Older Yet Fairer: How Extended Reproductive Time Horizons Reshaped Marriage Patterns in Israel

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Abstract

Women's time-limited fertility window, compared to men's longer period of fecundity, may be a key constraint shaping their marriage choices. Israel's 1994 policy change to make *in vitro* fertilization free provides a natural experiment for how fertility time horizons impact women's marriage timing and outcomes. We find an increase in average age at first marriage after the policy change, likely driven both by older women facing a more favorable marriage market and younger women delaying marriage. The evidence also suggests older women's matches improved, with increases in spousal income for those marrying above age 30.

JEL Codes: D10, J12, J13, J16.

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1 Introduction

The ability to control fertility at young ages revolutionized women's marriage patterns and career choices. More recent technological innovations, the most prominent being In-Vitro Fertilization (IVF), have promised greater control over fertility at *older* ages, specifically to avoid age-related infertility.[1] If declining later-life fertility is a materially important constraint, this increased control could potentially alter women's marriage decisions and outcomes.[2] Unlike the pill, whose channel of impact was through usage (e.g., Goldin and Katz (2002)), assisted reproductive technology may affect women who anticipate its availability, even before the realization of their actual later-life fertility. Thus, testing its impact requires a sharp and large increase in IVF availability, in order to facilitate a rapid update of women's expectations. In light of this, Israel's unprecedented decision to make IVF completely free to all citizens in 1994 provides an ideal natural experiment.

The unique advantage of Israel's policy decision is that it overcomes the tendency for technology change to diffuse slowly. It came at a time when IVF was relatively new and unknown, and therefore the change in fertility expectations was likely to be large. It affected the entire population, and was widely covered in the Israeli press so this information could disseminate. Thus, it caused a large and discontinuous increase in usage: in the three years following the policy change, live deliveries using IVF more than tripled. By 2002, 8 years after the policy change, 1,657 IVF treatment cycles per million people were performed in Israel, compared to 126 in the United States (Collins, 2002). Even without direct knowledge of the policy change, the large amount of media attention to older women having children, and first-hand experience of observing motherhood at older ages, could have facilitated a general shift in beliefs regarding the time horizon of fertility.

The challenge inherent in this setting, however, is that since all Israelis were exposed to the policy change at the same time, there is no spatial or other statutory variation induced by the policy. Therefore, to understand the causal impact of the policy change on women, we compare their outcomes to those of men. Prior to this change, men's and women's age at marriage moved in lockstep, experiencing a common trend over time, and seemingly responding in tandem to shocks. This makes sense, as Jewish men and women have a largely "closed" marriage market in Israel. Access to IVF, however, is a shock that specifically affects women, and does not have a direct impact on men's later life fecundity.[3] Thus, a disruption of the tight co-movement between men's

[1] Women lose 97% of eggs by 40 (Kelsey and Wallace, 2010), while remaining egg quality declines (Toner, 2003). Pregnancies are rarer (Menken, Trussell and Larsen, 1986), more likely to end in miscarriage (Andersen et al., 2000), and more likely to result in fetal abnormalities (Hook, Cross and Schreinemachers, 1983) later in life, before the complete cessation of fecundity in menopause. While it is difficult to separate fecundity from fertility choices, even prospective studies of women trying to conceive show an accelerating decline in fecundity by age 40 for women, whereas men's fertility is relatively stable. For example, Rothman et al. (2013) finds that women 35-40 years old will become pregnant 77% as frequently as women age 20-24, whereas for men this ratio is 95%.

[2] In this way, our work is related to literature on how life expectancy impacts financial planning (Skinner, 1985; Jayachandran and Lleras-Muney, 2009) and how health assets affect other decisions and investments (Delavande and Kohler, 2015; Hugonnier, Pelgrin and St-Amour, 2012).

[3] Although the policy also provided treatment of male infertility via access to sperm donation, this is "primary"

and women's outcomes precisely in 1994 would represent strong evidence of a causal impact of the IVF expansion.

A potential downside of this strategy is that because men and women's marriage decisions are so closely related, men's decisions could be affected in equilibrium by women's decisions as well as by their expectations about potential partners' fertility. However, we choose to compare women's outcomes to men's nonetheless because it is precisely the extent to which time-limited fertility constrains women's decisions *relative* to men's, despite equilibrium effects, that is of interest economically. If men's decisions fully adjust to the extension of female fecundity via IVF, then perhaps time-limited fecundity did not inordinately constrain women's decisions in the first place. If, on the other hand, we can show that the expansion of access to IVF differentially changes women's decisions, it is evidence that their time-limited fecundity asymmetrically affected them, and may therefore be linked to other persistent gender asymmetries, such as the gender wage gap.

To shed additional light on these equilibrium impacts, as well as to confirm that the effects we find are not driven by other policies that specifically affected women in Israel during this time, we also use Arab-Israeli women as an alternate comparison group. Arab-Israeli women were much less likely to use IVF at the time of the policy change due to religious and cultural restrictions, but *were* impacted by policies and shocks affecting specifically female Israelis. For example, both groups would be affected by the broader trend, both global and local, towards increased female education. This control strategy may suffer from its own shortcomings, as there are significant differences between the Arab and Jewish populations in Israel, but they are unlikely to be correlated with any issues with the male control strategy, and thus obtaining similar results using the two comparison groups would provide compelling corroboration of a causal impact.

We find that immediately following the policy change, women's age at first marriage increased sharply and discontinuously relative to men's. This continued in an upward trend over the next 15 years. Two distinct forces contribute to this increase: first, we show evidence that young women delayed marriage, removing these marriages from the market in the early years of the policy, and contributing to older marriage ages as time went on. Secondly, older women faced an easier marriage market, leading to an increase in older-age marriages. When using the Arab control group, we obtain slightly larger overall effect sizes, consistent with the idea that men's marriage age may have also been pushed up in equilibrium by women delaying.

The mechanism we propose for this effect is an increase in the marital surplus from marrying at an older age, due to the protection against possible infertility. Delaying marriage has the benefit of being able to make greater career investments,[4] as well as being able to search longer for a more

rather than "secondary," meaning age-related, infertility, and thus does not affect time horizons.

[4]The literature indicates a substantial family-career tradeoff (Loughran and Zissimopoulos, 2009; Buckles, 2008; Blackburn, Bloom and Neumark, 1993; Taniguchi, 1999; Gustafsson, 2003; Miller, 2011; Avellar and Smock, 2003; Wilde, Batchelder and Ellwood, 2010; Adda, Dustmann and Stevens, 2017; Kleven, Landais and Sogaard, 2016; Angelov, Johansson and Lindahl, 2016; Lundborg, Plug and Rasmussen, 2017).

suitable mate. However, this comes at the cost of an increased risk of infertility, which affects women's own utility in addition to making them less attractive to a potential spouse.[5] Using tax data on spousal income, we show direct evidence that older women's marriage matches improved following the policy change, supporting the idea that protection against infertility increases the surplus associated with marrying when older.

To rule out potential alternative explanations for our findings, we employ a number of robustness checks and placebo tests. We first use a Quandt-Likelhood Ratio breakpoint test and event-study analysis to pin down the precise timing of the change, showing that any alternative event would need to have an impact precisely at 1994. We also use the United States and an earlier round of Israeli census data to show placebo tests, and use a permutation analysis to demonstrate that our effects are uniquely empirically large. Finally, we show a striking echo of our effect among the Israeli population living in the United States: women who moved to the US before their marriage, and thus were impacted by the marriage market in the US, show no effect, while women who married in Israel and then moved to the US show an increase in age at first marriage starting precisely at 1994.

Our research builds on previous work that exploits variation in mandated insurance coverage of IVF across US states and over time, showing that IVF materially impacts the fertility of women who use it (Velez et al., 2014; Hamilton and McManus, 2012; Bitler and Schmidt, 2012, 2006; Bundorf, Henne and Baker, 2007; Buckles, 2013; Schmidt, 2007, 2005). This line of literature provides 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 uses the same state-year variation to explore the impact on the timing of marriage and childbearing, supporting the hypothesis that access to infertility treatments may influence the decisions of younger women. Unpublished work by Ohinata (2011) finds that ART mandates resulted in 1-2 year delays in first birth among highly educated white women, and Abramowitz (2017, 2014) shows that increased access is associated with marriage and childbearing delays for white women. Furthermore, an unpublished work by Buckles (2007), finds some evidence for a possible channel of impact on career choices, presumably enabled by the delay in family formation.[6]

These studies are based on a DID strategy over a long time period, not accounting for possible differential time trends between states that did and did not implement coverage mandates. Moreover, the approach of using state-year variation in IVF coverage mandates has limitations, especially when discussing general equilibrium shifts in perceptions of both men and women. Since

[5] Low (2017) presents experimental evidence on the impact of fertility on marriage markets, showing that when age is randomly assigned to dating profiles, men, but not women, prefer younger partners.

[6] The findings indicate that infertility insurance mandates led to increased labor force participation for women under the age of 35 (but a decrease for older women). In addition, Kroeger and La Mattina (2017), present evidence on changes in occupational choices towards "more professional" careers for college educated women who were younger than 35 at the time of the change.

these are small and localized policy changes, awareness may not be widespread, particularly with young women who may not even be managing their own insurance yet.[7] Nonetheless, the fact that these papers find effects even with this more limited variation suggests an important potential contribution in testing the hypothesis using a more discrete policy event.

The Israeli policy change provides a unique opportunity to study a large-scale event that could have affected not only the actual chance of older motherhood but also the *beliefs* of both young women considering marriage delays and their potential partners. Our findings are important since they demonstrate that the beneficiary population of assisted reproduction technologies extends far beyond the women who actually encounter infertility. We show that slowing down the biological clock for women has a transformative impact on the marriage market, changing not only the timing of marriage, but also the composition of couples. In related work, we show that the women impacted by the policy change also pursued greater education and ended up in higher-earning careers (Gershoni and Low, 2018). Thus, policies that alleviate the career-family tradeoff for women can have far-reaching impacts.

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

2 Setting and Empirical Approach

2.1 IVF in Israel

Although Israel was an early leader in IVF technology[8], usage of the technology in the early 1990s was still relatively low, and technological advances were slow in coming. IVF treatments were covered at least to some extent by Israel's four main health plans,[9] but in practice, couples often had to pay substantial fees to access services.[10] The extent of coverage and terms of eligibility varied between health plans and, in many cases, were vague or a priori undetermined.[11]

[7]More generally, because mandates may increase insurance premiums for the most affected workers, in equilibrium they could negatively affect their wages and employment. Lahey (2012) presents evidence on infertility mandates suggesting that wages fall to compensate for higher premiums, but because they do not fully offset costs for women in affected age groups, employment opportunities (and thus labor force participation) for this group decreases.

[8]The first Israeli "test tube baby," born in 1982, was only the fifth IVF birth worldwide

[9]The four health plans were partially subsidized by the government, but mostly relied on the membership fees they collected. Approximately 5% of the population, had no health insurance at all.

[10]See, for example "We will have to forgo having a child since we cannot afford fertility treatments," *Yedioth Ahronoth*, June 14, 1992

[11]The most generous coverage was offered by the largest health plan ("Clalit"), which placed almost no limitations on usage, but due to difficulty tracking treatments, rather than official policy (Birenbaum-Carmeli, 2004). The other health plans offered a limited number of treatment cycles and placed age restrictions on use, as well as requiring long qualification periods. For example in the "Leumit" health plan the number of treatment cycles was limited to six and the maximal age was 40. Membership in health plans was mostly based on political affiliation and parents' membership, and switching between them was very difficult.

In 1994, following a widely covered, public debate, the Knesset enacted the National Health Insurance Law (NHI), which included IVF tests and treatments in a “basket” of free health services that all health plans must provide. The law provides all Israeli citizens with guaranteed access to:

> “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.”

