394***	0.00315***
			(0.00118)	(0.000892)	(0.000726)	(0.000675)
treated × time					-0.00975***	-0.0394*
					(0.00210)	(0.0155)
cohort treated	0.0261	-0.00637	0.0725**	0.0502	0.127***	-0.0294
	(0.0153)	(0.0315)	(0.0207)	(0.0383)	(0.0208)	(0.0293)
female	0.0271***	0.0239***	0.0803***	0.0611***	0.0807***	0.0715***
	(0.00413)	(0.00390)	(0.0164)	(0.0121)	(0.0105)	(0.00896)
time	0.00314**	0.00227	0.000643	-0.000223	0.000839	0.0143**
	(0.000877)	(0.00114)	(0.00126)	(0.00146)	(0.000785)	(0.00467)
Constant	0.309***	0.297***	0.278***	0.258***	0.270***	0.544***
	(0.0116)	(0.0233)	(0.0165)	(0.0275)	(0.0108)	(0.0941)
Fixed Effects		YES		YES		YES
Observation	206921	206921	206921	206921	206921	206921
R-Squared	0.0203	0.0217	0.0205	0.0218	0.0213	0.0220

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Reevaluating Education Outcomes Using Longitudinal Data With all educational outcomes, one may be concerned about data censoring since we use data collected in a single year and therefore compare individuals of different ages, some of whom may not have completed their educational attainment. To minimize this problem, the youngest cohort we use in our estimation is 28 at the census year. Moreover, we use a second data set to verify that censoring is not what drives the result. The data we use in Figure 9 and Figure 10 is from the Israeli Annual Labor Force

Survey ("LFS data"), for the years 2001 to 2011. This sample is representative of the population, but much smaller than the Census sample.

The LFS data is a longitudinal survey following the size and characteristics development of the labor force in Israel at the household level.[37] We look at two birth cohorts per survey year only, to compare between individuals in the same two-year age group before and after policy change.We use two-year age groups to increase the number of observations per year and decrease variation (although the sample is still quite small). We choose ages to be high enough so we can be confident that there is minimal censoring due to ongoing education, but also not "too old" so we can follow what happens to this age group for several years after 1994.[38]

Figure 9 presents the percentage of college graduates in each cohort, separately for men and for women on the left hand side and differences on the right. We clearly see that while men stay on the same moderately increasing time trend, women's rate of college completion sharply increases starting with the 1971 cohort, similarly to what we see in the Census data. This result refutes the possibility that the results we presented above are the result of data censoring which is more severe for men.

The same analysis is presented in figure 10 for graduate level education. Interestingly, there are two 'jumps' for women, the first for the 1966 cohort and the second for the 1971 cohort. It seems reasonable that the first increase is driven by women who already completed college when the policy was introduced and due to the policy faced a decreased cost of attending graduate education. The second increase correlates with the increase in college attainment and is at least partially driven by the higher rates of women who are college graduates and can actually consider post college education.

As a final robustness check to rule out censoring as the driver of our results, we also present our main regression specifications with a shorter post period, in Tables A2 and A3. Although the decreased data compromises the significance of the results, they are still positive and consistent with our main results.

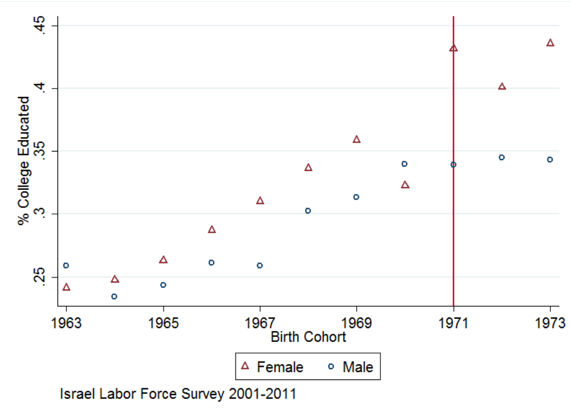
Quandt Likelihood Ratio breakpoint test To confirm that what we are picking up is truly a discontinuous shift in these outcomes—a break in the time series—rather than more gradual time

[37]Due to the nature of this data, households may have been surveyed multiple times within each survey year. In order to identify duplicate observations within each survey year we use the following methodology. First, we exclude any observations that have the same ID, cohort, and gender within the same year. Using this method, we identify 962,992 out of 1,586,133 as unique observations (61.75% of the data with ID variable). Secondly, the data states that there are 28,033 missing observations for the ID variable between 1970-1978. To address this issue we use nine "static" variables that should not vary or will have limited variation within the year: locality type, father's country of birth, mother's country of birth, district or sub-district of residence, type sample (new houses, kibbutizim, moshavim and village etc.), interview panel, gender, year of birth, and survey year. Using this second methodology, we are able to identify 14,180 out of 28,033 as unique observations (50.58% of the data without ID variables). As such, we identify a total of 976,322 unique observations from 1970 to 2011.

[38]The last survey year that is available is 2011. Individuals that are 38 or 39 in 2011 were born in 1973 and 1972, respectively. Note that our treated cohorts start at 1971 for college and 1966 for graduate education.

Figure 9: LFS Percentage of College Educated Female and Male, For 38-39 Year-Old Cohort

(a) Separate



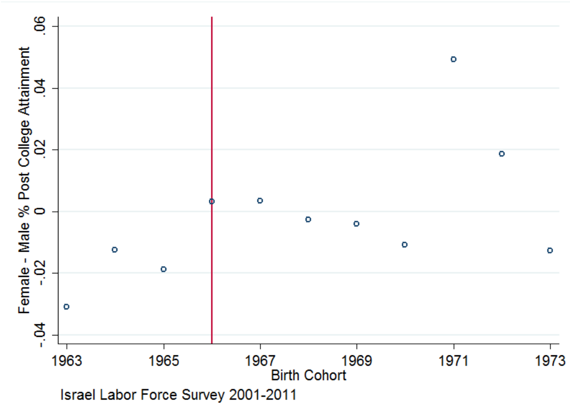
(b) Difference

Notes: Figure (a) presents the fraction of men and women with college education by year of birth. Figure (b) presents the difference between women and men. Data from the Labor Force Survey 2001-2011. The sample was limited to Israeli born Jews and treated to eliminate duplicate observations (due to the nature of the survey).

Figure 10: LFS Percentage of Graduate Educated Female and Male, For 38-39 Year-Old Cohort

(a) Separate

(b) Difference



Notes: Figure (a) presents the fraction of men and women with graduate education by year of birth. Figure (b) presents the difference between women and men. Data from the Labor Force Survey 2001-2011. The sample was limited to Israeli born Jews and treated to eliminate duplicate observations (due to the nature of the survey).

trends, we perform and Quandt Likelihood Test to "search" for the most likely break year in the data, over our entire sample period except for 15% "trimming" on either end, to account for limited data at the beginning and end of the sample period. We do this for college education and for age at first marriage, since the break should occur for different years, since one is based on year of birth corresponding to entering college cohort, while the other is based on year of marriage. To implement the test, we run a loop of regressions identical in specification to our panel B, column 1 regressions, except the "break" year changes in each regression. We then perform an F-test for whether the two "break" parameters—slope and intercept—are different from zero. Finally, we search for the maximal F-stat among these tests. The test for college education returns 1971 as the maximal F-stat, exactly the year when students would have been "treated" by the policy change at a time when it could still impact their college completion. The test for age at first marriage returns 1994, the year of the policy change, and our treatment year. The procedure for the QLR specifies comparing this "sup F-stat" to a table of critical values adjusted for the number of tests–the critical value for two restrictions and 15% trimming is 5.86, whereas the QLR statistic for age at first marriage for the "break" year is 7.68, and the statistic for college education is 24.8, as shown in Table 7.

4.2 Marriage Market Equilibrium

The additional reproductive years afforded by access to assisted reproduction technologies may have impacted not only women's decisions, but also men's marriage choices. Low (2014) shows that men respond to prospective mates' expected fertility when choosing a partner, trading off between so-called "reproductive capital" and more traditional human capital traits like income and education. As a result, women who are high-earning, but older, may marry poorer men than lower-earning, but younger, women. Because the increase in access to IVF technology lessens the perceived fertility cost of waiting to marry, "high-quality" men may have been more willing to marry older women following the policy change. If this is the case, we can expect equilibrium matching to adjust so that these women will match with higher quality partners.

We test this by examining the spousal quality of women who marry older versus younger before and after the policy change. If women's reproductive fitness is taken into account by men, we would expect the "spousal quality penalty" to older women to lessen once access to IVF expands.

We use a sample of women who are between 25 and 34 at marriage, and compare spousal quality, measured in a variety of ways, for women who were between 25 and 29 at the time of marriage versus women who were between 30 and 34 at the time of marriage. In practice we cannot use only first marriages for this analysis, since we only have spousal income data for the current spouse. This also means that we are excluding data from women who are divorced or widowed before the census year, which may become more likely as the year of marriage becomes longer ago. However,

Table 7: Quandt Likelihood Ratio test for break point

Age at first marriage		College education	
Year of Marriage	F-stat	Year of Birth	F-stat
1982	6.74	1954	0.41
1983	6.9	1955	2.92
1984	6.99	1956	4.08
1985	7.09	1957	5.9
1986	6.38	1958	10.1
1987	6.26	1959	11.9
1988	6.36	1960	11.5
1989	6.31	1961	19.4
1990	6.26	1962	19.9
1991	6.02	1963	20.4
1992	5.8	1964	19.3
1993	6.41	1965	22.1
1994	**7.68**	1966	20.8
1995	5.38	1967	23.2
1996	4.92	1968	23.3
1997	4.01	1969	24.5
1998	4.39	1970	24.6
1999	2.82	**1971**	**24.8**
2000	2.99	1972	23.9
2001	2.63	1973	23.9
2002	2.84	1974	23.8
2003	2.13	1975	23.6

Quandt-Likelihood ratio test using difference-in-differences regression specification as in all panel B, column 1, with break point at given year. The QLR critical value at 5% significance with 15% trimming and two restrictions (the slope and intercept break) is 5.86

this affects only a small number of observations.

Our main proxy for spousal quality is husband's income, as it is well established that income is an important quality that male spouses bring to the relationship (see, for example, Fisman et al. (2006)). However, we verify our results using other measures and the results are presented in the appendix. The raw data for this analysis is shown in Figure 11, showing again a discontinuous increase in spousal income for women who marry when older, compared to women who marry when young.

Figure 11: Husband's Income: Wife's Age at Marriage 30-34 vs. 25-29

Notes: The figure presents the difference in average raw spousal income between women who got married at the age range 30-34 and women who got married at the age range 25-29, as well as fitted lines for the pre and post periods. The 'F-stat' is for the Panel C specification (table A8), on the null-hypothesis that both coefficients on the interaction terms of interest (older×post and older×post×time) are equal to zero, i.e. the policy has no differential effect on the spousal income of women who marry older. The value of the F-stat confirms that the change is statistically significant.

The "V"-shape apparent in this outcome measure means that the specifications in Panel C are most likely to be able to pick up the correct effect. The initially decreasing trend in spousal income for women who marry older may be produced by macroeconomic changes that affect the relative salary of men in different age groups. For this reason, we attempt to carefully control for men's age in a number of different specifications. First, we regress men's income on a flexible polynomial of age, to remove cohort effects, and then use these income-age-residuals as our adjusted income variable. Table 8 presents the results from a regression of these income-age-residuals on marrying while older, before and after the policy change. The results confirm that there is a significant "penalty" in terms of spousal income for women who choose to marry over thirty. Panels A and C results show that this penalty significantly decreases in the post period. Panel C suggests that spousal income for "older" brides increased following the change and kept increasing over the post period. Note, that adding year of birth fixed effects increases the magnitude of the coefficient that represents the slope change while reducing the magnitude of the "one-time" increase and decreasing the significance level below conventional standards. As for our other outcomes, the change appears to be gradually increasing over time as both women's and men's beliefs are gradually updated. Panel B does not show a significant change, however in light of the "V"-shape apparent in the graph this is not surprising and probably should be interpreted as a mis-specification.

Table 8: Income-Age Residual

	Dependent Variable: Spouse's Income (Residual of Age polynomial regression)					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
older × post × time					5000.2***	5268.9***
					(1287.9)	(1377.2)
older × post	14275.4*	15502.1*	3848.8	-1706.3	18951.5*	15857.1
	(6836.1)	(6881.7)	(9974.5)	(9814.5)	(7961.7)	(8150.5)
older × time			668.7	1106.2	-3057.6*	-2978.3*
			(667.0)	(639.9)	(1191.1)	(1298.4)
post × time					-3948.6***	-3651.8***
					(856.5)	(177.1)
post	21333.7*		23359.2*		11877.0	
	(8342.7)		(9090.6)		(6358.8)	
marriedolder	-22057.8***	-22212.8***	-17477.8*	-14657.0*	-42901.9***	-42556.6***
	(5860.8)	(5949.0)	(6686.3)	(6751.0)	(6810.6)	(7129.8)
time	-1810.6**	-163.4***	-1948.1**	-251.3**	953.8	1389.3***
	(545.9)	(44.04)	(616.7)	(76.13)	(603.5)	(122.1)
Constant	14311.5**	5042.8***	13374.7*	4817.1***	33144.1***	25391.0***
	(4984.1)	(631.9)	(5332.1)	(573.3)	(2999.2)	(584.1)
Fixed Effects		YES		YES		YES
Observation	31245	31245	31245	31245	31245	31245
R-Squared	0.00337	0.00663	0.00341	0.00675	0.00499	0.00726

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In the appendix, we use another age correction, by using "spousal income percentile" in Table

A4, where we measure the spouse's income rank compared to his birth cohort. These results are consistent with the income-age-residual specification, and significant. We additionally use specifications for husband's college and post college education (Tables A5 and A6), and husband's unobserved quality measured as by the residuals from a Mincer regression of wage on education, age and demographic controls (Table A7). These results show that women who married older received better quality spouses in multiple ways: they were better educated, but they also possessed higher unobserved earning power, even after controlling for education.

Finally, in the appendix we present results for regressions that use raw income as the outcome. Raw income with no controls is presented in Table A8, but may be confounded by cohort affects. First we include fixed effects for spouse's year of birth, to more flexibly control for cohort than in the age polynomial, in Table A10. Then, we control for women's characteristics, to demonstrate whether the increase in spousal income for "older" brides stems from these women's increased quality. If this last specification shows no significant change, it suggests that there actually might not be any change in how men evaluate older women's potential to reproduce, but rather that women's increased education or income is responsible for better marriage outcomes. However, this specification, in Table A9, yields positive and significant coefficients on the interactions of interest, suggesting that the "older" marriage penalty is actually driven by reduced reproductive capital for women and its dissipation is related to the new fertility horizons.

Because the effects on people who marry older are being compared to those who marry younger here, we are able to introduce an additional control: the comparative gap between older and younger marriage for men. We do this by using a triple-difference specification, where spousal quality metrics for older versus younger women before and after the policy change are compared to the same metrics for older versus younger *men* before and after the policy change. The results, presented in Table 9, show that not only do older women's marriage outcomes (in terms of spousal quality) improve relative to younger women following the policy change, but they also improve relative to the change in older *men*'s spousal quality.

Together, these results show that women delayed marriage and made greater educational investments after the policy change. At the same time, women who delayed marriage were penalized less on the marriage market for their older age at marriage. These profound effects are consistent with women believing the policy (and the technology that they learned about through the policy change) provided some insurance against age related infertility, altering both their decisions and the decisions of men who may have updated their beliefs regarding older women's fertility prospects. The combination of these results and effects on different outcomes strengthen our conclusion that IVF availability was responsible for each individual change.

Table 9: Income-Age Residual, Triple Difference

	Dependent Variable: Spouse's Income (Residual of Age polynomial regression)					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
older × time × post × fem					4466.6**	4360.6**
					(1321.9)	(1331.1)
older × post × fem	14802.2	15710.4*	8966.2	7584.1	17452.2	16940.4
	(7251.6)	(7133.8)	(12693.6)	(12380.1)	(9397.8)	(9342.2)
fem × post × time					-7945.1***	-7839.0***
					(1145.5)	(1161.5)
older × time × fem			605.6	787.6	-2081.4	-1988.0
			(822.9)	(804.7)	(1155.5)	(1168.0)
older × post × time					533.5	730.1
					(506.6)	(497.5)
older × fem	-20535.7**	-21366.0**	-14666.8	-14552.6	-35809.1***	-35290.1***
	(6051.1)	(5942.2)	(7641.4)	(7467.7)	(7005.9)	(6978.1)
older × time			63.14	-127.4	-976.3*	-1087.5*
			(283.3)	(272.2)	(426.9)	(411.5)
older × post	-3486.5	-4707.1	-5117.5	-4048.4	1499.2	1370.2
	(2806.9)	(2844.8)	(4809.2)	(4697.9)	(4518.7)	(4497.3)
post × time					3996.5***	4553.9***
					(392.4)	(532.7)
fem × time			-4726.4***	-4871.9***	660.7	607.0
			(1071.6)	(1056.5)	(798.8)	(806.2)
fem × post	-43487.2***	-44203.2***	26111.2	27263.8	7156.5	7072.3
	(9058.5)	(9044.6)	(14765.9)	(14631.8)	(8229.3)	(8289.2)
post	24660.4***		-2752.0		4720.5	
	(5077.2)		(7159.6)		(3078.0)	
female	112782.0***	113296.1***	79422.7***	79083.9***	117671.1***	117364.6***
	(3081.5)	(3057.9)	(9175.9)	(9108.3)	(4323.3)	(4326.8)
marriedolder	-1397.5	-433.6	-2811.0	-2824.4	-7092.8	-7536.5*
	(2190.4)	(2285.8)	(2701.0)	(2640.6)	(3484.2)	(3417.6)
time	915.9***	1565.6***	2778.3***	2417.5***	293.1	78.11
	(177.2)	(138.0)	(517.4)	(222.8)	(307.0)	(243.2)
Constant	-79896.0***	-63163.1***	-66048.0***	-61889.7***	-84527.1***	-85535.7***
	(1925.9)	(2054.8)	(4379.2)	(1297.2)	(2620.0)	(2655.2)
Fixed Effects		YES		YES		YES
Observation	75202	75202	75202	75202	75202	75202
R-Squared	0.107	0.108	0.112	0.113	0.116	0.116

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

4.3 Alternative Explanations and Robustness Checks

In this section, we perform robustness checks of our results and examine some potential alternative explanations for our findings.

Allowing for men's later college entry We first consider the possibility that men entering college later than women may create censoring in our data, where some men who may go on to complete a college degree have not yet done so at the time of the 2008 census. To allow for this, we shift data for men by one year, to account for the average lag in them entering college, compared to women (the approximate one-year lag may result due to the extra year of mandated military service for men). This aligns men and women by college entry cohort, rather than by birth year. Table 10 shows these results for college education, while Table 11 performs the same exercise for graduate education. Both tables show our results are similar, though smaller in magnitude, when accounting for this lag, and that there still appears to be a strong increase in women's education after the policy change.

Event Study analysis The next potential confounding factor we explore is that long term time trends may be responsible for the effects we see. This is already partially addressed by the inclusion of group-specific time trends in our regressions. However, to further address this possibility, we perform an event study analysis (also known as distributed lag analysis), to pinpoint the timing of the changes we observe. We do this for our main outcome measures, college education, graduate education, age at first marriage, and spousal income for women who marry when older.

We use a distributed lag model, where the effect of being at or greater than a given year is captured (rather than a dynamic lag model, where each year is captured individually). We do so because we expect a lasting impact on our outcome variables, and that this impact may strengthen over time due to information dispersal and technological change, rather than a one-period effect that dissipates. The event study graphs depicted in Figures 12 − 15 are created by regressing our key outcome variable on a series of dummies for being greater than or equal to a given year, interacted with gender or age respectively (gender for educational outcomes, age at marriage for spousal income). The coefficients graphed represent the effect of being greater than or equal the given time period, controlling for all other time periods. The coefficient on the lag just before the policy change is normalized to zero, so that subsequent effects show the relative difference in the affected groups outcomes compared to the period just before the policy change.

Figures 12 and 13 show that in the period leading up to the policy change, educational outcomes were not significantly different from immediately prior to the policy change. In other words, there was no pre-existing pre-trend. For college education, though, being born at or after 1971, the birth cohort entering college at the time of the 1994 policy change, has a statistically significant divergent effect for women. None of the other distributed lags are significant. The same is true for graduate

Table 10: College Graduation Rates, Men's Cohort Adjusted

	Dependent Variable: College Education					
	Panel A: DiD		Panel B: DiD with slopes			
	(1)	(2)	(3)	(4)	(5)	(6)
fem × post × time			0.00737***	0.00243	0.00737***	0.00297
			(0.00144)	(0.00131)	(0.00144)	(0.00145)
fem × post	0.0702***	0.0694***	0.00895	0.0102		
	(0.00938)	(0.00821)	(0.00943)	(0.00891)		
fem × time			0.00205***	0.00331***	0.00205***	0.00366***
			(0.000458)	(0.000811)	(0.000458)	(0.000678)
post × time			-0.00242***	-0.00173*	-0.0114	-0.00142
			(0.000587)	(0.000803)	(0.00922)	(0.000761)
female	0.0345***	0.0353***	0.0539***	0.0677***	0.0569***	0.0723***
	(0.00391)	(0.00567)	(0.00473)	(0.00614)	(0.00424)	(0.00509)
time	0.00145***	0.00264***	0.000715***	0.00209***	0.00370	0.00190***
	(0.000284)	(0.000151)	(0.0000994)	(0.000436)	(0.00319)	(0.000366)
Constant	0.337***	0.315***	0.322***	0.323***	0.382***	0.320***
	(0.00568)	(0.00233)	(0.00199)	(0.00315)	(0.0638)	(0.00260)
Fixed Effects (YOB)	YES		YES		YES	
Fixed Effects (Cohort)		YES		YES		YES
Observation	204556	204556	204556	204556	204556	204556
R-Squared	0.0216	0.0215	0.0219	0.0218	0.0219	0.0218

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 11: Graduate Education Graduation Rates, Men's Cohort Adjusted

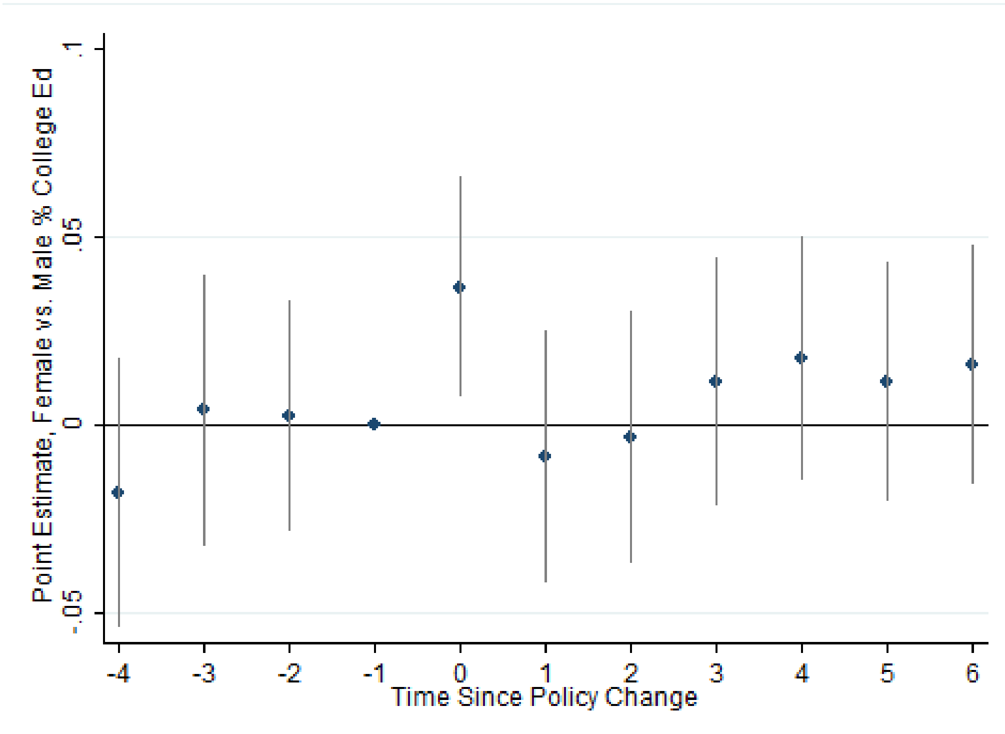
	Dependent Variable: Graduate Education					
	Panel A: DiD		Panel B: DiD with slopes			
	(1)	(2)	(3)	(4)	(5)	(6)
fem × post × time			0.00445***	0.00147	0.00445***	0.00144
			(0.00108)	(0.000988)	(0.00108)	(0.000980)
fem × post	0.0261***	0.0199***	-0.00416	0.00152		
	(0.00604)	(0.00477)	(0.00846)	(0.00633)		
fem × time			-0.000511	0.000471	-0.000511	0.000561
			(0.000563)	(0.000707)	(0.000563)	(0.000519)
post × time			-0.00663***	-0.00667***	-0.00247	-0.00671***
			(0.000556)	(0.000525)	(0.00833)	(0.000512)
female	0.00119	0.00428	-0.000366	0.00791	-0.00244	0.00883**
	(0.00310)	(0.00309)	(0.00444)	(0.00524)	(0.00430)	(0.00305)
time	-0.00304***	-0.00237***	-0.000642***	0.000567	-0.00272	0.000571*
	(0.000173)	(0.0000875)	(0.000139)	(0.000278)	(0.00430)	(0.000279)
Constant	0.0838***	0.0776***	0.120***	0.123***	0.0886	0.124***
	(0.00259)	(0.00127)	(0.00208)	(0.00180)	(0.0645)	(0.00154)
Fixed Effects (YOB)	YES		YES		YES	
Fixed Effects (Cohort)		YES		YES		YES
Observation	204556	204556	204556	204556	204556	204556
R-Squared	0.00420	0.00442	0.00454	0.00453	0.00454	0.00453

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

education, with one additional marginally significant three years after the cohort most affected by the policy change, here 1966, which may reflect the "echo" effect of greater college attendance among the younger cohorts now showing up in graduate school attendance.

Figure 12: Event Study: College Education

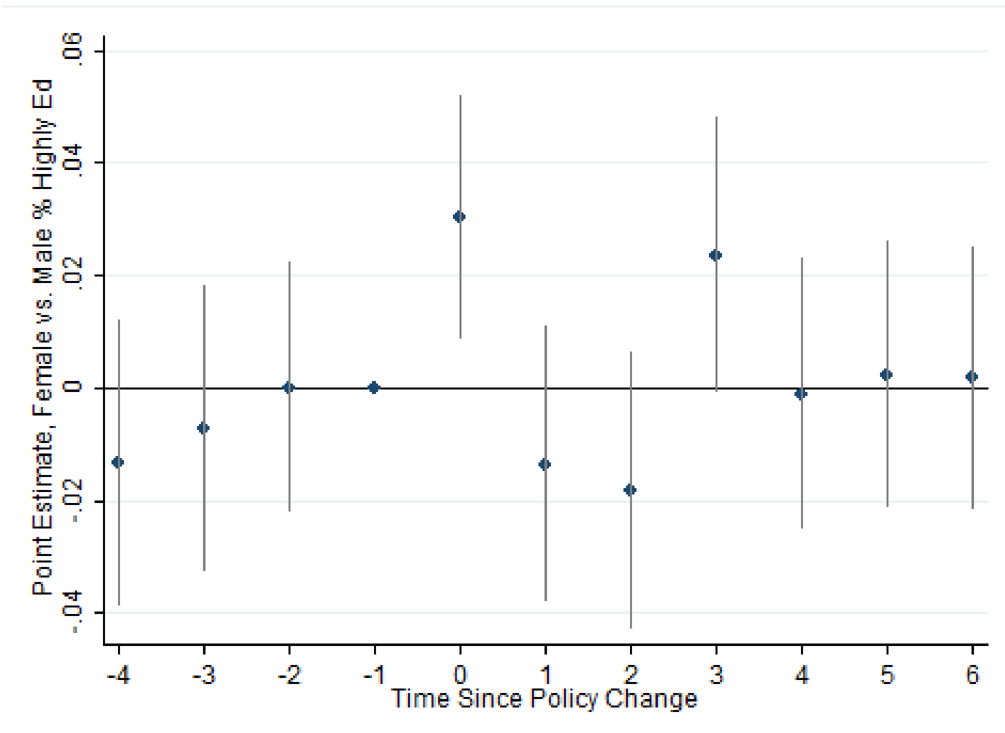


Notes: The figure presents point estimates and confidence intervals for coefficients on a series of dummy variables for being in a specific cohort or younger interacted with a dummy variable for female. The outcome variable is an indicator for college graduation. The 1971 cohort represents the first affected cohort and hence is the "time" of the policy change, noted as time 0 on the x-axis. The coefficient on being in the 1970 cohort or younger is normalized to zero.