The law, as originally written, did not place any restrictions on the age of women, or the number of attempts that could be made, and provided coverage for up to two “take-home babies.” This is in stark contrast to most IVF coverage policies, which usually entitle beneficiaries to a certain number of *treatments*, rather than a certain outcome. The 1994 law thus provided access to IVF that is unmatched anywhere in the world, ushering in an era of expanded usage and technological improvement.

Importantly, the passing of the law was driven by a pro-natalist agenda, rooted in the Jewish tradition of familism, rather than pro-women or “feminist” impulses which may have carried other effects.[12] This is exhibited in the Israeli supreme court’s ruling that the provision of IVF by the state is justified because becoming a parent is a fundamental human right.[13]

The new and unique Israeli funding policy facilitated fast adoption and increased usage of fertility-enhancing technologies. Figure 1 shows that the number of IVF treatment cycles (both total and relative to the population of fertile women) more than doubled in the 6 years following the approval of the new policy.[14] 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 of IVF availability.[15] The figure on the right hand side shows that in the year after usage increased, there was a sharp increase in live deliveries using IVF.

Figure 2 shows the direct impact on older women’s fertility, by measuring the increase in women over 40 with children under one year old. A large increase in older motherhood is apparent in 1995, immediately after IVF usage dramatically increased. This immediate change, amounting to more than 30% of the initial level of older motherhood,[16] persisted and kept expanding rapidly in the years that followed.

[12] For a thorough discussion of pro-natalist policies in Israel and their evolution over time, see Birenbaum-Carmeli (2003).

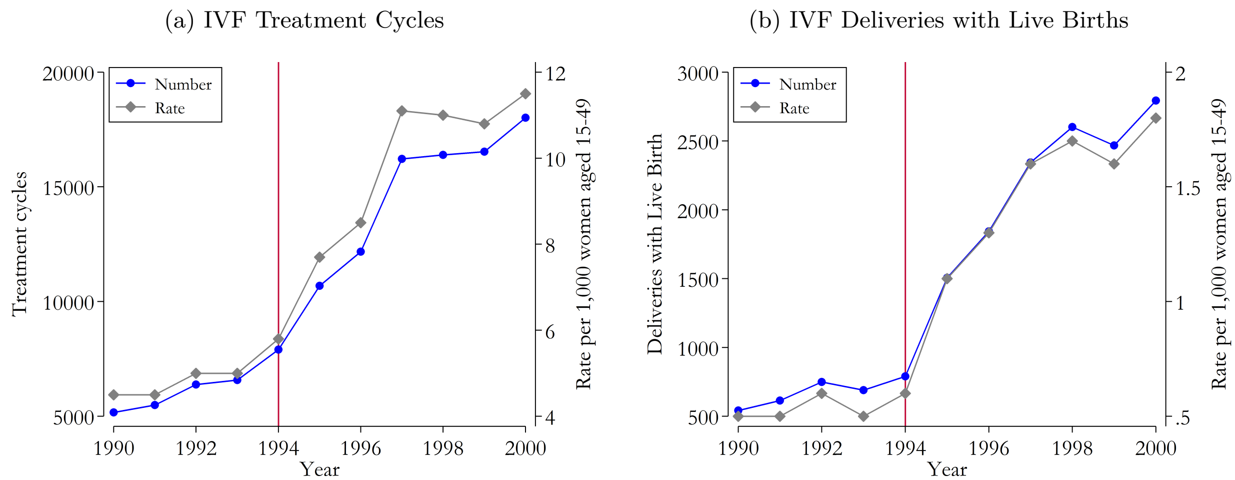
[13] See e.g. High Court 7052/03 Adalla vs. Ministry of Interior.

[14] 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. However, it is reasonable to assume that it is a result of a combination of these two.

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

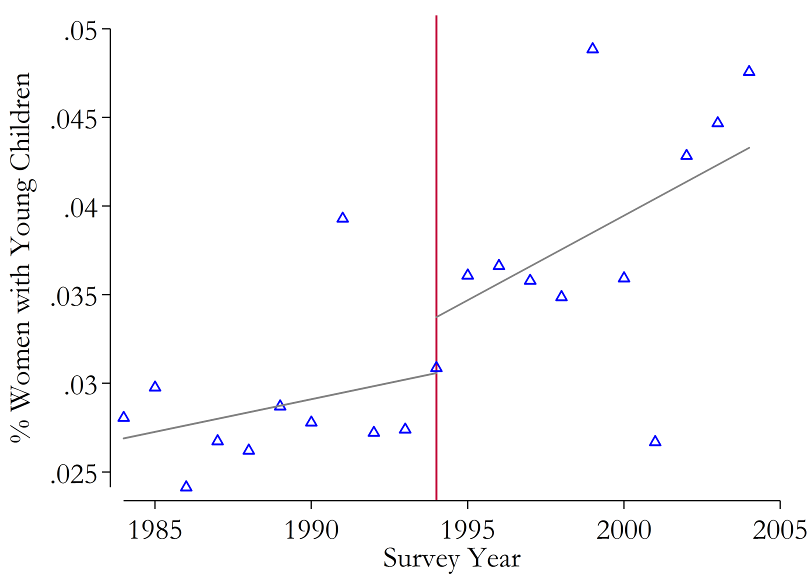
[16] We refer to 1993 as the pre-change reference since 1994 may have already been partially impacted

Figure 1: Direct Impacts of IVF Access



Notes: Administrative data from Israeli Ministry of Health, covering all women in Israel.

Figure 2: Percentage of Women over 40 with Children ≤1 year, Labor Force Survey



Notes: The figure presents the percentage of women above age 40 (>40 and <=47, since in practice very few women above 47 have young children) with children of age 1 or below. Data from the Israeli Annual Labor Force Survey 1984-2004, restricted to Israeli-born Jews.

As a result of this shift, the media was flooded with IVF success stories, such as extreme cases of women having children at advanced ages, further raising awareness of the new technology.[17] Local success stories were celebrated as "national accomplishments and symbols of local scientific excellence" (Birenbaum-Carmeli, 2004). The IVF law itself was also heavily covered in the press, and continued to be covered as debates ensued on whether to limit coverage.[18] Both 1994 and 1995 saw a spike in newspaper coverage of IVF and funding issues surrounding it.[19] Advocates of the health reform, including the minister of health, publicly touted the benefits of IVF funding in interviews as a reason to support the policy change.[20]

In the years following the policy change, there was expanding access to IVF services, and a standardization of practices surrounding IVF and its funding.[21] Nowadays, there are 26 IVF clinics spread throughout Israel, making treatment very easily accessible for most residents of Israel. Israel has become 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).[22] Three forces: improved access, technological innovation, and publicity, reinforced each other, driving a rapid and ongoing change in Israelis' attitudes and perceptions regarding IVF success rates, and thus the fertility time horizons for women.

Hashiloni-Dolev, Kaplan and Shkedi-Rafid (2011) surveyed undergraduates in 2009, and found that there was wide awareness of age-related fertility decline and of IVF. Appendix Table A1 presents an estimated comparison between perceived natural fertility versus IVF-enabled fertility for women of different ages, constructed based on this survey. It shows that students believed that the addition of IVF technology made 36-39 year-old women as fertile as those age 20-35, demonstrating the belief that this technology allowed a substantial delay in commencing childbearing without a decrease in expected fertility.[23] The survey respondents also tended to over-estimate IVF success rates, especially at an older age.[24] This provides important evidence that even college-aged women and men took IVF into account when thinking about future fertility horizons.

[17] For example, "World record: woman aged 60 gave birth to girl," *Yedioth Ahronoth*, February 22, 1994.

[18] 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).

[19] Based on a news search of Israel's leading Hebrew newspaper, 1994-1995 saw the largest number of articles on fertility, including success stories, reports about the law and the arrangements surrounding it (e.g. how to fund treatments for couples with different HMOs)

[20] See for example an interview with the minister of health Dr. Ephraim Sneh, *Yedioth Ahronoth*, December 15, 1994.

[21] 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).

[22] Compared to approximately 1-2% of the children born in other countries where IVF use is prevalent. The annual number of IVF cycles per million persons in Israel is the highest in the world and amounts to almost 3,500, compared to 2,000 in Denmark, which is second (Birenbaum-Carmeli, 2010).

[23] There is no similar study prior to 2009, so we are not able to offer direct evidence that beliefs evolved over time.

[24] For example, students believed that IVF would be 32% effective for women 40-43, whereas the true rate is around 20%.

2.2 Empirical Approach and Data

We now lay out our strategy to examine the impact of this discontinuous change in IVF access on the marriage market. As previously mentioned, the 1994 Israeli IVF policy applied to all Israeli citizens. This provides the advantage of potentially shifting widespread beliefs about reproductive time horizons, but the disadvantage of not providing statutory variation for identification. We thus employ a difference-in-differences strategy, comparing groups which are expected to be more versus less affected by the policy, within the country. We use two different control groups and a number of robustness and placebo checks to present a collage of evidence that the policy indeed causally impacted the outcomes we examine. We consider 1994 to be the first year of the treatment period, as our treatment is knowledge of IVF availability in the future and the resulting change of expectations, rather than the actual funding change.

Our data comes from the 2008 Israeli population census, covering approximately 20% of Israeli households.[25] This data is combined with administrative tax data, to accurately measure income. Our analysis is restricted to native born Israelis, to avoid potential bias from substantial immigrant inflows over time, including the mass migration from the former USSR during the late 80s and early 90s. Our "treated" group is Jewish-Israeli women, who are most likely to be responsive to a change in IVF access.[26]

We begin our analysis by comparing Jewish women's outcomes to Jewish men's outcomes because marriage ages—both the average and the distribution of ages—for these two groups have moved in parallel for a substantial period of time prior to the policy change. This makes intuitive sense, as Jewish men and women in Israel largely marry one another, and thus are similarly affected by shocks that change marriage patterns. In addition, both face similar economic conditions and experience the same demographic and labor market shifts. The challenge in this tight co-movement is of course that any change to women's marriage behavior may also impact men's decisions. However, IVF clearly has a differential impact on women, since it directly extends their later life fertility, an impact that men would only experience indirectly through their future partners. Women would not only experience a differential change to their own planning horizons, but also an update to their marriage market possibilities, since their appeal to partners who value fertility would change. Thus, if we can show a disruption of the co-movement between men's and women's outcomes exactly in 1994, it would present strong evidence of a causal impact of IVF on women's decisions about marriage, despite the numeric estimates perhaps being distorted by equilibrium impacts of the policy change on men.

More importantly, part of our hypothesis is that time-limited fertility affects women differently than men. For this to be the case, it has to constrain women's choices and options (e.g., in terms

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

[26]Judaism permits all forms of ART, whereas Islam places restrictions on certain practices and the Roman Catholic church bans it entirely (Birenbaum-Carmeli, 2003).

of available marriage partners at different ages) more than it constrains men through equilibrium effects. We therefore expect IVF to affect women differently than men, even taking into account equilibrium effects. And, it is precisely this net impact that may be most interesting to measure, since it sheds light on how much limited reproductive time horizons act as a differential barrier to women.

To then ensure that the source of the changes we measure is truly IVF, and not another systematic change affecting specifically women in Israel around this time,[27] we turn to the Arab control group. Arab-Israeli women are much less likely to use IVF due to stronger religious restrictions on its use (Birenbaum-Carmeli, 2003), making them a suitable control group for our purposes.[28] Moreover, Arab women got married almost 3 years younger on average before the change, and therefore were much less likely to be on the margin for delaying marriage into a zone where fertility would be a concern.[29] One reason for this gap in initial age at first marriage is that, as opposed to most of the Jewish population of Israel, they are exempted from obligatory military service. Also, Arab women in Israel come from more conservative backgrounds, with more traditional gender norms (Danziger and Neuman, 1999), and at the time had much lower education levels and labor market participation rates.[30]

This group has the added advantage of being unaffected in equilibrium by Jewish women's marriage decisions, since generally Arab and Jewish Israelis are not in the same marriage market. However, that also means this group may not as effectively control for common shocks that affect the Jewish-Israeli marriage market, and may be affected by different shocks and trends (especially in light of the significant differences between the two populations, as we describe above). However, while this strategy may have its own potential confounding factors, they should be orthogonal to any issues presented by the male control group, and thus if we estimate similar effects using the two strategies, it is unlikely that they are caused by a single omitted factor.

To assess the plausibility of the assumption that the treatment and comparison groups' age at

[27] One example is an ongoing reform in Israel that increased access to higher education, especially for disadvantaged populations in peripheral areas, which could in turn affect marriage timing. This reform could have a gender specific impact if, for example, the cost of moving away from home to acquire education is higher for women relative to men. However, both Arab and Jewish women in such areas would be affected by this change.

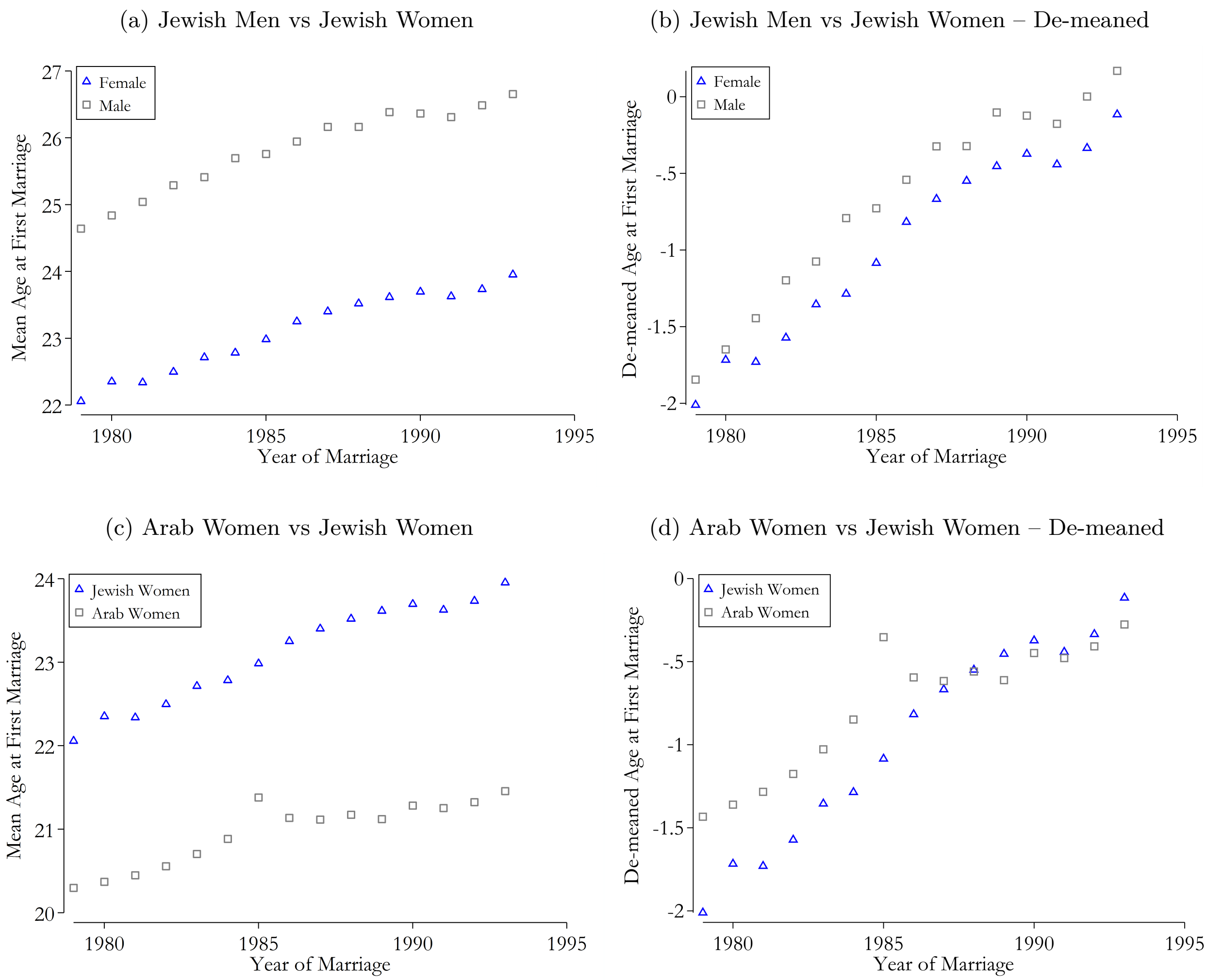
[28] Most Arab-Israelis are Muslim, and Islam places stringent restrictions on usage of assisted reproduction technology (ART), due to a particular requirement that the parental lineage can be traced to the married mother and father. This means IVF was viewed skeptically upon its introduction, and not immediately accepted by Muslim religious leaders, whereas Jewish religious leadership quickly approved of IVF and all accompanying technology. Even today, Muslim practices require IVF to be performed under stringent guidelines, and ban the use of donor eggs or sperm (Al-Bar and Chamsi-Pasha, 2015). As a result, IVF is seen as a last resort by the Arab-Israeli population (Remennick, 2010).

[29] According to annual data published by the Israeli central bureau of statistics, at 1993 (just before the policy change) the median age at first marriage for Arab-Muslim Israeli women was 20 compared to 23.3 for Jewish Israeli women (average age was 21.1 compared to 24). In the census data we also see a similar gap.

[30] At the baseline year 1993, there was a 25 percentage point difference in the rate of women's college attainment between Jews and Arabs, and a 10 percentage point difference in the same figures for graduate education. Labor force participation rates in the early-mid 90s ware approximately 13% for Arab-Israeli Muslim women compared to around 55% for Jewish-Israeli women (based on data from the Labor Force Surveys as reported by the Bank of Israel).

first marriage would move in parallel in the absence of the policy change, we plot the pre-period trends of for each group separately, over the 15 years prior to the policy change, 1979 to 1993. Figure 3 shows that men and women had strikingly parallel trends in age at first marriage during the pre-period, moving in lock-step and responding to common shocks. This is even more clear when, in panel (b), we artificially super-impose the two series by de-meaning each.[31] This may be partially attributable to the largely segregated marriage market of Israeli-born Jews—immigrants and other ethnic groups tend to marry within their own groups.

Figure 3: Alternative Control Groups – Pre-trends



Notes: Figure shows average age at first marriage for the treatment group – Jewish women and for two potential control groups: Jewish men and Arab women, by year of marriage, for the years prior to the policy change. Data from the 2008 Israeli population census, restricted to Israeli-born Jews.

For the Arab women control group, the pre-trends are not as parallel. This is not surprising,

[31] We use the mean age at first marriage in the year of the change, 1994, for each group, so for this year all demeaned values equal zero

Table 1: Summary Statistics

Marrying pre-1994:	Jewish women N=38,370		Jewish men N=33,949		Arab women N=14,901	
	Mean	SD	Mean	SD	Mean	SD
Ultra-Orthodox	0.09	0.28	0.09	0.29	N/A	N/A
European-born mother	0.24	0.43	0.28	0.45	N/A	N/A
Asian/African-born mother	0.54	0.50	0.52	0.50	N/A	N/A
Income in Shekels	95,629	92,393	186,757	173,543	53,299	49,999
Age	44.81	5.39	47.46	5.34	42.24	5.66
Age at first marriage	23.15	3.91	25.86	3.86	21.03	4.11
AFM pre-trend (SE)	0.13	(0.00)	0.14	(0.00)	0.08	(0.01)

Notes: 2008 Israeli population census (20% sample). Restricted to Israeli-born. Sample "marrying pre-1994" is those married 1979 - 1993, inclusive.

due to the fact that the two groups of women are not in the same marriage market, and may face different cultural forces. Moreover, the Arab population is smaller, and therefore shows more random variation. For these reasons, Arab women are not our preferred control group. However, we can see in panels (c) and (d) of figure 3 that the trends for these two groups become approximately parallel by the end of the 1980s. Thus, we shorten the pre period when using the Arab female control, and use only the years from 1988 onward (the estimated effects are not sensitive to our exact choice of pre period).

Table 1 shows summary statistics for our sample, comparing Jewish women to Jewish men (our main comparison) as well as Arab women, in the pre-change period. In addition to showing means for our key outcomes and controls, Table 1 compares pre-trends in outcomes for the different groups.

For both control groups, we first estimate a basic difference-in-differences specification, that measures the average change in Jewish-women's age at first marriage (AFM) relative to the comparison group, between the "pre" and "post" periods, according to the following equation:

$$AFM_i = \beta_0 + \beta_1 T_i + \beta_2 post_i + \beta_3 T_i \times post_i + \beta_4 time_i + X_i' \gamma + u_i,$$

where T is a dummy for Jewish-female, *post* is a dummy for marriage years 1994 and onwards (up to 2008), X is a vector of individual level controls, which includes indicators for religiosity (ultra-orthodox or not) and parents' origin (Europe and America, Asia and Africa, or Israeli born), to account for demographic shifts over time, and *time* is a linear time trend.

We then test for both 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 time:

$$\begin{aligned}AFM_i = \beta_0 + \beta_1 T_i + \beta_2 post_i + \beta_3 T_i \times post_i + \beta_4 time_i \\ + \beta_5 post_i \times time_i + \beta_6 T_i \times time_i + \beta_7 T_i \times post_i \times time_i + X_i' \gamma + u_i\end{aligned}$$

We repeat the estimation of both equations adding year of marriage fixed-effects, to more flexibly control for time trends and account for transitory shocks that may affect the marriage market.

We use two alternative methods to calculate standard errors. The first method has the advantage of accounting for cross-sectional correlation in outcomes as we cluster at the year $\times$ group level—this captures the most important source of correlated shocks, those that affect a certain group as a whole (Jewish women, Jewish men, Arab women) marrying in a certain year. Our second set of standard errors aims to deal with potential serial correlation in the outcome variables, which could lead to over-rejection of the null hypothesis in a DID framework (Bertrand, Duflo and Mullainathan, 2004). We divide our sample into sub-groups, based on the standard classification of Israel into 51 "natural regions" and cluster at the geography $\times$ group level (examples of similar sub-group clustering can be found in Agarwal et al. (2014), clustering at the product level, and Hanlon (2015), clustering at the patent level). Since the regions are defined to create homogeneous areas, both in terms of geographical characteristics and in terms of demographic and economic traits of their population, we expect any shocks related to women's access to education or health services to occur within these regions. Moreover, structural shifts in labor and marriage markets, which directly affect our outcomes of interest, are likely to have significant regional components. To further address the concern for serial correlation, we then re-estimate our main specification for each outcome with an explicit AR(1) error structure, by collapsing our data into year-group cells and estimating via Generalized Least Squares (GLS), allowing for correlation both *across* and *within* panels (as in Chandra, Gruber and McKnight (2010)). The within-panel correlation factor accounts for serial correlation, assuming an AR(1) process with a unique autocorrelation parameter for each panel (either gender or ethnic group). Finally, in the appendix, we show permutation tests for each of our main results, demonstrating that our effects are "large" relative to the actual variation present in the data.

We present a number of robustness checks and placebo tests to verify that the results stem from the 1994 policy change. We then separately examine the impact on marriage rates of women at different ages, to study how the distribution of age at first marriage changed. We also look at how marriage matches appear to have been impacted by the policy change, by comparing older women's marriage outcomes to younger women's.