Figure 14 shows the same post-period divergence in outcomes for age at first marriage, with being married at or after 1994 having a significant effect on women's marriage age relative to men's, with no other significant coefficients.

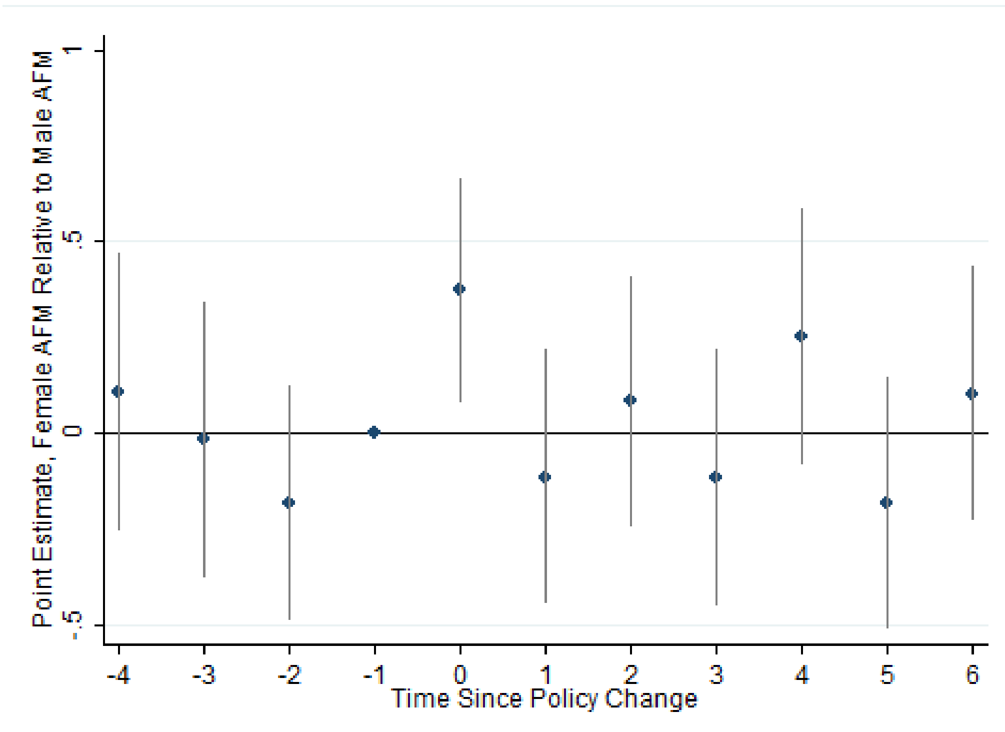
Figure 15 shows an event study analysis of the change in spousal income for women who marry older versus younger. This graph shows no significant effect in the period following the policy change, although it does also affirm there do not appear to be significant pre-trends prior to 1994. The weaker results in this event study may be due to men's beliefs about fertility of older partners take longer to update, as they first must observe older women getting pregnant and having children. These changed beliefs would come some time after the policy change, and thus effects would compound over time, and be more difficult to capture in an event-study format where future changes are controlled for.

Figure 13: Event Study: Graduate Education



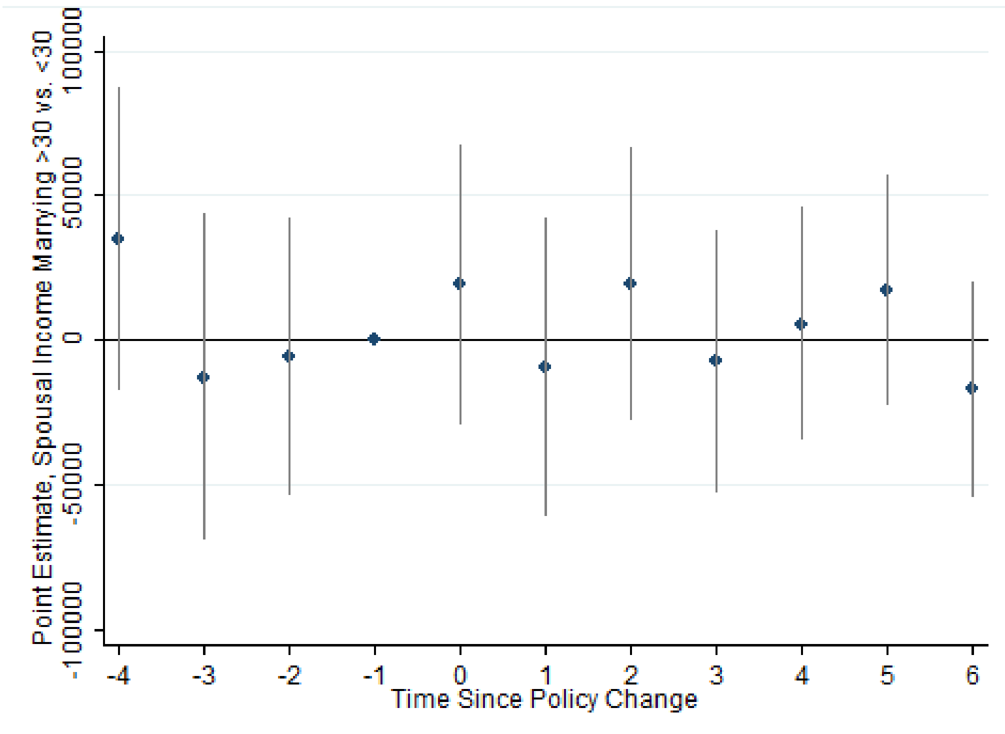
Notes: The figure presents point estimates and confidence intervals for coefficients on a series of dummy variables for being in a specific cohort or younger interacted with a dummy variable for female. The outcome variable is an indicator for post-college graduation. The 1966 cohort represents the first affected cohort and hence is the “time” of the policy change, noted as time 0 on the x-axis. The coefficient on being in the 1965 cohort or younger is normalized to zero.

Figure 14: Event Study: Age of First Marriage



Notes: The figure presents point estimates and confidence intervals for coefficients on a series of dummy variables for getting married in a specific year or afterwards interacted with a dummy variable for female. The outcome variable is age at first marriage. 1994 is the time of the policy change, noted as time 0 on the x-axis. The coefficient on getting married in 1993 or onwards is normalized to zero.

Figure 15: Event Study: Spouse Income



Notes: The figure presents point estimates and confidence intervals for coefficients on a series of dummy variables for getting married in a specific year or afterwards interacted with a dummy variable for being 30 or older at the year of marriage. The outcome variable is spousal income. 1994 is the time of the policy change, noted as time 0 on the x-axis. The coefficient on getting married in 1993 or onwards is normalized to zero. The sample is restricted to women with spousal matches that got married when they were older than 24 but younger than 35.

The distributed lag analysis we present suggests we have isolated a unique affect occurring in 1994, and impacting the cohorts entering school at that time, rather than a broader social change.

Next, we consider the possibility that other policy changes around the time of the expansion of IVF access could have been responsible for the changes we see. The two possible candidate alternative explanations are 1) an expansion of access to education, and 2) the other expansion of health services included under the 1994 law.

Higher-education reform The first alternative explanation that we consider is the higher education reform in Israel that was rolled out throughout the eighties and nineties, overlapping with our years of interest. Prior to the reform only the five universities could grant Israeli academic degrees. Starting in the seventies, slowly and gradually, colleges started to get permissions to grant academic degrees equivalent to the ones given by universities. This process accelerated during the eighties and early nineties, culminating in an official and comprehensive plan for the development of academic colleges. In the decade between 1992 and 2002 the number of students in academic programs approximately doubled (the effect of the reform was already apparent in the early nineties but really kicked in 1997-1998)(Volanski, 2005; Bernstein, 2002).

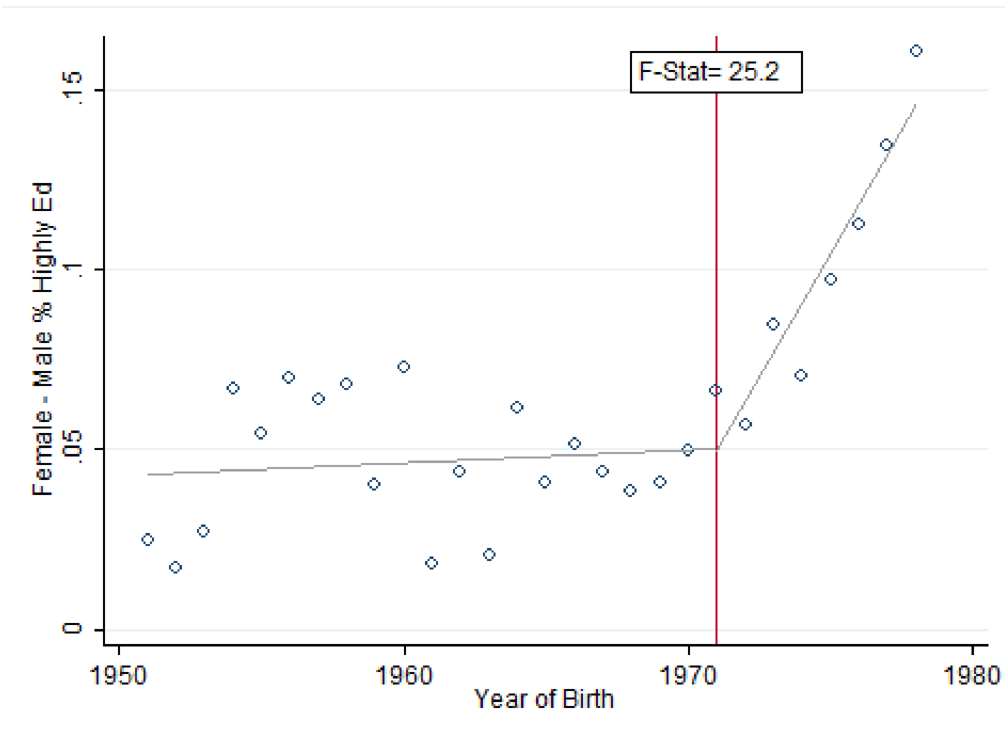
To verify that our results are not due to an “in name only” change in the degree individuals received, we graph the percentage of any post secondary education graduates, which will include those whose degree status would have been switched into the academic “college” category after the reform. Figure 16 shows that even if we add non-Academic degrees to our analysis, we get the same trends and the same change in trend only for women. This policy change also produces significant results in a regression, shown in Table A13. This, together with our strong results for graduate education, eases our concerns for higher education reform driving the results by re-labeling once non-academic degrees.

In addition, we find this alternative explanation to be unlikely, due to the different socio-economic classes targeted by the two reforms. At the time of the education reform, women already constituted more than 50% of undergraduate students. The main purpose of the reform was to make higher education institutions more accessible to a lower socioeconomic status population, mostly concentrated at peripheral regions (Volanski, 2005; Shavit et al., 2007), and increase higher education supply to match the rapidly increasing demand.[39] In addition, numerous studies were conducted to document the reform’s consequences, none of which report a distinctive effect on women’s participation in higher education (see for example Volanski (2005)). Over the years that followed the percentage of female students in colleges was actually lower than in universities.[40]

[39]The demand increase stems from the growing rate of high-school graduates that received certificates in matriculation exams (which are needed when applying for college)

[40]The only exception is teacher’s training colleges, where there is a vast majority if female students, however those colleges’ academization process in the early eighties. In addition, the students in these institutions constitute only a small share of the number of college students overall.

Figure 16: Difference in Percentage of Post-Secondary Education Attainment for Female and Male



Notes: The figure presents the difference in average post secondary education completion rates between women and men, as well as fitted lines for the pre and post periods. The 'F-stat' is for the Panel C specification (table A13), on the null-hypothesis that both coefficients on the interaction terms of interest (fem×post and fem×post×time) are equal to zero, i.e. the policy has no gender differential effect. The value of the F-stat confirms that the change is statistically significant.

Moreover, similar reforms in other countries were not found to affect women differently than men. One example is the higher education reform in Spain, which was enacted at approximately the same years as in Israel, and did not change the trend of women's education or of women's marriage decisions.[41]

It should also be noted that there is no reason to expect the reform to affect the way women's marriage outcomes depend on their age. We find that the previously existing penalty for older marriage practically disappears, even if we control for women's level of education. This finding cannot be explained by the increased supply of higher education.

We further explore this alternative explanation by reviewing our education outcomes for the Arab population of Israel, below. This population was more likely to be affected by the higher education reform due to lower high-school achievements (on average) and higher concentration in peripheral areas. However, they were less likely to be influenced by the change in access to IVF.

Other health expansions A different explanation of the improvement in marriage outcomes for older women might simply be the entire health services reform that the NHI law provided. Better health services can make age less important, if we believe that in the marriage market age is a proxy for health in general, rather than just fertility. However, decisions on education and marriage age should not be affected by the expansion of health services, especially since those decisions are made by young people who value those services less than older people. As far as the general insurance that better health insurance provides, there is no reason to expect that a health reform that provided the same benefits for all would have a gender divergent effect. Moreover, if anything, better public provision of health services could discourage educational and career investments, since health benefits will be provided regardless of future earnings.

Placebo tests To verify that broader international trends during the nineties are not responsible for our effects, we conduct placebo tests in the United States as well as four other countries with similar GDP per capita to Israel, and Census data availability. Results for college education in four "comparable" countries are shown in Figure A5. The United States American Community Survey also contains information on marriage age, so allows us to look at both educational and marriage outcomes, shown in Figure A6. None of these placebo tests produce positive, significant results.

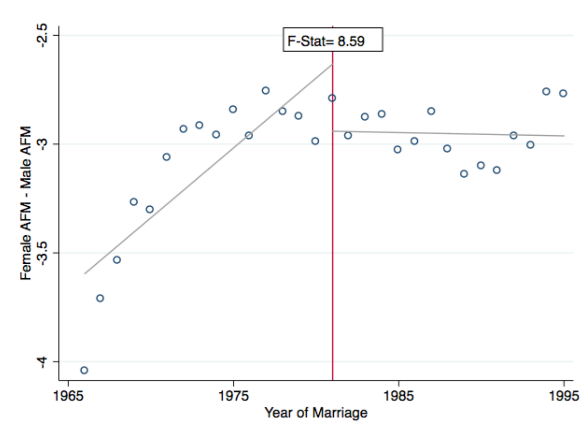
Due to the nature of using cross-sectional data, we necessarily conduct retrospective analysis of some of our outcome measures. Is it possible that looking backwards at education that occurred years ago creates discontinuous changes over time as an artifact of the data? This could be especially true if we thought men's educational outcomes were more censored than women's outcomes as we look at years closer to the present day, since men may take longer to finish their education. To

[41] See for example More(1996). We also use data from UNECE to examine age a first marriage, which changes smoothly throughout the reform period.