3 Results

Theoretically, we would expect two distinct, yet related, impacts of extended reproductive time horizons: an improvement in the marriage market for older women, and an increase in the number of women delaying marriage. In a simple search framework, the expected surplus of matching with women on the older side of the marriage market increases, since their expected fecundity is higher, and thus these women should be able to match more quickly to higher quality partners. At the same time, the reduced fertility penalty increases the value for younger women of delaying marriage to either continue searching for a better partner or engage in other activities, such as education.

Both effects are likely to reinforce one another, as some young women might delay at first because of the increased attractiveness of career opportunities now that *their own* concern for their fertility is lessened, and then others be encouraged to delay by the improved marriage market chances for older women. In turn, young women delaying, and therefore being less available on the marriage market, adds to the increase in the attractiveness of older partners. Note, even women unaware of the policy change itself might thus be affected by the shifting terms of the marriage market for those who delay. These forces predict not only an immediate impact, but a feedback loop that creates a positive trend over time.

We analyze these potential impacts by examining how the average age at marriage, the distribution of marriage age, and spousal matches for older women changed following the 1994 policy.

3.1 Age at First Marriage

3.1.1 Average Effects for Women Compared to Men

The primary outcome we consider is age at first marriage. We first plot the raw averages for age at marriage over a 30 years period, before and after the change, in order to visually inspect the trends and differences between the groups.

If indeed the marriage market improved for older women, due to higher expected fertility, we would expect more older women marrying, pulling up the average age at marriage. Similarly, if young women chose to delay marriage both in response to the improvement in later-life fertility prospects and the aforementioned improvement in marriage market possibilities, this would also increase marriage age in the long run. However, the immediate effect of women delaying marriage is ambiguous, and depends on the age of the women delaying. If women delay who are older than the average age at first marriage, the average could go down at first, before those women who delay rejoin the marriage market in later years, pushing the average up. If, however, the women who delay are younger than average, average age at first marriage would go up immediately.

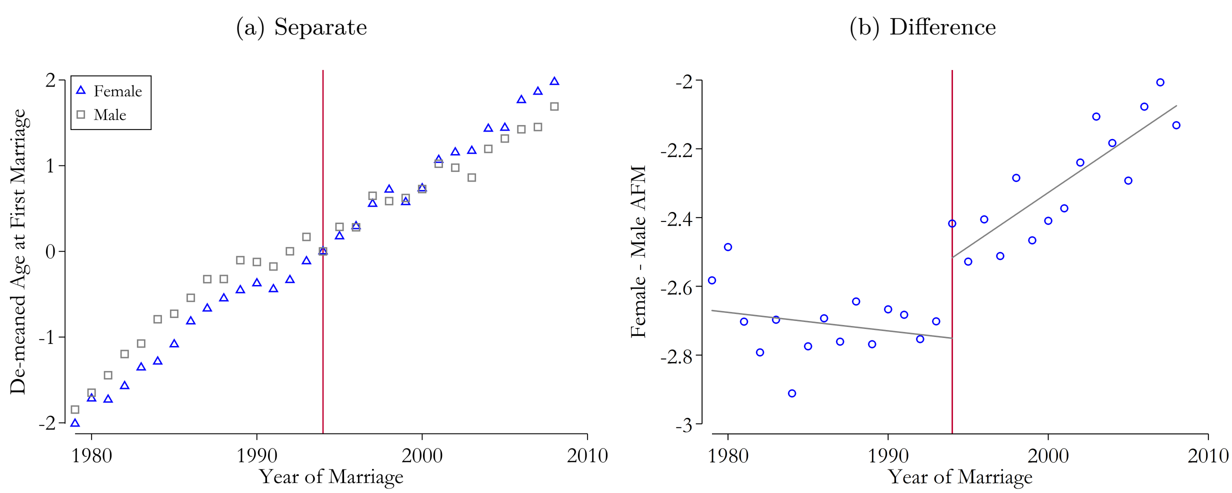
Note that because we are comparing to men, we should use caution in interpreting our numeric estimates, as women changing their marriage behavior may also influence men's age at marriage.

However, given that the age gap between spouses can adjust, men's marriage age should be less affected than women's, and, as previously discussed, this net effect is economically relevant.

Figure 4 clearly shows that pre-trends for men and women were parallel, in the left-hand figure that graphs outcomes separately (with age at first marriage de-meaned), implying that women's marriage age was practically constant relative to men's until 1994. Men and women's age at first marriage also appears to similarly respond to shocks in the pre-period. Starting in 1994, women's marriage age increases discontinuously relative to men, and trends diverge, as women's average age keeps increasing relative to men's. In 1994 itself, men's age at first marriage appears to decline and, for the first time in this year range, women do not respond in tandem. Thus, we conclude that relative to the common shock, women's age increases. The apparent change in men's outcomes could either be a random shock, or could be a result of an equilibrium shift due to certain women choosing to delay marriage, a possibility we discuss more in Section 3.2.

When we graph the difference between men and women on the right-hand side, we clearly see a sharp increase in level immediately at 1994, followed by a substantial positive change in trend. This striking discontinuity in age at first marriage is unprecedented in Israel, is unique amongst other countries over the same time period, and has no clear explanation other than the introduction of free IVF. Note that the immediate increase of women's age at first marriage relative to men's suggests that either the women delaying marriage were of average age or younger, or there was a sufficient increase in marriages by older women to balance out any delays by women older than average. We show the distribution of impact in more detail in Section 3.2.

Figure 4: Female vs. Male Age at First Marriage



Notes: Figure (a) shows average age at first marriage for women and men, by year of marriage, de-meaned so that the relative changes can be seen more clearly. Figure (b) presents the difference in average age at first marriage between women and men, as well as fitted lines for the pre (1979-1993) and post (1994-2008) periods. Data from the 2008 Israeli population census, restricted to Israeli-born Jews.

Table 2 analyses this change using a regression, in both a simple DID framework (columns 1 and 2), and an analysis demonstrating the change both in level and trend (columns 3 and 4). The latter allows us to estimate an increasing change in the outcome over time, which correlates with the gradual adjustment to the new possibilities and expectations, rather than just a one-time jump. We find that women's age at marriage jumps by approximately three months, relative to men, immediately following the policy change, and continues to increase steadily thereafter.

The estimated discontinuity as well as the estimated slope change are positive and highly significant in all specifications, when we use the year-group clustering. In the Generalized Least Squares (GLS) specification, where the standard errors are adjusted to account for cross-sectional correlation and serial correlation, assuming a panel-specific AR(1) process, estimates and standard errors are of similar magnitude. When we use the geographical clustering, standard errors generally increase, however the p-values remain around 0.06.

Consistent with the idea that for men's and women's age at marriage to diverge from the common trend, the age gap between spouses must adjust, the raw data shows the age gap between spouses changed by about 3.5 months (from 3 to 2.71 years) over the first three years of the policy change, and by 0.6 years over the 15 years following the policy change (compared to 0.007 since 15 years prior to the benchmark year 1993).[32]

3.1.2 Confirming a Trend Break in 1994

Quandt Likelihood Ratio breakpoint test To confirm that what we are picking up is truly a discontinuous shift in age at first marriage—a break in the time series—rather than more gradual time trends, we perform a Quandt Likelihood Test to "search" for the most likely break year in the data, over our entire sample period except for the standard 15% "trimming" on either end, to account for limited data at the beginning and end of the sample period. To implement the test, we run a loop of regressions identical in specification to our columns 3 and 4 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, and search for the maximal F-statistic among all years.

As shown in Table 3, the test returns the highest F-statistic for 1994, which stands out as other years are more similar to each other and have distinguishably lower values. This indicates that the year of the policy change and hence our treatment year is the most probable break year. Moreover, this break is significant even when accounting for the multiple tests used to identify it. 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 10.38 or 10.78, depending

[32] Numbers from the 2008 Israeli population census, with matches between spouses, and thus are for couples that "survive" into 2008 only.

Table 2: Age at First Marriage

	Dependent variable: Age at First Marriage					
	DiD		Slope-Change DiD		GLS Slope-Change DiD	
	(1)	(2)	(3)	(4)	(5)	(6)
fem × post	0.412	0.415	0.241	0.246	0.247	0.195
	(0.073)***	(0.036)***	(0.127)*	(0.044)***	(0.082)***	(0.058)***
	[0.221]*	[0.220]*	[0.135]*	[0.133]*		
fem × post × time			0.039	0.040	0.035	0.036
			(0.013)***	(0.005)***	(0.010)***	(0.007)***
			[0.021]*	[0.022]*		
fem × time			-0.008	-0.008	-0.009	-0.007
			(0.012)	(0.003)**	(0.008)	(0.005)
			[0.015]	[0.015]		
post × time			0.001		-0.002	
			(0.010)		(0.013)	
			[0.014]			
post	-0.440		-0.322		-0.395	
	(0.084)***		(0.091)***		(0.108)***	
	[0.141]***		[0.079]***			
female	-2.649	-2.651	-2.710	-2.715	-2.783	-2.757
	(0.059)***	(0.016)***	(0.114)***	(0.031)***	(0.067)***	(0.045)***
	[0.236]***	[0.235]***	[0.253]***	[0.250]***		
time	0.176		0.168		0.138	
	(0.004)***		(0.008)***		(0.010)***	
	[0.011]***		[0.011]***			
Constant	26.319	23.947	26.251	23.521	26.856	24.628
	(0.070)***	(0.078)***	(0.096)***	(0.063)***	(0.088)***	(0.022)***
	[0.198]***	[0.187]***	[0.188]***	[0.185]***		
YOM FEs		YES		YES		YES
Observations	167416	167416	167416	167416	60	60
R-Squared	0.246	0.246	0.246	0.247		

Notes: Columns 1–4: Ordinary least-squares difference-in-differences regression using micro data, including controls for religiosity and parents' origin. Robust standard errors clustered at the gender × year level in parentheses; robust standard errors clustered at the gender × geography level in square brackets. Columns 5–6: Generalized least squares regression with data collapsed to the gender-year-of-marriage level. Robust standard errors that allow for cross-sectional correlation and for panel-specific serial correlation (i.e. estimate a unique autocorrelation parameter for each group) , in parentheses. Data from the 2008 Israeli population census, restricted to Israeli-born Jews.

*** p<0.01, ** p<0.05, * p<0.1

on whether fixed effects are used or not.[33] This shows strong evidence of a break specifically at 1994.

Table 3: Quandt Likelihood Ratio test for break point

	F-Statistic	
Year of Marriage	No FEs	With YoM FEs
1983	7.05	7.40
1984	7.28	7.67
1985	7.29	7.69
1986	7.40	7.85
1987	7.83	8.27
1988	8.06	8.35
1989	8.12	8.36
1990	8.09	8.27
1991	8.03	8.16
1992	7.83	8.11
1993	8.57	8.91
1994	**10.38**	**10.78**
1995	7.19	7.49
1996	6.49	6.75
1997	5.61	5.86
1998	5.97	6.14
1999	3.95	4.07
2000	4.52	4.69
2001	4.02	4.16
2002	3.95	4.03
2003	2.60	2.64
2004	1.03	1.04

Notes: Table reports F stats from a regression according to the specification in columns 3 and 4 of Table 2, where the hypothesis is that the coefficients on $post \times fem$ and $post \times fem \times time$ equal 0, and "post" is defined as being greater than or equal to the indicated year. Standard errors are not clustered in this case, as clustering is not conventional in QLR models, but similar results are obtained with clustering.

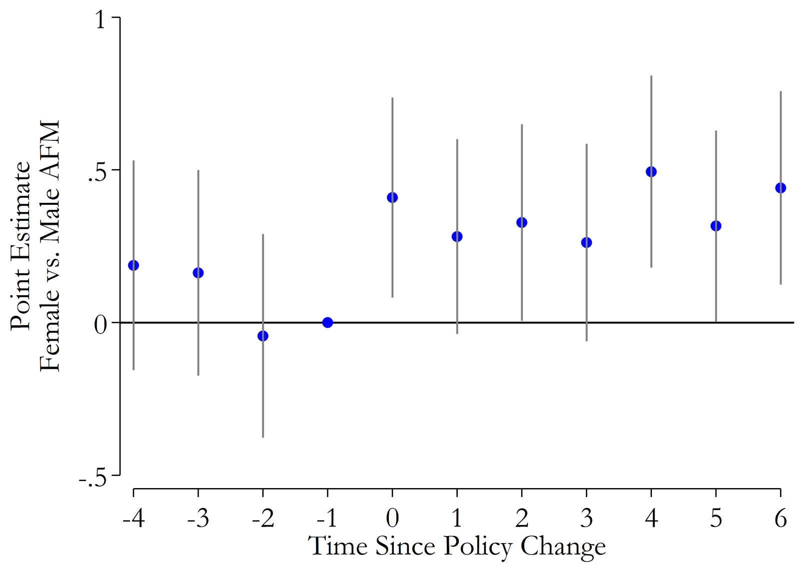
Event Study analysis To further verify that long term time trends are not responsible for the effect we see, we perform an event study analysis (also known as dynamic lag analysis), to pinpoint the timing of the changes we observe.

The event study graph depicted in Figure 5 is created by regressing our key outcome variable on a series of dummies for each year, interacted with gender and including the same controls as our

[33]Note, some of the other F-stats are still large, as is common in QLR tests where there is indeed a break in the data, so moving additional data points to either side of the "break" does not completely eliminate its statistical impact on the test.

regression specifications. Because *each* year of the entire sample period is allowed to have a group-specific effect, this is a demanding test. The coefficients graphed represent the differential effect on women (compared to men) for each time period relative to the last year of the pre-change period. This analysis shows no significant differences for the years prior to the policy change, whereas in the year of the policy change, there is a large and permanent change to subsequent outcomes. This confirms that the increase in women's average age at first marriage is not driven by pre existing trends.

Figure 5: Event Study Analysis



Notes: The figure presents dynamic lag event study analysis for the impact of the policy change on age at first marriage for women relative to men. Point estimates and confidence intervals are for the coefficients on yearly dummy variables interacted with an indicator for women (treatment group). Time is year of marriage, with 1994 as time "zero." The regression equation includes all year-group interactions for the entire sample period, as well as a linear time trend and demographic controls. All regressions restricted to Israeli-born Jews.

3.2 How the Marriage Market was Reshaped

3.2.1 Distribution of Age at First Marriage

After establishing that women's average age at first marriage significantly increased relative to men's, we turn to explore more thoroughly what happened to the distribution of age at first marriage. This analysis sheds light on two important issues. First, we would like to better understand if the differential increase in the average is driven by a decrease in marriage rates of younger women who choose to delay marriage due to the alleviation of later life fertility constraints, or by an increase in marriage rates for older women who could not find a match before the change, or both. This in turn will facilitate a more comprehensive understanding of how we should interpret the main results, recognizing that men are affected in equilibrium by the change in women's marriage timing.

To define the specific age range that should be considered as "younger" or as "older" with regards to our research question, we need to consider the distribution of age at first marriage in Israel at the time. As Israeli women marry on average three years younger than in other OECD countries, the fulcrum point for who is on the "older" side of the marriage market is very different in this setting. Of those who married for the first time in 1993, 71.36% were younger than age 25, and 98.42% were 35 or younger. This is partly due to higher fertility rates—having more children requires women to start families earlier, if they want to avoid age-related infertility. On average, Israelis have approximately 1.5 more children than in other OECD countries. This means that women in their late twenties are already on the "older" side of the Israeli marriage market.

First, we show in Figure 6 that the same discontinuity we observe for average marriage age, is present if we compare the proportion of men and women marrying at or below the (approximate) average age at marriage for each in 1993. In panel (a), in the pre period, we see strongly parallel trends—though fewer men and women marry at a young age over time, the rates decline at an equal pace. Starting in 1994, there is a sharp divergence of the two series. In 1994, more men married below the 1993 average age than did women. The differences in this series, shown in panel (b), confirm a large trend break in 1994, demonstrating that men's and women's age at marriage departed in opposite ways from what had been the average age for marriage prior to the policy change. Because men and women were impacted differently, and thus the age gap between spouses changed, we are able to measure the differential effect of IVF on women compared to men.[34]

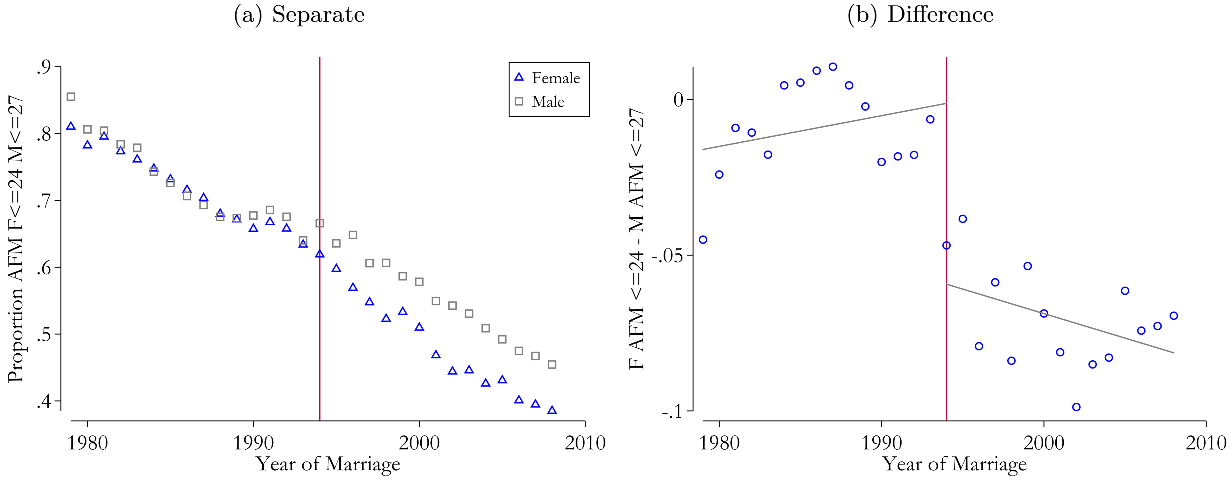
The fact that we capture a break in this measurement implies two key effects: It suggests that either some women who were marrying at younger-than-average ages delay to older-than-average ages or new women entered the marriage market at older ages, or both. That is, it suggests that the policy did not *only* shift women who already married older than average to marry at even older ages, as that would not have shifted the proportion marrying at the average.

Thus, we now turn to investigating the *counts* of marriages by age in the key ranges implied by the shifting proportion marrying at 24. Looking at counts can reveal whether the shift in proportion was caused by only an increase in older women marrying, only a decrease in younger women marrying, or both.

We look at those marrying at 24 and younger, average and below, and those marrying at 27 and older, who would be on the "old" side of the marriage market. We show as a reference the series

[34]This picture does not show as strong of a differential trend as does average age at marriage, because what is being measured is the proportion marrying younger than the 1993 average. If women who already marry above the 1993 average continue to marry at older ages, this would increase average age overall, but not the proportion above average.

Figure 6: Female vs. Male Proportion Married at or below 1993 Average Age at First Marriage



Notes: Figure (a) shows the proportion of women and men married at or below the average age at first marriage, by year of marriage. Figure (b) presents the difference in this proportion between women and men, as well as fitted lines for the pre (1979-1993) and post (1994-2008) periods. The average age at first marriage is 24 for women and 27 for men. It is calculated for 1993, just before the policy change, and rounded to full years in order to fit the way age is calculated in the data. Data from the 2008 Israeli population census, restricted to Israeli-born Jews.

of counts for men three years older than the ages for women, assuming that if there is a common trend towards older marriages, it will appear for men and women with this age gap, as this is the average age gap between spouses pre-change. Indeed, although these series are somewhat noisy, since variation in cohort size affects the number of marriages each year, the figures show that pre trends were roughly parallel.

We first look at women on the young side of the marriage market, zeroing in on those marrying at or below the 1993 average age of 24. These are women who might have married before completing college, or not attended at all, as women start college relatively late in Israel.[35] Given the average gap in marriage age, comparable men are those who marry at 27 and younger.[36] We see evidence, in Figure 7 panel (a), that the number of marriages in this group decreased relative to the pre-trend, indicating women in this group may have delayed marriage, and thus been removed from the “count.” Post 1994, men’s marriage numbers in this age group remained relatively stable, while women’s precipitously dropped.

If these women forego marriage, their partners might be affected as well, and thus we expect some “missing” men, too. However, we do not expect *all* of them to come from the less than 27 group, and hence the decrease for women should not necessarily be matched by men in this age group. This is even more pronounced, as the average age gap for spouses decreases with age, so the

[35] The median age for college entrants at the relevant period was 22.5 for women.

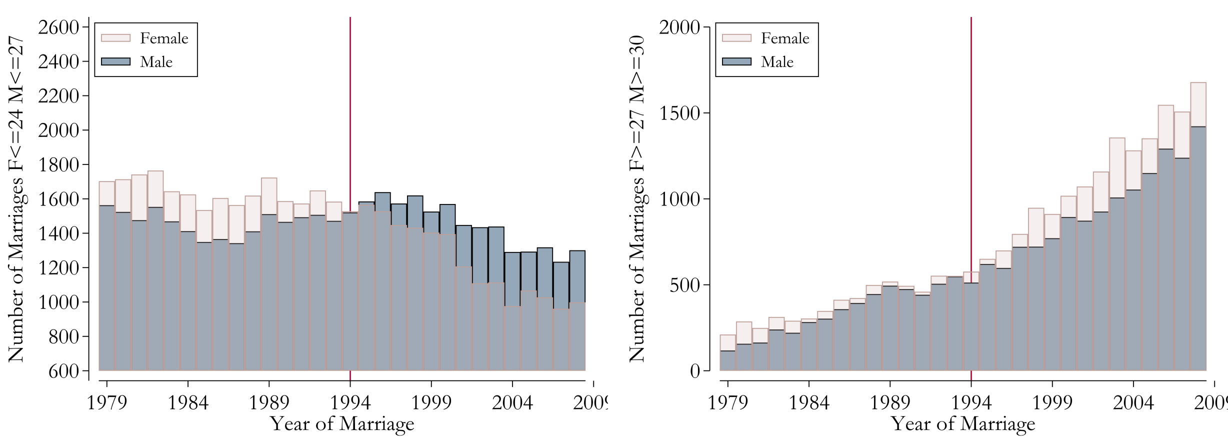
[36] Although 24 is the average age at marriage for women and 27 is the average age at marriage for men, because there is a long tail in men’s age at first marriage that is absent for women, there are fewer men marrying below 27 than there are women marrying below 24).

actual age gap for women younger than 24 is more than 3 years.[37] This fact could help explain the slight decrease in men's average age at marriage exactly in 1994, shown in Figure 4. If the women who delayed had slightly *older* than average partners, and their partners had to either wait with them or re-match, this would lead to a decrease in men's average age at marriage in the initial year.

Next, in panel (b) we examine women that are older than the bulk of the marriage market, and have potentially completed college before looking for a partner, those marrying at or above age 27, and men marrying at or above age 30. For this group, the pre-trends track one another extremely closely, and almost the same number of men and women are marrying in these age groups in the ten years preceding the policy change. Post 1994, however, marriage counts appear to go up for women at a much faster rate than they do for men. This shift appears as a gradual trend, rather than an immediate increase, consistent with the idea that these older women must first search for a mate, and then marry, which cannot occur immediately at a large scale.[38]

Figure 7: Number of Marriages in Different Age Groups

(a) Women<=24 and Men <=27 (b) Women>=27 and Men >=30



Notes: Figure (a) shows the number of women married at or below age 24 and men married at or below age 27, by year of marriage. We exclude the ultra-Orthodox population, most of which marry in this age group, since the large population growth they experience during this period adds irrelevant noise to these counts. Figure (b) shows the number of women married at or above age 27 and men married at or above age 30, by year of marriage. Data from the 2008 Israeli population census, restricted to Israeli-born Jews.