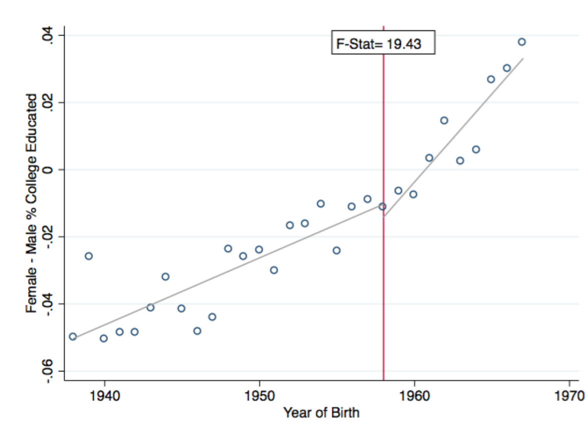
check whether the retrospective nature of the analysis could create similar breaks in the data without a real policy effect, we use the 1995 Israeli Population Census to replicate our analysis for a fake policy in 1981, 14 years before the Census is conducted, as the real 1994 policy change is 14 years before the 2008 Census. We find no evidence of a break in age at first marriage, graduate education, or spousal income based on age at marriage. There is, however, a significant F-stat on college education, although the graph does not show the same kind of sharp discontinuity as seen at the real policy change.

Figure 17: Placebo Test using 1995 Israeli Census

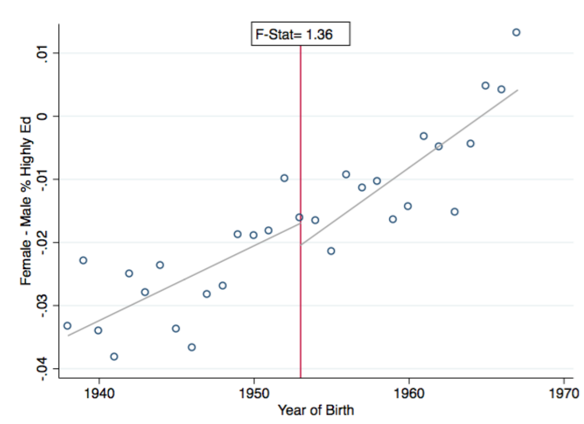
(a) Age First Marriage



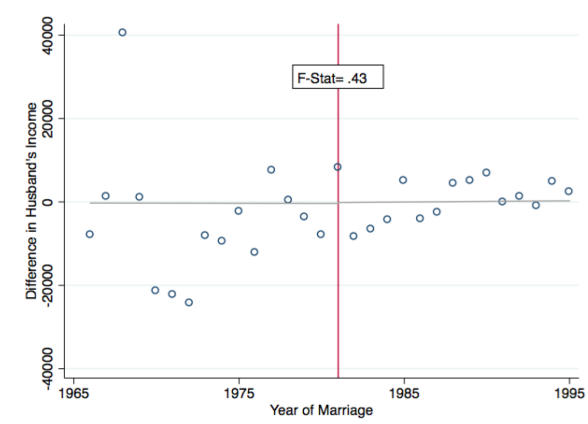
(b) College Education



(c) Graduate Education



(d) Husband's Income



We additionally conduct a placebo test using Arab-Israelis, who we believe will be less affected by the policy for three main reasons: First, most Arab Israelis are Muslim and Islam places more stringent restrictions on the use of *in vitro* fertilization than does Judaism.[42] In addition, the

[42]For example, Islam prohibits the use of donor eggs or sperm, the former being extremely important and even crucial for women in their fourties. In addition, the Israeli Jewish religious leadership very quickly addressed the innovative IVF technology and approved usage with practically no limitations, whereas other religions took longer to

Arab population is on average more religious and traditional. This is demonstrated by a much lower average age at first marriage for women, a higher average birth rate and a much lower labor participation for women.[43] Second, Arab-Israeli women are much less likely to be on the margin of large career investments in the 1990s, as average educational levels were substantially lower than the Jewish population. At our baseline year 1993, there is a 25 percentage points difference in the rate of women's college attainment between Jews and Arabs, and a 10 percentage points difference in the same figures for graduate education (Arab women's attainment in graduate education was actually very close to zero at that time). Third, since the Arab population are not subject to obligatory military service as the Jewish population, they tend to make decisions about career investment when they are 2-3 years younger. This makes fertility considerations much less relevant, especially for women, where this time difference was demonstrated to be critical later on.

These placebo tests, in Figure 18, reveal that Arab women did not experience the same differential shifts relative to Arab men that we see in the Jewish population. The tests show either no effect or, in the case of age at first marriage, the opposite effect. Note that the affected cohort for college and graduate education is adjusted to reflect the timing of Arab-Israelis entering college, which is younger due to no military service requirement. The dotted line reflects the cohort used in the main analysis, and there is no break apparent at that point, either.

This placebo test helps rule out that other educational expansions or policies that benefit women relative to men may have been responsible for the changes, since the Arab population benefits from these policies as well. Moreover, much of the expansion of educational access that occurred in Israel during this time period was specifically targeted at outlying areas and under-served populations. This provides further evidence that the changes to education for Jewish women during this time were driven by the IVF law.

4.4 Using Arab Population as Control Group

Our identification strategy relies on the post-1994 time-path of men's outcomes being similar to women's once pre-trends and level effects have been controlled for. A threat to this identification would be a policy or any other exogenous shock, that affected Israeli women, but not men, commencing at or around the time of the 1994 IVF policy change. The event study analyses show that such a change would need to be very precisely timed to coincide with the IVF policy change in order to produce similar results. We have already shown via a placebo test with Arab-Israelis that gender-divergent trends are unlikely to account for our result, since Arab-Israeli men and women would be likely to experience similar impacts from other policies, but are unlikely to respond to IVF availability. We now take this one step further, and replace men as our control group with

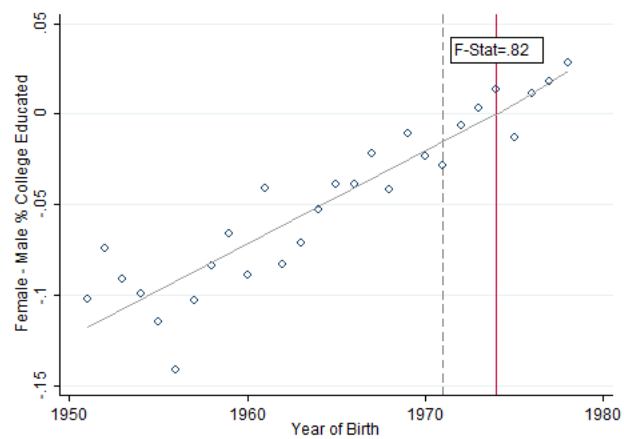
respond.

[43] In our baseline year 1993, for example, Arab women appear to marry 2.5 years earlier than Jewish women and the pre-trend for Arabs is positive but much more moderate.

Figure 18: Placebo Test using Arab-Israelis

(a) Age First Marriage

(b) College Education



(c) Graduate Education

(d) Husband's Income

Arab women, which will enable us to difference out the impact of being female following the policy change. The validity of this strategy relies on Arab-Israeli women being much less likely to use IVF due to religious restrictions, as well as less likely to be on the margin of a large career investment, due to lower baseline education, but being similarly affected by other policy changes, such as general expansion of female access to education. Since the Arab population may also be affected by events that are specific to their distinct culture and irrelevant to the Jewish population in Israel, this strategy would certainly be imperfect on its own. However, any bias should be uncorrelated with the bias from using men as the principal control group, and thus, the highly consistent findings presented here provide another piece of evidence for the effect of access to IVF on Jewish women in Israel. In this section we mainly focus on educational outcomes since, at least for college, the pre-trends exhibited by Jewish men and women differ, suggesting the possibility that different forces drive the educational choices of men and women. In addition, this second analysis of the educational outcomes should help eliminate the concerns for the higher education being the mechanism behind the boost we observe for women's college attainment.

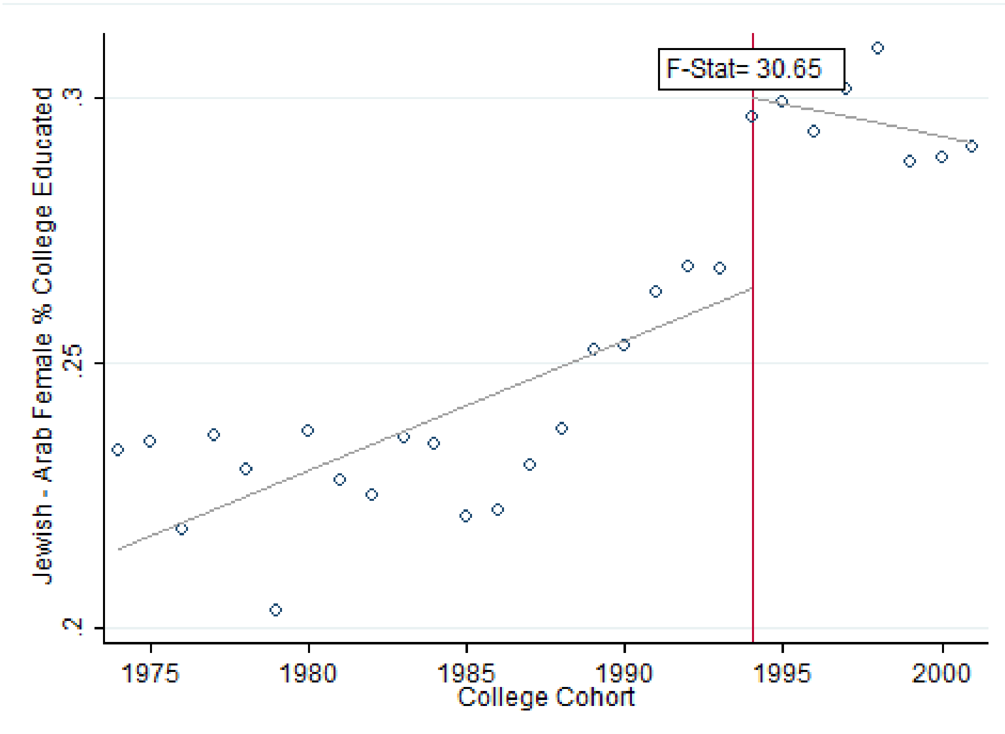
For these results, because we want to capture any other effects that could have possibly impacted women entering college in 1994, we need to use a different birth cohort of Arab women, as they enter college earlier mainly because they do not have a military service requirement. Although military service for Jewish women is two years long, macro data shows a three year difference in the median age of college applicants, between the two populations.[44] So in our figures and the following regression analysis we align the affected cohorts for the two groups to match college entry at 1994. As a result we compare Jewish women to three-years-younger Arab women. Nevertheless, all of the results hold and are qualitatively similar when we do not adjust for this difference and conduct the analysis using year of birth.

Figures 19 and 20 show the difference between Jewish and Arab women using raw data on college and graduate education completion. Figure A7 shows these outcomes separately by population group. Although the pre-trends are not parallel, as with the men control group, a similar increase in both types of education is clearly observed at 1994. Combining the results in this section with the ones in our main specifications, establishes that the most likely cause for the observed change in Jewish women's educational choices is the increased access to IVF. Any other explanation would have to prove to induce both a gender divergent and a religion based divergent impact, in addition to affecting all of the outcomes we consider. Such an occurrence is highly unlikely. Tables 12 and 13 confirm that the results for education hold using the alternate Arab-female control group. The magnitude of the coefficients is strikingly similar, at approximately 3 percentage points increase for college and 4 percentage points for graduate education, though the pattern appears to be different based on the Panel C results. Note that Jewish-Israeli women appear to take longer to complete

[44] This is not surprising since some military occupations require a prolonged service duration and also since there is an average waiting period of 5 months between highschool graduation and induction date.

their degrees and have a higher variation for age at college entry, resulting in a slight negative time trend after the policy change (and thus a negative coefficient on *jewish* $\times$ *post* $\times$ *time* in Panel C), but a robustly positive "post" effect.

Figure 19: Percentage of College Education by College Cohort (Arab Control Group)



Notes: The figure presents the difference in average college completion rates between Jewish and Arab women, as well as fitted lines for the pre and post periods. The affected cohorts are aligned so that the first affected cohort for each population appears for the first year in the post period, i.e. 1994. This cohort is the 1971 cohort for Jewish women and the 1974 cohort for the Arab women. The three years difference is based on the difference in median age for college applicants reported in macro data published by the Israeli CBS. The 'F-stat' is for the Panel C specification (table 12), on the null-hypothesis that both coefficients on the interaction terms of interest (fem$\times$post and fem$\times$post$\times$time) are equal to zero, i.e. the policy has no differential effect by population group. The value of the F-stat confirms that the change is statistically significant.

We further challenge our results by including both the Arab control and the male control in a triple difference specifications, shown in Table A15 for college education and Table A16 for graduate education. The results hold qualitatively and exhibit very similar magnitudes.

Although we use the Arab control principally to address educational results, Table A14 shows the results for age at first marriage, where we see that Jewish-Israeli women experience a differential increase in age at first marriage, beginning in 1994, compared to Arab-Israeli women. For spousal income, we did not use men as the principal control group, but rather women who married younger, and thus there is no scope for the alternate Arab control.

Table 12: College Graduation Rates by College Cohort (Arab Control)

	Dependent Variable: College Education					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
jewish × post × time					-0.00925***	-0.00928***
					(0.00186)	(0.00186)
jewish × post	0.0446***	0.0447***	0.0320*	0.0310*	0.0532***	0.0523***
	(0.00910)	(0.00914)	(0.0132)	(0.0133)	(0.00927)	(0.00919)
jewish × time			0.000906	0.000995	0.00242***	0.00250***
			(0.000870)	(0.000881)	(0.000497)	(0.000514)
post × time					0.00825***	0.00618***
					(0.000784)	(0.00159)
post	0.0159		0.0252*		0.00642	
	(0.0101)		(0.0109)		(0.00587)	
jewish	0.223***	0.223***	0.232***	0.232***	0.246***	0.246***
	(0.00473)	(0.00472)	(0.0100)	(0.0100)	(0.00554)	(0.00553)
time	0.00572***	0.00556***	0.00504***	0.00515***	0.00368***	0.00367***
	(0.000455)	(0.000240)	(0.000610)	(0.000485)	(0.000409)	(0.000375)
Constant	0.147***	0.151***	0.141***	0.151***	0.129***	0.135***
	(0.00491)	(0.00419)	(0.00624)	(0.00437)	(0.00480)	(0.00544)
Fixed Effects		YES		YES		YES
Observation	143103	143103	143103	143103	143103	143103
R-Squared	0.0754	0.0760	0.0754	0.0760	0.0756	0.0762

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

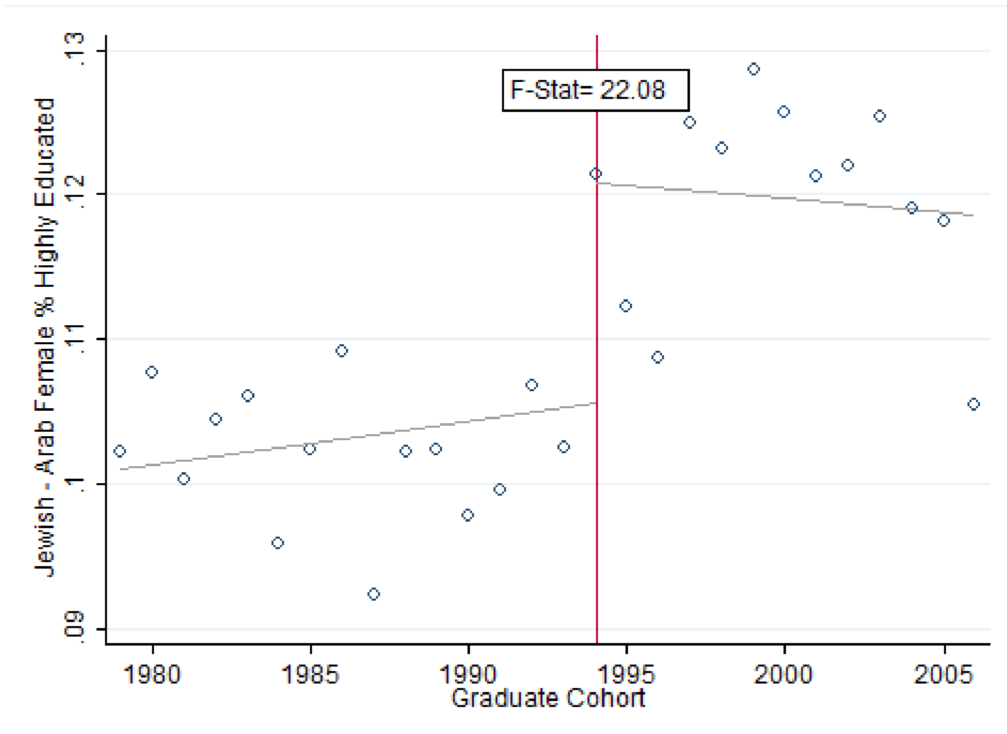
Table 13: Rates of Graduate Education by Graduate Cohort (Arab Control)

	Dependent Variable: Graduate Education					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
jewish × post × time					-0.00265	-0.00248
					(0.00141)	(0.00140)
jewish × post	0.0131*	0.0118	0.0407**	0.0400**	0.0372***	0.0361***
	(0.00546)	(0.00589)	(0.0114)	(0.0111)	(0.00899)	(0.00868)
jewish × time			-0.00189*	-0.00194*	-0.000372	-0.000420
			(0.000922)	(0.000941)	(0.000209)	(0.000209)
post × time					-0.000848*	-0.00145
					(0.000336)	(0.00139)
post	0.0192		-0.000968		-0.00235	
	(0.0123)		(0.00290)		(0.00298)	
jewish	0.0957***	0.0969***	0.0820***	0.0829***	0.0941***	0.0939***
	(0.00122)	(0.00114)	(0.00769)	(0.00732)	(0.00223)	(0.00226)
time	-0.00101	-0.00109***	0.000396	-0.000345	0.000920***	0.000195
	(0.000820)	(0.000152)	(0.000200)	(0.000460)	(0.000242)	(0.000409)
Constant	0.00792	-0.00999***	0.0179***	-0.0108***	0.0216***	0.00670
	(0.00616)	(0.00227)	(0.00181)	(0.00175)	(0.00238)	(0.00603)
Fixed Effects		YES		YES		YES
Observation	143103	143103	143103	143103	143103	143103
R-Squared	0.0249	0.0269	0.0250	0.0270	0.0255	0.0271

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 20: Percentage of Graduate Education by Graduate Cohort (Arab Control Group)



Notes: The figure presents the difference in average graduate education completion rates between Jewish and Arab women, as well as fitted lines for the pre and post periods. The affected cohorts are aligned so that the first affected cohort for each population appears for the first year in the post period, i.e. 1994. This cohort is the 1966 cohort for Jewish women and the 1969 cohort for the Arab women. The three years difference is based on the difference in median age for college applicants reported in macro data published by the Israeli CBS. The 'F-stat' is for the Panel C specification (table 13), on the null-hypothesis that both coefficients on the interaction terms of interest (fem$\times$post and fem$\times$post$\times$time) are equal to zero, i.e. the policy has no differential effect by population group. The value of the F-stat confirms that the change is statistically significant.

5 Conclusion

Increased access to *in vitro* fertilization offers women security of a second-line option in case they do not naturally achieve their desired level of fertility. Like any insurance, this guaranteed access to IVF causes individuals to be more willing to take actions that expose themselves to risk. Here that translates into women delaying starting families, using the time to pursue additional education and potentially other opportunities. The delay in starting families is shown by the stark increase in age at first marriage for women, following the policy change. The productive use of this time is demonstrated by the rise in completion of college and graduate education.

Moreover, the effect of this policy went beyond ameliorating women's tradeoff between human capital investments and fertility–it also appears to have updated men's beliefs about older women's value as partners. The evidence we show that older women marry richer partners after the policy is consistent with Low (2014)'s model of assortative matching among men and women along income dimensions being disrupted by fertility: when older women are expected to be less fertile, they may match with poorer spouses than younger women. However, with a lower decline in fertility with age, older women will match with higher income men. This shift in the marriage equilibrium may further reflect in women's decisions—knowing they will not lose as much reproductive capital by delaying marriage, and that their later-life marriage opportunities will be more favorable as a result, they will have fewer impediments to pursuing desired educational or career investments.

By testing what happens when the threat of later life fertility is attenuated, this research suggests that depreciating reproductive capital may represent a key source of asymmetry between men and women. When better insured against later life infertility, women delay marriage, invest in more education, and marry richer partners after doing so. In the absence of such insurance, this female-specific sharp decline in fertility may contribute to lower human capital investments by women during their reproductive years. In Israel, this manifests as women investing in more education, because women start families quite young. In other OECD countries, however, this underinvestment may take place after women have completed their education, but are required to pursue further on-the-job investments in order to climb the corporate ladder: late nights at the law firm, medical residencies, or the tenure sprint. Thus, depreciating reproductive capital may help to explain the lack of women in higher-level management positions as well as the upper-level gender wage gap. Policies that alleviate this burden through greater support for child-rearing in two-career households, access to maternity leave and career re-entry, or, as in Israel, insurance against later life fertility, could have far-reaching effects in increasing overall societal human capital, enabling firms to retain the best employees, while promoting social equity.

In regard to the specific Israeli policy we evaluate here, our findings demonstrate that the beneficiary population extends far beyond the women who actually use IVF or other assisted reproduction technologies. Rather, because the guaranteed access acts as insurance in case natural conception fails, all women considering further educational investments or delayed marriage may benefit. This is of critical importance because the cost per user of free IVF, due to Israel's generous policy, is enormous, and Israel is currently considering measures to limit the policy, having already placed age limits on use, and restricted the number of cycles for certain women. When taking into account the women who are provided insurance and subsequently obtain more education and marry richer husbands, not to mention those who are simply afforded peace of mind from having a backup option, the potential benefits to be weighed against those costs expand considerably.

One slight caution in regards to this cost benefit calculation is that the type of benefits we describe may not be what the Israeli government had in mind when they enacted the policy. The objectives of the policy were not to increase women's education and career outcomes, but were rather explicitly pro-natalist, aimed at increasing the birth rate of Israeli citizens.[45] Thus, policymakers should note that the behavioral response to IVF access may cause fertility effects to go in the opposite direction. If women do delay starting families, assured against the outcome of having zero children, they may nonetheless end up with a smaller overall family size, due to the late start. Moreover, since some evidence suggests individuals are *overly* optimistic about IVF's success rates, some women may delay, fail to conceive naturally, and go on to use the technology, only to be unsuccessful.

These questions of the tradeoff between further human capital investments and labor market productivity versus satisfaction derived from family and home life extend beyond Israeli policy-making. As more and more US companies consider measures such as paying for employees to freeze eggs, which similarly creates insurance against later life infertility, some women who are already planning to delay childbearing may be relieved by the benefit, while others could see a constantly moving finish line for how long they are expected to delay, to work at peak capacity, before starting a family. With the new knowledge that reproductive capital is a critical determinant in women's human capital investments and marriage market outcomes, the next frontier is to design policies that strike a delicate balance: working to remove the one-sided burden of depreciating reproductive capital on women without further burdening them with an impossible juggling act.

[45] The policy was defended in courts and described as a part of the fundamental human right to give birth and build a biological family.

References

Abramowitz, J. (2012). Assisted Reproductive Technology and Women's Marriage and Birth Timing: Duration and Competing Risks Analyses. *Working Paper*.

Abramowitz, J. (2014). Turning Back the Ticking Clock: the Effect of Increased Affordability of Assisted Reproductive Technology on Women's Marriage Timing. *Journal of Population Economics 27*(2), 603–633.

Adda, J., C. Dustmann, and K. Stevens (2011). The Career Costs of Children. *CEPR Discussion Paper No. DP8697*.

Andrews, D. (1993). Test for Parameter Instability and Structural Change with Unknown Change Point. *Econometrica 59*, 817–858.

Avellar, S. and P. J. Smock (2003). Has the Price of Motherhood Declined Over Time? A Cross-Cohort Comparison of the Motherhood Wage Penalty. *Journal of Marriage and Family 65*(3), 597–607.

Bailey, M. J. (2006). More Power to the Pill: The Impact of Contraceptive Freedom on Women's Life Cycle Labor Supply. *Quarterly Journal of Economics 121*(1), 289–320.

Bailey, M. J. (2010). "Momma's Got the Pill": How Anthony Comstock and Griswold v. Conneticut Shaped US Childbearing. *American Economic Review 100*(1), 98–129.

Bernstein, S. (2002). The Rise and Fall of Private Higher Education in Israel. *Policy Studies 52*.

Birenbaum-Carmeli, D. (2003). Reproductive Policy in Context: Implications on Women's Rights in Israel, 1945–2000. *Policy Studies 24*(2-3), 101–113.

Birenbaum-Carmeli, D. (2004). 'Cheaper than a Newcomer': on the Social Production of IVF Policy in Israel. *Sociology of Health & Illness 26*(7), 897–924.

Bitler, M. and L. Schmidt (2006). Health Disparities and Infertility: Impacts of State-level Insurance Mandates. *Fertility and Sterility 85*(4), 858–865.

Bitler, M. P. and L. Schmidt (2012). Utilization of Infertility Treatments: the Effects of Insurance Mandates. *Demography 49*(1), 125–149.

Blackburn, M. L., D. E. Bloom, and D. Neumark (1993). Fertility Timing, Wages, and Human Capital. *Journal of Population Economics 6*(1), 1–30.

Bretherick, K. L., N. Fairbrother, L. Avila, S. H. Harbord, and W. P. Robinson (2010). Fertility and Aging: Do Reproductive-aged Canadian Women Know What They Need to Know? *Fertility and Sterility 93*(7), 2162–2168.

Bronson, M. A. and M. Mazzocco (2013). Cohort Size and The Marriage Market. *Working Paper*.

Buckles, K. (2007). Stopping the Biological Clock: Infertility Treatments and the Career-family Tradeoff. *Boston University, Unpublished Manuscript*.

Buckles, K. (2008). Understanding the Returns to Delayed Childbearing for Working Women. *The American Economic Review 98*(2), 403–407.

Buckles, K. S. (2013). Infertility Insurance Mandates and Multiple Births. *Health Economics 22*(7), 775–789.

Bundorf, M. K., M. Henne, and L. Baker. Mandated Health Insurance Benefits and the Utilization and Outcomes of Infertility Treatments.

Collins, J. (2001). Cost-effectiveness of in vitro fertilization. In *Seminars in Reproductive Medicine*, Volume 19, pp. 279–289.

Collins, J. A. (2002). An International Survey of the Health Economics of IVF and ICSI. *Human Reproduction Update 8*(3), 265–277.

Danziger, L. and S. Neuman (1999). On the Age at Marriage: Theory and Evidence from Jews and Moslems in Israel. *Journal of Economic Behavior & Organization 40*(2), 179–193.

Dessy, S. and H. Djebbari (2010). High-Powered Careers and Marriage: Can Women Have It All? *The BE Journal of Economic Analysis & Policy 10*(1).

Fisman, R., S. S. Iyengar, E. Kamenica, and I. Simonson (2006). Gender Differences in Mate Selection: Evidence From a Speed Dating Experiment. *Quarterly Journal of Economics 121*(2), 637–697.

Goldin, C. and L. F. Katz (2002). The Power of the Pill: Oral Contraceptives and Women's Career and Marriage Decisions. *Journal of Political Economy 110*(4), 730–770.

Gustafsson, S. (2003). Optimal Age at Motherhood. Theoretical and Empirical Considerations on Postponement of Maternity in Europe. In *Family, Household and Work*, pp. 345–367. Springer.

Haimov-Kochman R, Lifshitz M, E. M. E. B. Y. and H. A (2012). Misconceptions Regarding Age-Related Fertility Potential Among Israeli Students. *a Research Presented in the Israeli Fertility Association Annual Conference*.

Hamilton, B. H. and B. McManus (2012). The Effects of Insurance Mandates on Choices and Outcomes in Infertility Treatment Markets. *Health Economics 21*(8), 994–1016.

Hashiloni-Dolev, Y. (2013). *The Fertility Revolution.* Ben Shemen: Modan Publishing House, in Hebrew.

Hashiloni-Dolev, Y., A. Kaplan, and S. Shkedi-Rafid (2011). The Fertility Myth: Israeli Students' Knowledge Regarding Age-related Fertility Decline and Late Pregnancies in an Era of Assisted Reproduction Technology. *Human Reproduction 0*(0), 1–9.

Kleven, H. J., C. Landais, and J. E. Sogaard (2015). Children and Gender Inequality: Evidence from Denmark. *Working Paper*.