To further confirm how the entire distribution of marriage age was affected, we next look at the percentage of women who were married by certain ages over time, in a series of regressions. For each year we include in the analysis all individuals who were at this certain age or above it at this year (as opposed to previous results that only included individuals who actually got married that year). Thus, the pre and post periods are defined according to the year in which the individual

[37]From Census data.

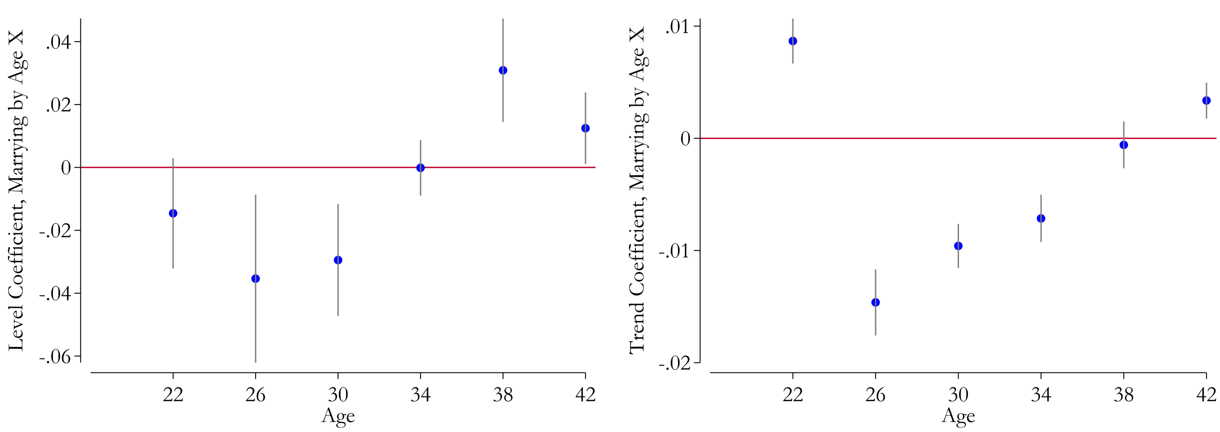
[38]Men's marriages in this group are slightly lower than the overall series (although it could be noise) exactly in 1994, indicating some of these men may have been partners of younger women who chose to delay, as mentioned above.

reached the specified age, since, for example, individuals who were already 22 before the time of the policy change, could not change their outcome (married by age 22) due to the policy. Hence, this analysis also captures the possibility of remaining unwed. As before, we use a DID specification, that allows for shifts both in levels and trends, with men as the control group. We add controls and flexibly control for group specific time trends similar to the column (4) specification in table 2.

Figure 8 shows the point estimates and confidence intervals for the impact of being in the treated groups after the policy change on both the level and trend of being married by a certain age. The estimates reflect the change in the percentage of women married by the age specified on the x axis. Figure 8(a) presents estimates for the immediate (and permanent) change in level and figure 8(b) shows the estimated change in trend. The two graphs show no decrease in marrying by age 22, which 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 to be affected by access to IVF. We see the largest reduction in marriage by age 26, and from there a steadily decreasing impact, until the total 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 by women delaying marriages from their mid- and late-twenties into their thirties and even late thirties. [39]

Figure 8: Regression Coefficients for Effect of IVF Law on Marrying by a Given Age

(a) Intercept coefficient (b) Slope coefficient



Notes: The figures presents the point estimates and confidence intervals of the coefficients on (a) the interaction term fem×post and (b) 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 (4) in table 2. Data from the 2008 Israeli population census, restricted to Israeli-born Jews.

[39]To test the robustness of this result, we present the same analysis in appendix figure A1 with men's age shifted by three years, to reflect the average age-gap between spouses. The main conclusions hold.

3.2.2 Spousal Matching

We expect the age at first marriage results are driven by two principal mechanisms—young women wishing to delay marriage in order to search longer for a spouse or benefit from a longer period of human capital investment, and older women facing a more favorable marriage market due to their increased expected fertility. We have already demonstrated that these two groups were indeed affected. In Gershoni and Low (2018), we demonstrate that beginning in 1994, women's education not only increased discontinuously relative to men, but women also ended up in more high-paying and prestigious careers, providing evidence that women who delayed partly did so to pursue greater educational investments.

In this section, we examine if "older" women indeed faced a more advantageous marriage market. If women's reproductive fitness is taken into account by men, we would expect to see a "spousal quality penalty" to older women and to see it lessen once access to IVF expands. We test this directly by examining the quality of women's spouses, measured in various ways, when they marry above age 30 compared to marrying below age 30, before and after the policy change. We restrict the sample to women who got married between age 25 and 34, to ensure that we compare relatively similar groups of women. We use age 30 as the cutoff between "younger" and "older" brides, assuming that expected fecundity becomes significantly more relevant when a woman hits her thirties.

One concern with this approach is that during this time period women are also delaying marriage, changing the selection of women in the "older" and "younger" groups. It could be that the couples that delay are the ones where men are of higher quality, or that women who delay have time to invest in their education, which makes them better able to attract a high quality spouse. We believe this exercise is useful to shed light on the mechanisms of impact, despite the selection issues, for several reasons. First, Low (2017) shows theoretically that if fertility is extended, the group that delays marriage would be *negatively* selected compared to women who married older before this extension. This works against finding an improvement in spousal quality. Second, even if matching was not affected and the same couples just postponed their marriages, this would practically mean the same thing—the same woman can marry older without being penalized on the marriage market versus before 1994. Similarly, if older women invest in their education and this yields higher quality spouses, it still shows an improvement relative to women of the same age prior to the policy change, whose educational investments were insufficient to make up for their older age. Furthermore, we show that the improvement in spousal income occurs even when we hold the observable characteristics of the wife constant.

Our first proxy for spousal quality is husband's income, which comes from administrative tax data,[40] as it is well established that income is an important quality that male spouses bring to

[40]Women are matched to their spouses in the data based on relationship to household head. We only have spousal income data for the current spouse, so this analysis excludes women who are divorced or widowed before the census

the relationship (see, for example, Fisman et al. (2006)). To control for age effects, since we use cross-sectional data, and women who marry at different ages may have spouses of different ages as well, we include a flexible polynomial in age as a control, and restrict our years to 1986 - 2003, to limit confounding effects from retrospective measurement. This restriction also ensures that we avoid distortions from including spouses who are very young and may be still completing school and career training, or very old and retired.[41] Due to the relatively narrow band of years, we use a standard DID specification (that does not measure the change in trend).

In the top panel of table 4, columns (1)- (4) present the results from a regression of spousal income on marrying older, before and after the policy change, controlling for spousal age with a flexible polynomial. In columns (3) and (4) we include additional controls for wife's characteristics — age, income and education — to ensure that the increase in spousal income is not a result of the improved quality of older brides themselves.[42] In addition, in columns (5)-(6), we use the income percentile of spouses to confirm that the results are not driven by non-linear trends of income over age and avoid distortions due to the long right tail of the income distribution.

The results confirm that there is a significant "penalty" in terms of spousal income for women who choose to marry over thirty, and that this penalty significantly decreases in the post period. In columns (1) and (2) we observe a reduction of approximately 100 percent in the "older marriage penalty" for women. In appendix figure A3 we shows how the residual of spousal income regressed on a flexible age polynomial evolves over time, by year of marriage. Starting in 1996, two years after the policy change was announced, we observe a distinct shift upward in spousal income for women who marry over 30, relative to the same measure for women who marry younger, which remains very stable over time. The fact that this break appears with a lag may be attributed to men's perceptions taking longer to update or simply to the fact that reaching this new matching equilibrium takes time.

To supplement this analysis, in the bottom part of table 4, we use three alternative measures of spousal quality that should be less sensitive to age effects: spouse's college education, full-time employment, and occupation (measured by participation in a "prestigious," high-paying field such as management, medicine, and law). All three measures of spousal quality show an increase in the post period for women who marry over 30 relative to those who marry younger.

Together, these results provide strong suggestive evidence that older women faced a more favorable marriage market after the policy change. They married men who were higher earning, more likely to be college educated, more likely to be employed, and more likely to be in a high-paying oc-

year.

[41]Note that restricting the range of marriage years, also restricts the age range for spouses at the year of the census. This year range was chosen based on an analysis of men's rate of full-time employment over age and the distribution of average husband's age by wife's age. Our qualitative results are not sensitive to the exact year-range chosen.

[42]Some of these covariates could be affected by treatment, and hence estimates for the main effect may be biased. Nevertheless, this specification shows that improvement in women's "quality" cannot be entirely responsible for the effect we observe.

cupation. Together with the results on marriage timing, this change in the matching equilibrium is consistent with women and men changing their beliefs regarding older women's fertility prospects.

3.3 Alternative control: Arab-Israeli women

Our identification strategy relies on the post-1994 time-path of men's outcomes being similar to women's in the absence of the IVF policy change. 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. Additionally, because men co-exist in the same marriage market with women, it may be difficult to use men to pin down the exact magnitude of the policy's impact, as women delaying marriage may influence men's marriage timing.

The advantage of using Arab-Israeli women as a control group is that they would have the same exposure to other forces that might influence women's age at marriage, e.g. higher education expansion.[43] As we describe in detail in section 2.2, Arab-Israelis were less likely (if at all) to respond to the change in IVF funding for two main reasons. First and foremost, most Arab-Israelis are Muslim and Islam places more stringent restrictions on the use of IVF than does Judaism. Second, the initially younger average age at marriage for Arab-Israeli women, as well as their extremely low labor market participation, makes fertility horizons a less material constraint in the first place. Also related to this point, is the small number of single "older" women who could potentially enjoy the benefits of IVF and improve their marriage market status. However, it should also be noted that some of these differences account for Arab women not being our preferred control group. The lower rates of educational attainment and labor force participation mean that there are important differences in the social forces influencing the two groups.

Figure 9 compares Jewish and Arab women's average age at marriage before and after the policy change, as we did in figure 4 for Jewish men and women. It is restricted to the pre-period during which the trends for Arab and Jewish women were found to be parallel. The figure shows a strong divergence in trends starting in 1994, with an increase of about 1.5 years for Jewish women's age at first marriage relative to Arab women's by 2008.

Table 5 confirms that Jewish-Israeli women experience a differential increase in age at first marriage, beginning in 1994, compared to Arab-Israeli women. Note, that the estimated effect is somewhat larger than the one reported in Table 2, which may be because Arab women are not affected in equilibrium by Jewish women postponing marriages, while Jewish men, the marriage partners of Jewish women, are likely to be. Thus, the Arab female control group may be useful in understanding the true effect size.

[43] In fact, Arab-Israelis were much more likely than Jewish-Israelis to be affected by the higher education reform, which gradually enabled colleges (rather than universities) to grant academic degrees, due to lower high-school achievements (on average) and higher concentration in peripheral areas. This effect is described in Volanski (2005) and also in various reports issued by the Israeli council for higher education (e.g. Higher education in Israel 2014, pp. 29-31).

Table 4: Spousal Quality

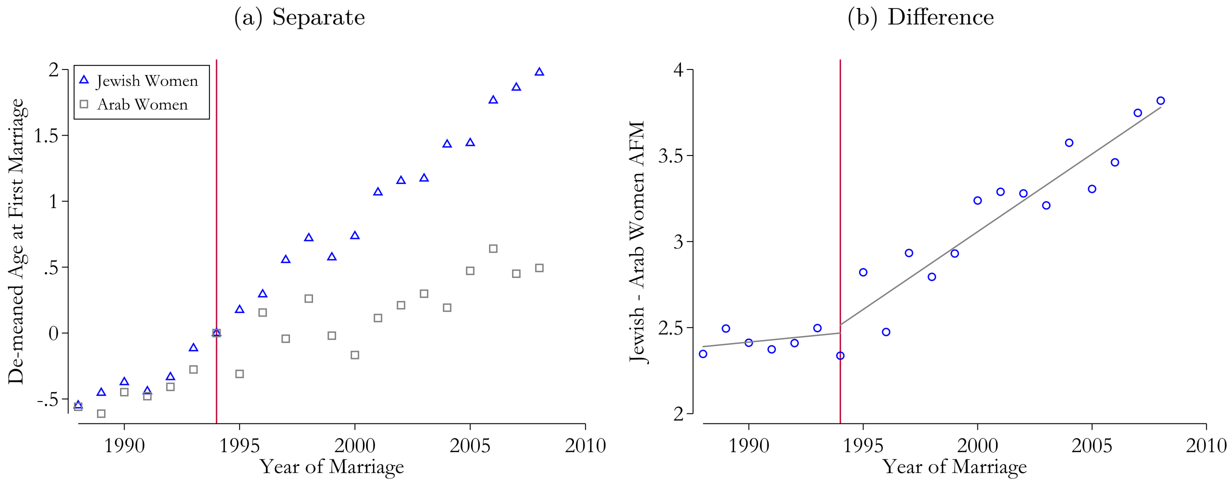
Dep. Var.:	Income		Income Wife Controls		Income Pctile	
	(1)	(2)	(3)	(4)	(5)	(6)
older × post	19316.144	20697.644	18407.16	17905.36	0.026	0.026
	(7616.659)**	(6392.176)***	(9397.97)*	(8160.31)**	(0.011)**	(0.007)***
	[7862.496]**	[8196.525]**	[8359.51]**	[8408.73]**	[0.015]*	[0.015]*
married older	-19882.047	-20813.945	-24604.60	-26872.22	-0.051	-0.050
	(6133.012)***	(5872.047)***	(11038.23)**	(10227.43)**	(0.006)***	(0.005)***
	[13070.829]	[12923.828]	[14415.39]*	[14811.97]*	[0.016]***	[0.015]***
Constant	-393935.497	-589998.553	-167890.13	-520406.54	0.619	0.622
	(939517.549)	(930193.480)	(721687.21)	(756961.82)	(0.011)***	(0.010)***
	[835939.612]	[789565.836]	[744709.09]	[749225.14]	[0.012]***	[0.018]***
Age Poly.	YES	YES	YES	YES		
YOM FEs		YES		YES		YES
Observations	18543	18543	16693	16693	18543	18543
R-Squared	0.0518	0.0530	0.104	0.105	0.0496	0.0510

Dep. Var.:	College Education		Full-time Employment		Prestigious Occupation	
	(1)	(2)	(3)	(4)	(5)	(6)
older × post	0.039	0.039	0.045	0.043	0.053	0.053
	(0.021)*	(0.019)**	(0.018)**	(0.012)***	(0.018)***	(0.015)***
	[0.023]*	[0.022]*	[0.014]***	[0.014]***	[0.017]***	[0.017]***
married older	-0.051	-0.051	-0.052	-0.050	-0.042	-0.042
	(0.015)***	(0.015)***	(0.017)***	(0.010)***	(0.014)***	(0.012)***
	[0.027]*	[0.027]*	[0.013]***	[0.013]***	[0.027]	[0.027]
Constant	0.555	0.549	0.946	0.946	0.473	0.484
	(0.017)***	(0.014)***	(0.008)***	(0.006)***	(0.014)***	(0.013)***
	[0.024]***	[0.039]***	[0.008]***	[0.016]***	[0.022]***	[0.026]***
YOM FEs		YES		YES		YES
Observations	19511	19511	16730	16730	18178	18178
R-Squared	0.0782	0.0791	0.114	0.116	0.0389	0.0400

Notes: Ordinary least-squares difference-in-differences regression using micro data, including controls for religiosity and parents' origin. Robust standard errors clustered at the age group × year level in parentheses; robust standard errors clustered at the age group × geography level in square brackets. Top panel: All columns control for age through a flexible polynomial. Columns 3 and 4 add controls for wife's age, income, and education. Columns 5–6 use spouse's income-for-age percentile as the outcome. Bottom panel: Columns 1–2 use spouse's college education as the outcome variable. Columns 3–4 use spouse's full-time employment, defined on entire male population. Columns 5–6 use spouse's participation in prestigious occupations such as management, law, medicine, engineering, and academia. All specifications include coefficients for post and time. Data from the 2008 Israeli population census, restricted to Israeli-born Jews.

*** p<0.01, ** p<0.05, * p<0.1

Figure 9: Jewish vs. Arab Women's Age at First Marriage



Notes: Figure (a) shows average age at first marriage for women and men, by year of marriage, de-meaned so that the relative changes can be seen more clearly. Figure (b) presents the difference in average age at first marriage between women and men, as well as fitted lines for the pre (1979-1993) and post (1994-2008) periods. Data from the 2008 Israeli population census, restricted to Israeli-born Jews.

Note that in both in the figure and regression results, we do not observe a discontinuity precisely in 1994 as we do for men. This could be attributed to the fact that the Arab women sample is much smaller and their trend over time is not as smooth as for the Jewish population (both men and women), so the series is more noisy, and there may be a random positive error term for Arab women precisely in 1994. A second option, is that when we compare to men, the immediate increase in the difference between the groups is created by the dual effect of young women's marriage delays impacting men's age at first marriage. This duality, of course, is not present when we compare to Arab women instead. As explained above, if Jewish women close to the average age at marriage delayed marriage, and their partners are older than the average for men, we will observe both an increase in women's average and a decrease in men's. This in turn, would increase the immediate impact of the policy, beyond the net change for Jewish women, but this effect would quickly dissipate as the men who dropped out of the marriage market in 1994 reenter in subsequent years.

Overall, these findings corroborate our main results, in that they show a substantial change for Jewish women compared to Arab women. The magnitude of the change in the longer run is larger than the one observed with the male control group, indicating that the echo effect on men may have caused an underestimation of the true effect, as we predicted. This conclusion is also in line with our analysis of how the marriage market for the Jewish population in Israel was reshaped.

Table 5: Age at First Marriage (Arab Control)

	Dependent variable: Age First Marriage					
	DiD		Slope-Change DiD		GLS Slope-Change DiD	
	(1)	(2)	(3)	(4)	(5)	(6)
jewish × post	0.772	0.767	0.076	0.083	0.054	0.096
	(0.127)***	(0.099)***	(0.140)	(0.091)	(0.188)	(0.160)
	[0.179]***	[0.178]***	[0.184]	[0.182]		
jewish × post × time			0.117	0.116	0.113	0.118
			(0.024)***	(0.018)***	(0.046)**	(0.038)***
			[0.047]**	[0.047]**		
Constant	21.815	21.167	21.648	21.666	21.635	21.151
	(0.059)***	(0.242)***	(0.025)***	(0.039)***	(0.191)***	(0.080)***
	[0.277]***	[0.327]***	[0.313]***	[0.315]***		
YOM FEs		YES		YES		YES
Observation	95540	95540	95540	95540	42	42
R-Squared	0.109	0.110	0.110	0.111		

Notes: Columns 1–4: Ordinary least-squares difference-in-differences regression using micro data (no controls included since religiosity and parents' origin controls used only apply to Jewish population). Robust standard errors clustered at the group × year level in parentheses; robust standard errors clustered at the group × geography level in square brackets. Columns 5–6: Generalized least squares regression with data collapsed to the group-year level. Robust standard errors that allow for cross-sectional correlation and for panel-specific serial correlation (i.e. estimate a unique autocorrelation parameter for each group), in parentheses. All specifications include coefficients for group, post, time, post × time, and group × time. Data from the 2008 Israeli population census, restricted to Israeli-born.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

3.4 Placebo and Robustness Checks

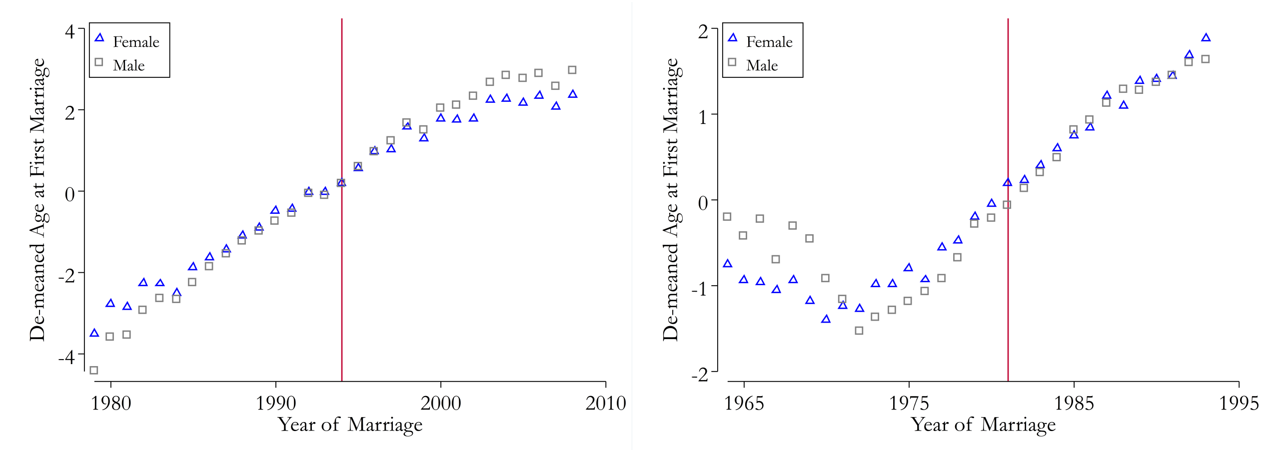
Placebo tests In this section, we perform placebo tests to demonstrate that our effects are substantial relative to shocks in other domains. First, we look at average age at first marriage in the US, and observe men's age versus women's age at marriage has changed smoothly over time. Figure 10, panel (a), shows the average age at marriage of men and women over time in the US American Community Survey. Unlike in the Israeli data, the US data shows a consistent trend—women's age at marriage has been falling relative to men. This trend has been constant, though, exhibiting no apparent breaks, and certainly not one around the 1994 break in the Israeli data.

Second, we look at Israel in the past using the 1995 census, to show that there has not been a similar disruption of women's versus men's age at marriage in historical data. Figure 10, panel (b), shows that men and women's age at marriage has moved roughly in parallel since 1973. Prior to that, there were some differences, but this group is also heavily selected, as few Israelis marrying at that time had parents who were born in Israel (our sample restriction). Fourteen years prior to this earlier census, in 1981, there is no evidence of a trend break. This demonstrates that our effect is not the result of a data artifact due to retrospective analysis.