Lahey, J. N. (2012). The Efficiency of a Group-Specific Mandated Benefit Revisited: The Effect of Infertility Mandates. *Journal of Policy Analysis and management 31*(1), 63–92.

Loughran, D. S. and J. M. Zissimopoulos (2009). Why Wait? The Effect of Marriage and Childbearing on the Wages of Men and Women. *Journal of Human Resources 44*(2), 326–349.

Low, C. (2014). Pricing the Biological Clock: Reproductive Capital on the US Marriage Market. *Working Paper*.

Miller, A. R. (2011). The Effects of Motherhood Timing on Career Path. *Journal of Population Economics 24*(3), 1071–1100.

Ohinata, A. (2011). Did the US Infertility Health Insurance Mandates Affect the Timing of First Birth? *CentER Working Paper Series.*

Remennick, L. (2010). Between Reproductive Citizenship and Consumerism: Attitudes towards Assisted Reproductive Technologies among Jewish and Arab Israeli Women. *Kin, Gene, Community: Reproductive Technologies among Jewish Israelis 19*, 318.

Schmidt, L. (2005). Infertility Insurance Mandates and Fertility. *American Economic Review 95*(2), 204–208.

Schmidt, L. (2007). Effects of Infertility Insurance Mandates on Fertility. *Journal of Health Economics 26*(3), 431–446.

Shalev, C. and S. Gooldin (2006). The Uses and Misuses of In Vitro Fertilization in Israel: Some Sociological and Ethical Considerations. *Nashim: A Journal of Jewish Women's Studies & Gender Issues 12*(1), 151–176.

Shavit, Y., R. Arum, and A. Gamoran (2007). Stratification in Higher Education: A Comparative Study.

Simonstein, F. (2010). IVF Policies with Emphasis on Israeli Practices. *Health Policy 97*(2), 202–208.

Siow, A. (1998). Differential Fecundity, Markets, and Gender Roles. *Journal of Political Economy 106*(2), 334–354.

Taniguchi, H. (1999). The Timing of Childbearing and Women's Wages. *Journal of Marriage and the Family 61*(4), 1008–1019.

Velez, M., M. Connolly, I.-J. Kadoch, S. Phillips, and F. Bissonnette (2014). Universal Coverage of IVF Pays Off. *Human Reproduction 29*(6), 1313–1319.

Volanski, A. (2005). *Academy in a Changing Environment.* Kotar, in Hebrew.

Wilde, E. T., L. Batchelder, and D. T. Ellwood (2010). The Mommy Track Divides: The Impact of Childbearing on Wages of Women of Differing Skill Levels. *Working Paper* (16582).

6 Appendix

Figure A1: Percentage of Women with Children ≤1 year by Age Group

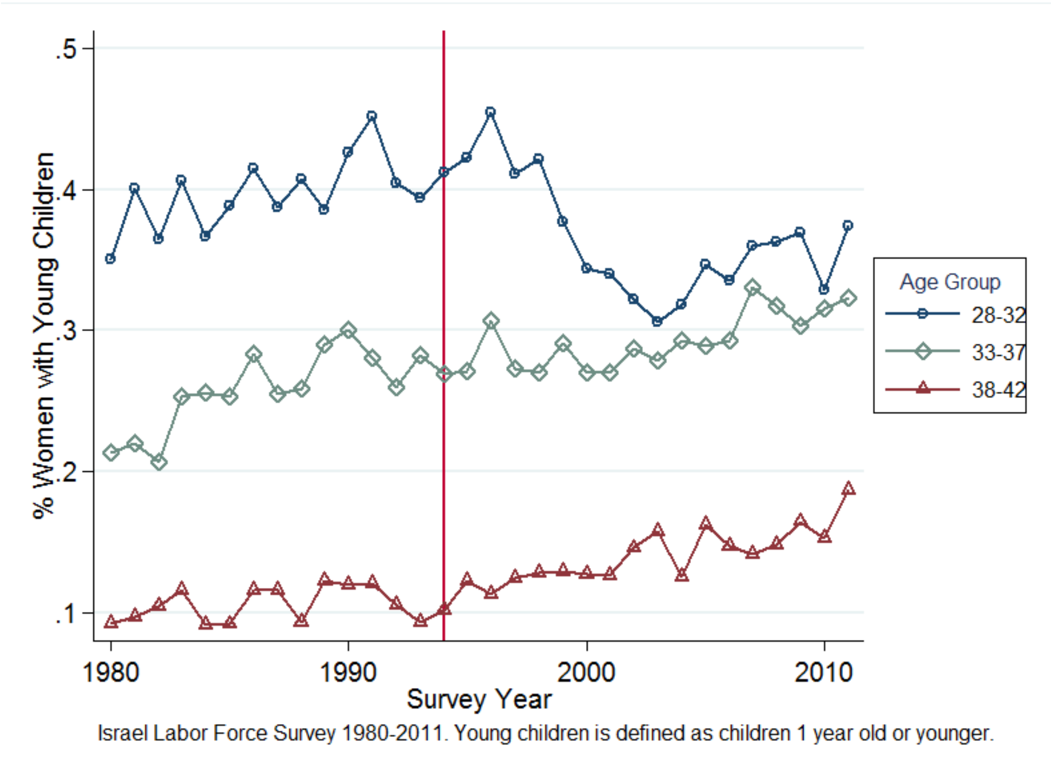


Figure A2: Percentage of Women with Children ≤1 year for Women Ages 38-42

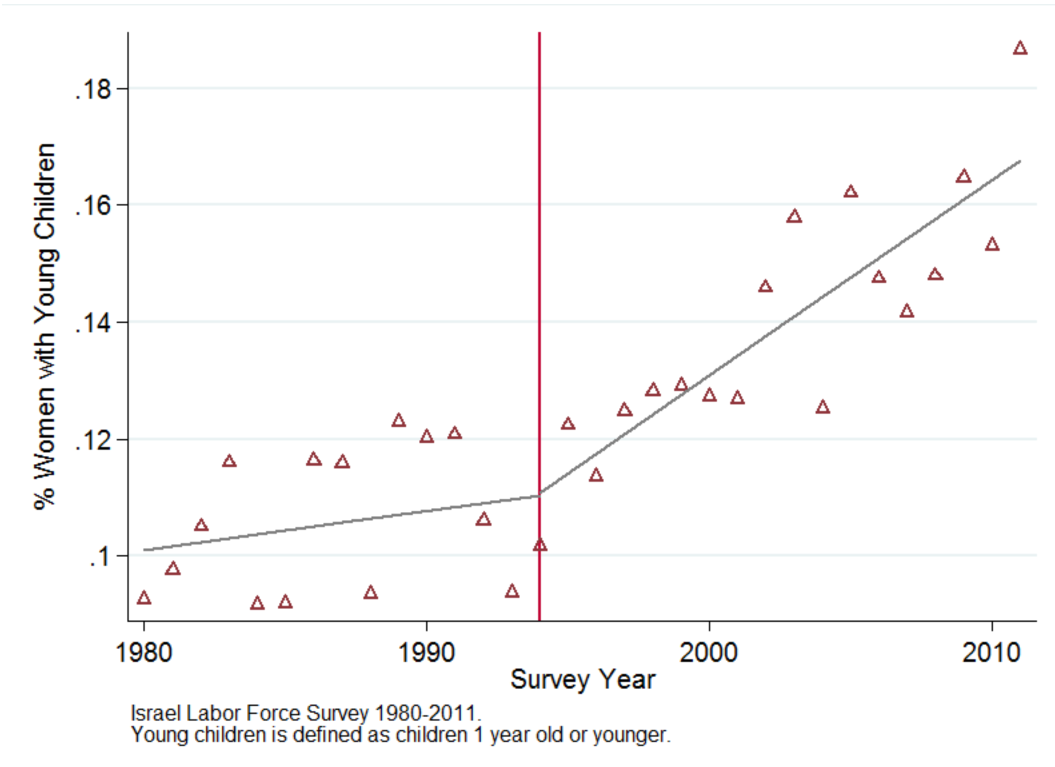
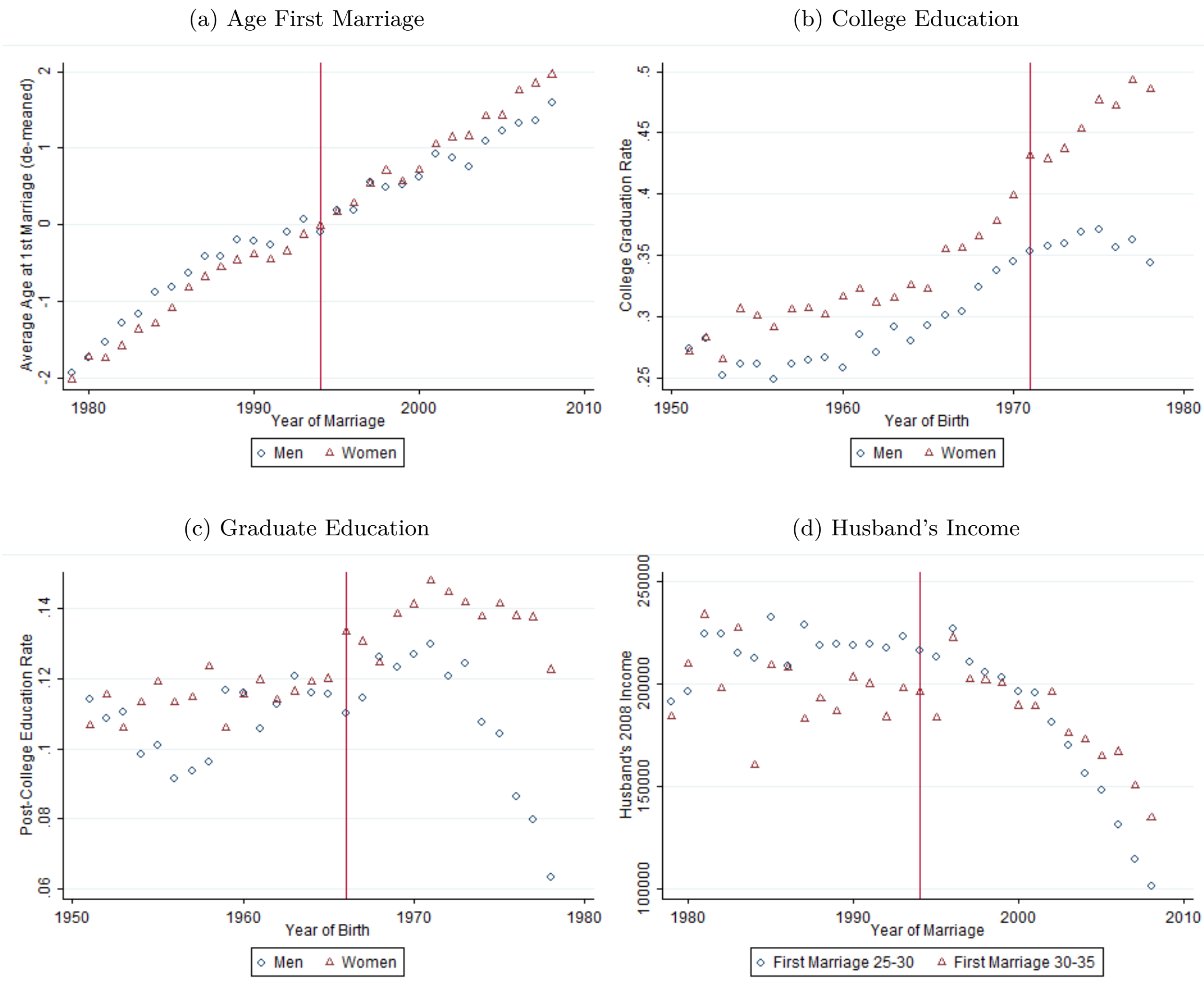


Figure A3: Outcome Variables Separated by Gender



Notes: This figure presents the age of first marriage, college education attainment, graduate education attainment by gender and spousal income differences between young and older women by age groups. This allows us to confirm that these groups exhibited parallel trends prior to the change, and that the changes we see are being driven by women, or women who marry older.

Table A1: College and Graduate Education Logit and Probit Regressions

Dependent Variable:	College		Graduate	
	(1) Logit	(2) Probit	(3) Logit	(4) Probit
fem × post × time	0.0356***	0.0220***	0.0512***	0.0262***
	(0.00706)	(0.00430)	(0.00955)	(0.00487)
fem × post	0.0193	0.0138	-0.124	-0.0646
	(0.0436)	(0.0267)	(0.0802)	(0.0417)
fem × time	0.00829***	0.00520***	0.000543	0.000338
	(0.00246)	(0.00145)	(0.00704)	(0.00364)
post × time	-0.0436***	-0.0264***	-0.0917***	-0.0481***
	(0.0125)	(0.00769)	(0.0182)	(0.00928)
post	0.220***	0.136***	0.438***	0.235***
	(0.0660)	(0.0406)	(0.130)	(0.0689)
female	0.236***	0.144***	0.0623	0.0328
	(0.0235)	(0.0141)	(0.0650)	(0.0339)
time	0.0203***	0.0121***	0.00908	0.00468
	(0.00378)	(0.00227)	(0.00465)	(0.00239)
Constant	-0.740***	-0.461***	-2.054***	-1.208***
	(0.0408)	(0.0248)	(0.0370)	(0.0193)
Observations	206921	206921	206921	206921

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A2: College Graduation Rates (Different End Year)

	Dependent Variable: College Education					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
fem × post × time					0.00479*	0.00476*
					(0.00227)	(0.00225)
fem × post	0.0546***	0.0544***	0.0272***	0.0266**	0.0170	0.0164
	(0.00738)	(0.00734)	(0.00724)	(0.00720)	(0.00922)	(0.00917)
fem × time			0.00221***	0.00224***	0.00202***	0.00205***
			(0.000457)	(0.000447)	(0.000470)	(0.000459)
post × time					-0.00125	-0.000891
					(0.00202)	(0.000849)
post	0.0146		0.0283*		0.0310**	
	(0.00902)		(0.0107)		(0.00958)	
female	0.0327***	0.0330***	0.0544***	0.0550***	0.0526***	0.0532***
	(0.00396)	(0.00391)	(0.00492)	(0.00492)	(0.00486)	(0.00484)
time	0.00513***	0.00480***	0.00403***	0.00431***	0.00408***	0.00429***
	(0.000628)	(0.000168)	(0.000758)	(0.000178)	(0.000783)	(0.000229)
Constant	0.332***	0.343***	0.321***	0.344***	0.322***	0.343***
	(0.00702)	(0.00285)	(0.00867)	(0.00226)	(0.00882)	(0.00239)
Fixed Effects		YES		YES		YES
Observation	172055	172055	172055	172055	172055	172055
R-Squared	0.0172	0.0178	0.0173	0.0180	0.0173	0.0180

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3: Graduate Education Graduation Rates (Different End Year)

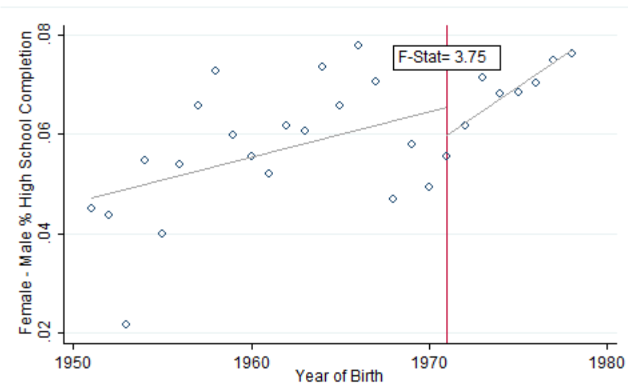
	Dependent Variable: Graduate Education					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
fem × post × time					0.00350	0.00351
					(0.00223)	(0.00223)
fem × post	0.00620	0.00607	0.00394	0.00377	-0.00585	-0.00588
	(0.00516)	(0.00518)	(0.00935)	(0.00937)	(0.0117)	(0.0117)
fem × time			0.000220	0.000223	0.0000947	0.0000937
			(0.000661)	(0.000661)	(0.000696)	(0.000695)
post × time					0.00193	0.00184
					(0.00118)	(0.00101)
post	0.00695*		0.00809		0.00284	
	(0.00325)		(0.00527)		(0.00604)	
female	0.00573	0.00576	0.00731	0.00736	0.00641	0.00643
	(0.00328)	(0.00328)	(0.00644)	(0.00644)	(0.00662)	(0.00661)
time	0.00105***	0.00121***	0.000941*	0.00116***	0.000869	0.000756*
	(0.000229)	(0.000135)	(0.000443)	(0.000179)	(0.000446)	(0.000351)
Constant	0.115***	0.124***	0.114***	0.124***	0.114***	0.117***
	(0.00227)	(0.00160)	(0.00361)	(0.00160)	(0.00368)	(0.00334)
Fixed Effects		YES		YES		YES
Observation	128076	128076	128076	128076	128076	128076
R-Squared	0.00120	0.00139	0.00120	0.00139	0.00129	0.00140

Robust standard errors in parentheses clustered at the year level.

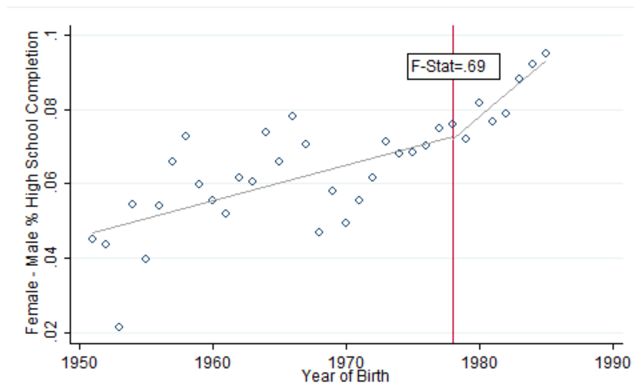
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure A4: High School Placebo

(a) 1971 Cutoff



(b) 1978 Cutoff



Notes: We test for a change in high school completion rates to rule out broader increases in education driving our effects. We test this for both the 1971 cohort, which would have been affected if the impact was something that affected all individuals born in 1971, and the 1978 cohort, which would have been affected if the impact affected all individuals pursuing schooling in 1994. We find insignificant F-stats in both cases.

Table A4: Spouse Income Percent, Marrying Older vs. Younger

	Dependent Variable: Spouse Income Percent					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
older × post × time					0.00616**	0.00664**
					(0.00212)	(0.00212)
older × post	0.00923	0.0107	0.0181	0.00975	0.0368**	0.0319**
	(0.00970)	(0.00959)	(0.0137)	(0.0136)	(0.0112)	(0.0112)
older × time			-0.000567	0.0000631	-0.00517*	-0.00509*
			(0.000991)	(0.000964)	(0.00193)	(0.00195)
post × time					-0.00459***	-0.00356***
					(0.00121)	(0.000300)
post	0.0226		0.0209		0.00752	
	(0.0128)		(0.0131)		(0.00935)	
married older	-0.0412***	-0.0415***	-0.0451***	-0.0411***	-0.0765***	-0.0763***
	(0.00859)	(0.00844)	(0.00840)	(0.00845)	(0.00832)	(0.00837)
time	-0.00150	0.0000515	-0.00138	0.0000465	0.00199**	0.00145***
	(0.000882)	(0.0000595)	(0.000928)	(0.000115)	(0.000619)	(0.000193)
Constant	0.538***	0.526***	0.539***	0.526***	0.562***	0.542***
	(0.00717)	(0.000850)	(0.00724)	(0.000866)	(0.00335)	(0.00110)
Fixed Effects		YES		YES		YES
Observation	31245	31245	31245	31245	31245	31245
R-Squared	0.00297	0.00521	0.00298	0.00521	0.00364	0.00545

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A5: Spouse College Education Rate

	Dependent Variable: Spouse College Education					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
older × post × time					0.0219***	0.00932
					(0.00420)	(0.00495)
older × post	0.0290	0.0223	-0.0406	0.0694	0.0295	0.0975*
	(0.0245)	(0.0207)	(0.0501)	(0.0487)	(0.0375)	(0.0439)
older × time			0.00444	-0.00308	-0.0121**	-0.0100*
			(0.00284)	(0.00272)	(0.00358)	(0.00382)
post × time					-0.00541*	0.00254
					(0.00223)	(0.00269)
post	0.0579**		0.0716**		0.0563**	
	(0.0179)		(0.0236)		(0.0176)	
married older	-0.0290	-0.0296	0.00203	-0.0503	-0.114**	-0.0989**
	(0.0198)	(0.0178)	(0.0307)	(0.0251)	(0.0313)	(0.0322)
time	0.000752	0.000603	-0.000172	0.00137	0.00375**	0.000803
	(0.00115)	(0.00107)	(0.00154)	(0.00130)	(0.00124)	(0.00132)
Constant	0.396***	-0.00177	0.390***	0.00651	0.417***	-0.0118
	(0.0102)	(0.0164)	(0.0130)	(0.0196)	(0.00860)	(0.0146)
Fixed Effects		YES		YES		YES
Observation	32984	32984	32984	32984	32984	32984
R-Squared	0.00458	0.0226	0.00480	0.0227	0.00582	0.0228

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A6: Spouse Graduate Education Rate

	Dependent Variable: Spouse Graduate Education					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
older × post × time					0.0153***	0.00841*
					(0.00349)	(0.00385)
older × post	0.0397*	0.0206	0.0159	0.0404	0.0607	0.0658
	(0.0175)	(0.0144)	(0.0346)	(0.0325)	(0.0333)	(0.0354)
older × time			0.00152	-0.00129	-0.00968**	-0.00758*
			(0.00203)	(0.00186)	(0.00318)	(0.00341)
post × time					-0.0125***	0.000158
					(0.00201)	(0.00130)
post	0.0533*		0.0580*		0.0227	
	(0.0247)		(0.0282)		(0.0174)	
married older	-0.0142	-0.0197	-0.00365	-0.0284	-0.0815**	-0.0723*
	(0.0154)	(0.0136)	(0.0197)	(0.0170)	(0.0292)	(0.0296)
time	-0.00521***	-0.0000984	-0.00553**	-0.000109	0.00349*	0.0000298
	(0.00142)	(0.000627)	(0.00170)	(0.000600)	(0.00144)	(0.000938)
Constant	0.133***	-0.00826	0.131***	-0.00976	0.194***	-0.0166
	(0.0161)	(0.00808)	(0.0175)	(0.00834)	(0.0113)	(0.00894)
Fixed Effects		YES		YES		YES
Observation	32984	32984	32984	32984	32984	32984
R-Squared	0.00443	0.0185	0.00448	0.0186	0.00774	0.0188

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A7: Spouse Quality

	Dependent Variable: Spouse Quality 2					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
older × post × time					0.0346***	0.0192*
					(0.00871)	(0.00719)
older × post	-0.0833	0.110**	-0.134	0.0553	-0.0101	0.115*
	(0.0494)	(0.0395)	(0.0756)	(0.0544)	(0.0557)	(0.0540)
older × time			0.00323	0.00358	-0.0243**	-0.0110
			(0.00432)	(0.00316)	(0.00779)	(0.00643)
post × time					0.00822	-0.0210***
					(0.00619)	(0.00347)
post	0.130*		0.140*		0.163**	
	(0.0552)		(0.0581)		(0.0492)	
married older	-0.0700	-0.114**	-0.0478	-0.0908*	-0.237***	-0.191***
	(0.0434)	(0.0374)	(0.0535)	(0.0406)	(0.0450)	(0.0492)
time	0.00355	-0.0174***	0.00289	-0.0176***	-0.00315	-0.00831**
	(0.00365)	(0.00182)	(0.00385)	(0.00181)	(0.00351)	(0.00276)
Constant	0.0924**	-1.777***	0.0879**	-1.776***	0.0467	-1.656***
	(0.0264)	(0.0223)	(0.0282)	(0.0222)	(0.0288)	(0.0117)
Fixed Effects		YES		YES		YES
Observation	31245	31245	31245	31245	31245	31245
R-Squared	0.00911	0.0365	0.00915	0.0366	0.0105	0.0367

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A8: Spouse Income, Marrying Older vs. Younger

	Dependent Variable: Spouse Income					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
older × post × time					5844.8***	6264.5***
					(1286.1)	(1368.4)
older × post	36273.6***	39133.5***	8071.4	-2205.2	23015.3**	18676.8*
	(7626.2)	(7439.3)	(11289.9)	(10616.2)	(7897.0)	(8016.7)
older × time			1808.7*	2657.2***	-2298.2	-2199.0
			(791.1)	(688.6)	(1186.2)	(1296.6)
post × time					-9681.0***	-9025.3***
					(1076.2)	(172.0)
post	37298.0*		42776.6*		14624.9	
	(15524.6)		(17141.9)		(8304.6)	
married older	-24226.4***	-24374.1***	-11838.3	-6223.2	-39679.9***	-39394.4***
	(5560.3)	(5498.3)	(7331.2)	(7211.7)	(6734.2)	(7087.6)
time	-5857.8***	-2709.1***	-6229.8***	-2920.3***	884.9	1174.9***
	(1030.3)	(56.99)	(1148.7)	(80.57)	(652.5)	(120.9)
Constant	181634.3***	158331.0***	179100.5***	157788.8***	227570.4***	214419.2***
	(10200.9)	(836.2)	(10959.5)	(598.8)	(3228.1)	(554.0)
Fixed Effects		YES		YES		YES
Observation	31245	31245	31245	31245	31245	31245
R-Squared	0.0346	0.0466	0.0349	0.0473	0.0442	0.0479

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A9: Spouse Income, Marrying Older vs. Younger With Wife Controls

	Dependent Variable: Spouse Income					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
older × post × time					4847.1***	5600.9***
					(1254.6)	(1496.5)
older × post	35789.9***	32402.3***	18325.0	11670.2	30001.8**	31071.9**
	(7316.0)	(7580.8)	(10984.2)	(11317.0)	(8906.1)	(10175.8)
older × time			1120.9	1329.6	-2206.1	-3094.1*
			(738.4)	(697.4)	(1136.5)	(1383.8)
post × time					-9265.7***	-8075.4***
					(979.6)	(203.1)
post	32521.0*		35928.9*		9198.7	
	(14774.6)		(16066.0)		(7583.9)	
married older	-29983.0***	-19247.5**	-22483.5**	-10289.6	-48449.2***	-40120.2***
	(8144.1)	(6538.7)	(7958.9)	(7864.0)	(8506.3)	(9012.8)
time	-4686.7***	-2050.4***	-4883.4***	-2113.5***	2581.8*	659.4***
	(1144.1)	(56.59)	(1155.1)	(71.24)	(1197.6)	(136.0)
Constant	69973.4	117233.1***	67067.1	117592.5***	86243.1*	155368.8***
	(41992.9)	(2432.9)	(43112.9)	(2679.6)	(39129.5)	(3048.6)
Fixed Effects		YES		YES		YES
Observation	31245	28301	31245	28301	31245	28301
R-Squared	0.101	0.122	0.101	0.122	0.109	0.122

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A10: Spouse Income, Spouse YOB FEs and Clustering

	Dependent Variable: Spouse Income			
	One Fixed Effect		Double Fixed Effect	
	(1)	(2)	(3)	(4)
older × post × time		2880.816		3198.3*
		(2061.6)		(1297.4)
older × post	13907.32*	27885.92*	22692.8**	26336.1**
	(5955.5)	(13136.4)	(6209.4)	(9022.2)
older × time		-2036.467		-2016.4
		(1762.8)		(1201.7)
post × time		-4445.224***		-4267.2***
		(995.1)		(586.9)
post	6567.203		-65313.8***	
	(6552.5)		(10280.3)	
married older	-15945.52**	-34692.36*	-21685.8***	-35598.0***
	(5462.3)	(13151.8)	(5676.6)	(7304.1)
time	-3112.041***	-109.1576	603.5	258.5
	(384.8)	(762.9)	(499.2)	(445.0)
Constant	-14703.2***	311.2118	-4879.8	-13002.4***
	(1923.9)	(3814.3)	(2922.0)	(3186.4)
Fixed Effects (YOB Spouse)	YES	YES	YES	YES
Fixed Effects (YOM)			YES	YES
Cluster (YOB Spouse)	YES	YES		
Cluster (YOM)			YES	YES
Observation	31245	31245	31245	31245
R-Squared	0.0556	0.0567	0.0578	0.0580

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A11: Regression All Populations, Difference-in-Difference with Slopes

	Dependent Variable			
	Age First Marriage	College	Post-College	Conditional Post-College
	(1)	(2)	(3)	(4)
fem × post × time	0.0309***	0.00778***	0.00250**	0.00787***
	(0.00790)	(0.00141)	(0.000685)	(0.00173)
fem × post	0.132	-0.00218	-0.00361	-0.000138
	(0.0656)	(0.00874)	(0.00586)	(0.0158)
fem × time	-0.0181**	0.00177***	0.000557	-0.00101
	(0.00628)	(0.000348)	(0.000383)	(0.00109)
post × time	0.0524***	-0.00427*	-0.00586***	-0.0204***
	(0.0108)	(0.00185)	(0.000875)	(0.00194)
post	-0.164			
	(0.105)			
post			0.0111	0.0113
			(0.00710)	(0.0169)
post		0.0318**		
		(0.00925)		
female	-3.092***	0.0387***	0.00309	-0.0409**
	(0.0530)	(0.00337)	(0.00386)	(0.0113)
time	0.0858***	0.000489	-0.000938**	-0.00314**
	(0.00800)	(0.000284)	(0.000338)	(0.000943)
Constant	26.46***	0.286***	0.124***	0.446***
	(0.0757)	(0.00368)	(0.00333)	(0.00969)
Observation	317717	374472	374472	115935
R-Squared	0.116	0.00663	0.00558	0.0419

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A12: Spouse Income (Merged All Population)

	Dependent Variable	
	Spouse Income	Spouse Percent Income
	(1)	(2)
older × post × time	3961.0***	0.00576***
	(785.4)	(0.00149)
older × post	8476.1	0.0178
	(4848.4)	(0.0113)
older × time	-327.7	-0.00265*
	(722.2)	(0.00129)
post × time	-7693.2***	-0.00410**
	(894.9)	(0.00133)
post	14692.5*	0.0138
	(7116.9)	(0.00961)
married older	-31567.7***	-0.0765***
	(3986.2)	(0.00860)
time	912.0*	0.00271***
	(445.7)	(0.000719)
Constant	197096.0***	0.571***
	(2734.4)	(0.00434)
Observation	46555	46555
R-Squared	0.0255	0.00608

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A13: Post-Secondary Education Rates

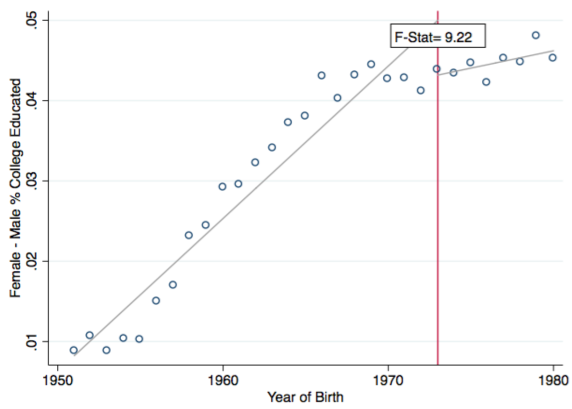
	Dependent Variable: Post-Secondary Education					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
fem × post × time					0.0160***	0.0149***
					(0.00242)	(3.75e-15)
fem × post	0.0780***	0.0772***	0.0301	0.0679***	-0.00845	0.00392***
	(0.0166)	(6.27e-15)	(0.0187)	(1.01e-14)	(0.0135)	(2.81e-14)
fem × time			0.00333**	0.000621***	0.000860	0.000526***
			(0.00119)	(5.26e-16)	(0.000599)	(1.03e-15)
post × time					-0.0144***	-0.0189***
					(0.00348)	(9.59e-15)
post	-0.00572		0.0182		0.0529**	
	(0.0163)		(0.0206)		(0.0179)	
female	0.0380***	0.0379***	0.0706***	0.0444***	0.0465***	0.0429***
	(0.00420)	(1.89e-15)	(0.0130)	(6.05e-15)	(0.00608)	(1.16e-14)
time	0.00400***	0.00264***	0.00233	0.00205***	0.00455***	0.00582***
	(0.000873)	(5.04e-16)	(0.00137)	(6.44e-16)	(0.000874)	(2.94e-15)
Constant	0.468***	0.366***	0.452***	0.371***	0.473***	0.508***
	(0.0108)	(5.55e-15)	(0.0163)	(6.29e-15)	(0.00957)	(5.57e-14)
Fixed Effects		YES		YES		YES
Observation	206921	206921	206921	206921	206921	206921
R-Squared	0.0162	0.0189	0.0165	0.0189	0.0177	0.0189

Robust standard errors in parentheses clustered at the year level.

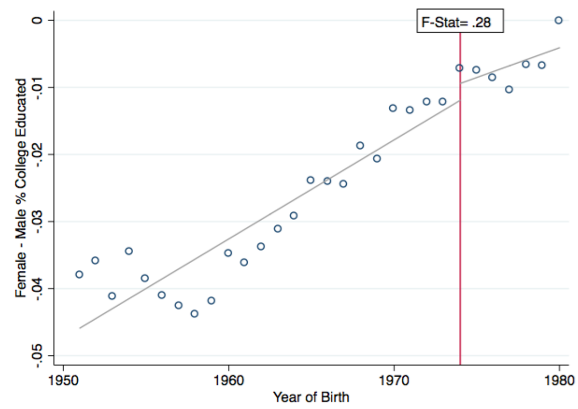
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure A5: College Attainment by Birth Cohort in Comparable Countries

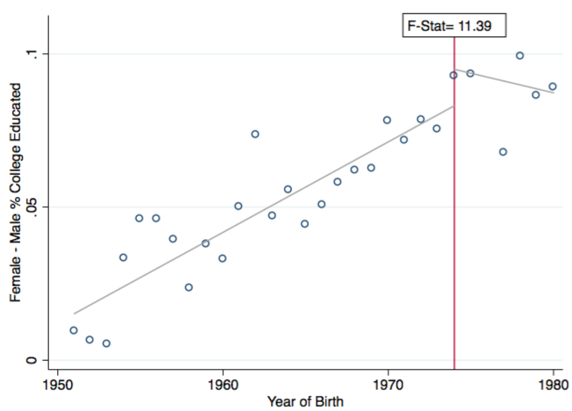
(a) Brazil



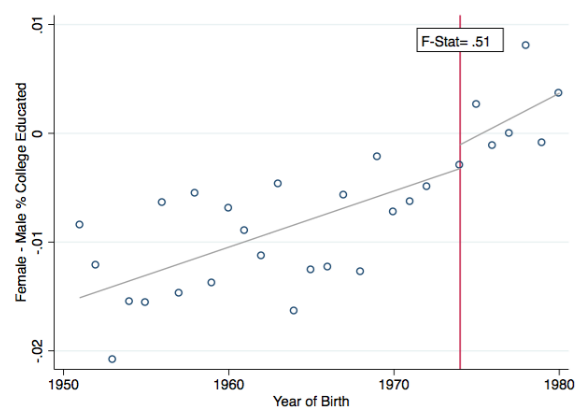
(b) Mexico



(c) Panama



(d) South Africa



Notes: We use countries that have Census's around the time of Israel's 2008 Census and similar GDP per capita as Israel to conduct placebo tests, showing that the cohort entering college in 1994 in other countries was not similarly affected (in each country, the red line is shifted according to typical college entry age of students in that country). This would be the case if broader international shifts in the nineties were responsible for the effects that we see. We do not observe similar discontinuous increases in female versus male college attainment over time in any of the other countries. The same lack of discontinuous trends is true when looking at graduate school attainment as well, although the data is somewhat noisier.

Figure A6: United States College Attainment and Age at Marriage

(a) College Attainment (b) Age at First Marriage

Notes: In the United States data, we can observe both education and marriage, since the American Census records year of marriage. Again, there is no discontinuous break observable, and the F-stats are insignificant.

Figure A7: Women's College and Graduate Education, by Population Group

(a) College Education (b) Graduate Education

Table A14: Age at First Marriage (Arab Control)

	Dependent Variable: Age at First Marriage					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
jewish × post × time					0.0584***	0.0589***
					(0.0124)	(0.0123)
jewish × post	1.019***	1.016***	-0.0456	-0.0274	-0.0758	-0.0566
	(0.137)	(0.138)	(0.169)	(0.167)	(0.109)	(0.108)
jewish × time			0.0750***	0.0735***	0.0466***	0.0447***
			(0.00999)	(0.00995)	(0.00595)	(0.00568)
post × time					-0.0202	-0.0291***
					(0.0135)	(0.00739)
post	-0.870***		-0.108		-0.0979	
	(0.158)		(0.130)		(0.123)	
jewish	2.066***	2.064***	2.573***	2.560***	2.374***	2.365***
	(0.0647)	(0.0649)	(0.0871)	(0.0865)	(0.0491)	(0.0478)
time	0.125***	0.101***	0.0698***	0.0733***	0.0798***	0.0862***
	(0.00496)	(0.00373)	(0.00748)	(0.00415)	(0.00638)	(0.00422)
Constant	21.98***	21.56***	21.62***	21.62***	21.69***	21.63***
	(0.0831)	(0.0544)	(0.0518)	(0.0309)	(0.0361)	(0.0329)
Fixed Effects		YES		YES		YES
Observation	124744	124744	124744	124744	124744	124744
R-Squared	0.121	0.122	0.122	0.123	0.122	0.123

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A15: College Graduation Rates (Arab Control), Triple Difference

	Dependent Variable: College Education					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
jewish × time × post × fem					0.00340**	0.00344**
					(0.00119)	(0.00119)
jewish × post × fem	-0.0210*	-0.0209*	0.0281*	0.0280*	0.0191*	0.0190*
	(0.00805)	(0.00802)	(0.0110)	(0.0111)	(0.00922)	(0.00925)
fem × post × time					0.00496**	0.00494**
					(0.00155)	(0.00155)
jewish × time × fem			-0.00383***	-0.00382***	-0.00429***	-0.00430***
			(0.000786)	(0.000788)	(0.000948)	(0.000943)
jewish × post × time					-0.0127***	-0.0128***
					(0.00206)	(0.00202)
jewish × fem	0.0861***	0.0864***	0.0541***	0.0545***	0.0491***	0.0494***
	(0.00750)	(0.00750)	(0.00745)	(0.00754)	(0.00815)	(0.00819)
jewish × time			0.00474***	0.00483***	0.00671***	0.00686***
			(0.00128)	(0.00130)	(0.00117)	(0.00117)
jewish × post	0.0674***	0.0673***	0.00398	0.00261	0.0341*	0.0327*
	(0.0128)	(0.0129)	(0.0194)	(0.0196)	(0.0131)	(0.0129)
post × time					0.00328*	0.00170
					(0.00151)	(0.00115)
fem × time			0.00713***	0.00716***	0.00632***	0.00635***
			(0.000747)	(0.000747)	(0.000692)	(0.000690)
fem × post	0.0978***	0.0978***	0.000986	0.000672	-0.0102	-0.0105
	(0.0142)	(0.0142)	(0.0101)	(0.0101)	(0.00924)	(0.00920)
post	-0.0527***		0.0242*		0.0167	
	(0.00853)		(0.00980)		(0.00837)	
female	-0.0536***	-0.0537***	0.0109	0.0111	0.00344	0.00363
	(0.00884)	(0.00886)	(0.00703)	(0.00707)	(0.00654)	(0.00657)
jewish	0.136***	0.135***	0.178***	0.178***	0.197***	0.197***
	(0.0109)	(0.0108)	(0.0138)	(0.0139)	(0.00989)	(0.00988)
time	0.00358***	0.000489	-0.00210**	-0.00263***	-0.00264***	-0.00281***
	(0.000563)	(0.000366)	(0.000623)	(0.000529)	(0.000706)	(0.000611)
Constant	0.181***	0.122***	0.130***	0.124***	0.125***	0.132***
	(0.00830)	(0.00375)	(0.00645)	(0.00372)	(0.00704)	(0.00442)
Fixed Effects		YES		YES		YES
Observation	283972	283972	283972	283972	283972	283972
R-Squared	0.0561	0.0568	0.0568	0.0575	0.0574	0.0581

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A16: Rates of Graduate Education (Arab Control), Triple Difference

	Dependent Variable: Graduate Education					
	Panel A: DiD		Panel B: DiD GSTT		Panel C: DiD with slopes	
	(1)	(2)	(3)	(4)	(5)	(6)
jewish × time × post × fem					0.00317	0.00328***
					(0.00165)	(0.000827)
jewish × post × fem	-0.00833	-0.00808	-0.00650	-0.00646	0.00458	0.00475
	(0.00536)	(0.00540)	(0.00949)	(0.00957)	(0.00653)	(0.00543)
fem × post × time					0.00000922	-0.0000264
					(0.00138)	(0.000861)
jewish × time × fem			-0.000238	-0.000223	-0.00241*	-0.00246***
			(0.000529)	(0.000535)	(0.00116)	(0.000621)
jewish × post × time					-0.00659**	-0.00660***
					(0.00192)	(0.00145)
jewish × fem	0.0454***	0.0454***	0.0447***	0.0448***	0.0280**	0.0277***
	(0.00441)	(0.00443)	(0.00620)	(0.00622)	(0.00860)	(0.00484)
jewish × time			-0.00166	-0.00171	0.00222**	0.00231***
			(0.00110)	(0.00111)	(0.000753)	(0.000588)
jewish × post	0.0223**	0.0207*	0.0472**	0.0465**	0.0374***	0.0359**
	(0.00760)	(0.00796)	(0.0163)	(0.0158)	(0.00960)	(0.0101)
post × time					-0.000632	0.00151
					(0.000685)	(0.00151)
fem × time			0.00200***	0.00200***	0.00199*	0.00203***
			(0.000284)	(0.000279)	(0.000927)	(0.000547)
fem × post	0.0348***	0.0347***	0.00610	0.00600	0.00614	0.00583
	(0.00374)	(0.00371)	(0.00427)	(0.00424)	(0.00525)	(0.00432)
post	-0.00257		-0.00707		-0.00812	
	(0.0121)		(0.00427)		(0.00467)	
female	-0.0413***	-0.0412***	-0.0271***	-0.0269***	-0.0271***	-0.0268***
	(0.00282)	(0.00279)	(0.00236)	(0.00229)	(0.00659)	(0.00393)
jewish	0.0498***	0.0514***	0.0372**	0.0384**	0.0674***	0.0679***
	(0.00437)	(0.00432)	(0.0109)	(0.0106)	(0.00576)	(0.00453)
time	-0.00192*	-0.00268***	-0.00160***	-0.00248***	-0.00121*	-0.00352***
	(0.000806)	(0.000171)	(0.000238)	(0.000532)	(0.000535)	(0.000531)
Constant	0.0427***	0.0147***	0.0450***	0.0142***	0.0477***	0.0169*
	(0.00604)	(0.00251)	(0.00228)	(0.00229)	(0.00406)	(0.00658)
Fixed Effects		YES		YES		YES
Observation	283972	283972	283972	283972	283972	283972
R-Squared	0.0193	0.0215	0.0196	0.0218	0.0209	0.0222

Robust standard errors in parentheses clustered at the year level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$