Permutation approach to significance levels We can also confirm that our effects are large relative to variation within the same data being used for the analysis. To do this, we use two

Figure 10: Placebo Tests

(a) United States (b) 1995 Census



Notes: Panel a: Placebo test for global 1994 shift, using the 2010 American Community Survey. Panel b: Placebo test for spurious results due to retrospective analysis, using 1995 census and fictitious 1981 ”policy change.” Figure uses data up to 1993, to avoid the “post treatment” period (which starts at 1994).

types of permutation analyses on the coefficient in the standard DID (since these have only a single coefficient of interest, unlike the regressions that allow for a slope change). The results are presented in Figure A2. We first perform a permutation test that respects the potential serial correlation in the data, by implementing a standard DID specification (like in column (1) in Table 2) for only ten years of data, five years pre and five years post, with 1994 as the treatment year. We then compare this coefficient to the coefficient obtained from taking each possible sequential interval of ten years within our study period, and each corresponding false “treatment” year in the middle of the interval.[44] In addition, we perform a more standard permutation test, where we run the baseline regression with a “post treatment” period that is randomly drawn from the entire study period (for an example of this approach, see Agarwal et al. (2014)). This approach does not respect the underlying serial correlation in the data, since the years are drawn randomly, but does account for intra-group correlation or other non-standard error structures. We perform 1,000 such random draws, and compare our true treatment coefficient to the resulting normal distributions. Both tests show that the true effect is far to the right of the counterfactual tests.

Israeli women in the US As a final robustness check, we try to find a way to compare treated women to other women who are also Jewish-Israeli. To do this, we examine women of Israeli origin living in the US. Although we see no aggregate break in US women’s age at marriage around 1994, one would expect women who were married in Israel to demonstrate the same shift in age at marriage as our main sample. For these women, we have a natural control group—those women

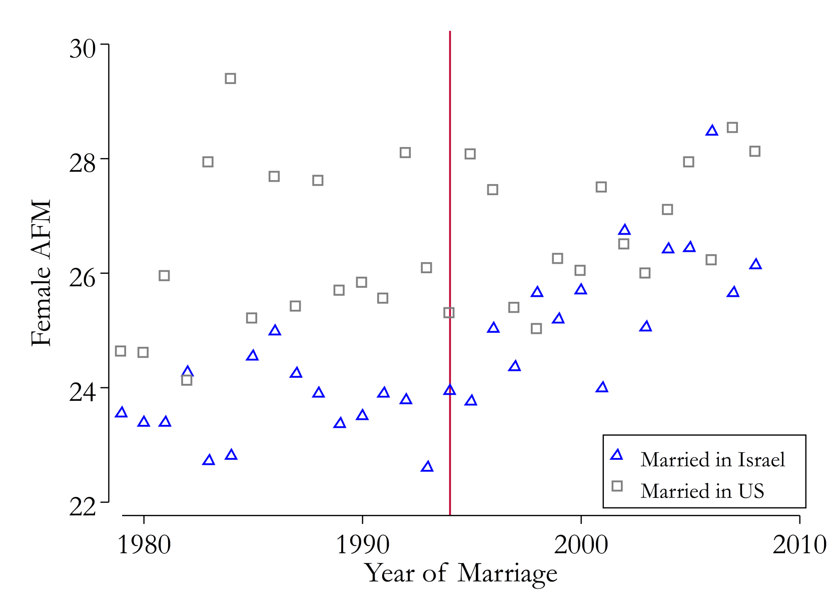
[44]This test does not yield a normal distribution of coefficients, as there are a limited number of ten-year intervals in the study period.

who moved to the US before marriage, and thus were not exposed to the IVF policy change when making their decision about when to marry. If the effects we see in Israel are driven by some broader demographic shift among Israelis, rather than IVF availability, one might expect Israeli migrants to be similarly affected.

Using the 2006-2016 American Community Survey (multiple years are required, due to the restrictiveness of the sample), we examine married women who were born in Israel but now reside in the US. We divide the sample based on whether the women were married in Israel or were married once they came to the US. This contrast is shown in Figure 11. The figure shows a break in the age at marriage for the women married in Israel that mirrors the trend in our main sample. At the same time, the age at marriage for women who married in the US stays approximately constant. This pattern is somewhat remarkable: it is possible to detect the impacts of the Israeli policy change on American women, differentiated based on their place of marriage, and thus which policies they were impacted by at the time of marriage.

Together, these placebo and robustness checks provide further evidence that we have shown a real and statistically large impact of the 1994 policy change on Jewish-Israeli women.

Figure 11: Comparison of Israelis in the US: Married in Israel vs. Married in US



Notes: Data from the 2006-2016 American Community Survey, restricted to Israeli-born women who have been married.

3.5 Heterogeneous Effects

As discussed above, the availability of later life fertility enhancing technology is expected to improve fertility and marriage market results for unmarried "older" women. This is expected to encourage women to postpone marriage and childbearing, allowing them to potentially invest in more demanding careers with longer investment periods or just search longer and increase the quality of their matches on the marriage market. Therefore, we may observe different impacts for women who come from different backgrounds and religious traditions. The Jewish population in Israel is extremely diverse in terms of cultural backgrounds and levels of religiosity, which are on average correlated with gender norms, desired fertility rates, higher education rates, parental income and women's labor force participation, all of which could have an impact on the shift in marriage timing due to the policy change. This offers a unique opportunity to explore the impact of extended reproductive time horizons on women from different cultural traditions, under the same regime and institutional setting.

In table 6 we run the main specification (column (4) in table 2) for different groups of Jewish Israelis. In the top panel of table 6, columns (1) and (2) compare the impact for Ashkenazi and Sephardic Jews.[45] The point estimates for the Ashkenazi group are larger, both in level and in trend. On average, this group was characterized at the time by higher age at marriage and education rates. In addition, Ashkenazi Jewish women tended to have lower fertility rates. This may imply that a larger impact appears where traditional social norms are less restrictive for women's choices.

When we divide our sample by level of religiosity, in columns (3) and (4), we observe again that less religious, and hence less traditional, women have a stronger response to the increased availability of IVF. Note that our identification of religiosity is imprecise, as it is based on their neighborhood of residence, and thus measurement may be imperfect.[46] However, the point estimates say that even in ultra-Orthodox neighborhoods there was a gradual increase in average age at marriage. This could be attributed to the fact that fecundity is a more fundamental trait for women on the Ultra-Orthodox marriage market, hence older women are initially penalized much more (in addition to being considered "old" at a younger age). We do not expect women in this group to delay marriage immediately in response to the policy change, which is consistent with the insignificant effect for the intercept change, or to delay marriage into their thirties, but it is still possible that it improved the fortunes for women who were before considered too old to be fertile enough for a large family, and this created an upward trend for women's marriage age over time.

[45] We define a person as "Ashkenazi" if at least one of their parents is from European or American origin and none of their parents are from African or Asian origin, and the opposite for "Sephardic". Jews from Asian origin would mostly come from the middle-eastern countries and the Asian part of the former Soviet Union. Jews from African origin mainly come from Northern Africa. We exclude from this analysis people whose parents are both Israeli-born since they could potentially belong to either group of origin, and we do not have data on grandparents' origin.

[46] We use the Israeli Central Bureau of Statistics classification of neighborhoods as (mainly) ultra-Orthodox which is based on patterns of voting in the national elections (for ultra-Orthodox parties).

The bottom part of the table, presents results for 4 levels of socioeconomic status (SES), based on the rank of the neighborhood of residence. This rank goes from 1 to 20 (where 20 is the highest SES), and we create groups of individuals that live in neighborhoods ranked 1-5, 6-10 11-15 and 16-20, which are shown in columns (1)-(4) accordingly. Interestingly, we see the most pronounced effect appears for individuals ranked 11-15, so for relatively high SES but not the highest group. This relates to the other findings we report in the top part of this table, as the lower SES is correlated with the other traits that show a lessened effect. Lower-SES women may be less likely to consider delaying marriage for the sake of making career investments, and may marry at very young ages where fertility is not a relevant constraint. For the top SES group, it could be claimed that their marriage market status would be better to begin with, and hence the fecundity change will be less dominant. Additionally, the initial knowledge of and access to IVF may have been greater for this highest-status group. Last, this analysis also helps refute concerns that the general change in health insurance is driving our results, since then we would expect to see the largest shifts in the bottom SES group.

4 Conclusion

Increased access to IVF offers women the security of a second-line option in case they do not naturally achieve their desired level of fertility. This extension of the reproductive time horizon for women should both increase the value of "older" brides on the marriage market as well as make women more willing to delay marriage, whether to invest in their careers or search longer for a mate.

We show a stark increase in age at first marriage for women relative to men immediately following the policy change, and rule out that it is caused by gradual trends using break-point and event study analyses. Identifying this change is especially interesting given that men may also be impacted in equilibrium by women's decisions. The fact that we are able to identify a shift in women's outcomes relative to men's resulting from IVF lends credence to the idea that women's time-limited fertility represents a differential constraint on women's marriage timing compared to men's.

We analyze the composition of marriages by age to confirm that the differential increase in marriage age for women is driven both by young women delaying marriage and by older women actually getting married. Inevitably men's marriage patterns are impacted by this change, leading to a reshuffling of the marriage market, demonstrated by a decrease in the spousal age gap. We also show direct evidence of improved marriage market fortunes for older women, with the penalty to marrying over age 30 in terms of spousal income completely dissipating post policy-change.

If women who marry at an older age use the time to pursue additional education and potentially other career opportunities, IVF availability can have significant consequences relating to women's

Table 6: Heterogeneous Treatment Effects

By:	Parent's Origin		Religiosity	
	Ashkenazi	Sephardic	Ultraorthodox	Non-Ultraorthodox
	(1)	(2)	(3)	(4)
fem × post	0.454	0.255	0.110	0.257
	(0.116)***	(0.070)***	(0.087)	(0.053)***
fem × post × time	0.069	0.024	0.038	0.043
	(0.012)***	(0.008)***	(0.009)***	(0.006)***
Constant	26.734	24.107	22.008	23.408
	(0.156)***	(0.163)***	(0.119)***	(0.083)***
FEs	YES	YES	YES	YES
Observation	45347	77591	19125	148291
R-Squared	0.223	0.255	0.166	0.201

By:	Socio-Economic Status			
	(1)	(2)	(3)	(4)
fem × post	0.055	0.111	0.367	0.155
	(0.104)	(0.075)	(0.114)***	(0.130)
fem × post × time	0.044	0.031	0.074	0.038
	(0.011)***	(0.008)***	(0.013)***	(0.015)**
Constant	26.701	23.587	22.288	23.194
	(0.074)***	(0.083)***	(0.115)***	(0.201)***
FEs	YES	YES	YES	YES
Observation	24581	57315	33256	12287
R-Squared	0.213	0.217	0.231	0.195

Notes: Ordinary least-squares difference-in-differences regression using micro data, including controls for religiosity and parents' origin (where the sample is not split on these characteristics). Robust standard errors clustered at the age group × year level in parentheses; robust standard errors clustered at the age group × geography level in square brackets. Top panel: Columns 1 and 2 present results by parents' origin. Columns 3–4 present results by level of religiosity. Bottom panel presents results by socioeconomic status, ranked between 1 (lowest) and 20 (highest). Column 1 is for SES rank 1-5, (2) for 6-10, (3) for 11-15 and (4) for 16-20. Data from the 2008 Israeli population census, restricted to Israeli-born Jews.

*** p<0.01, ** p<0.05, * p<0.1

labor market outcomes and the gender wage gap. Indeed, in Gershoni and Low (2018) we show that the cohorts of women exposed to the IVF policy change went on to get more education and have higher paying and more prestigious careers.

This is of critical importance because the cost per user of free IVF with Israel's generous coverage is high, 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 "insurance effect" of the policy, 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 ability to choose when to marry or to impact any other life choices, but were rather explicitly pro-natalist, aimed at increasing the birth rate of Israeli citizens by providing aid for infertile couples (or single women).[47] Thus, policymakers should note that the behavioral response to IVF access may cause fertility effects to be attenuated, or even 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 and go on to use the technology, only to be unsuccessful conceiving.

More broadly, the substantial impact on marriage markets reported in this paper, should be considered as the availability of ARTs increases worldwide and as even more innovative fertility extending technologies are introduced. While IVF technology may allow women to extend their chances of conceiving a biological child as long as they still have viable eggs, cryopreservation technology may potentially extend this option indefinitely. Realizing this, private firms have already begun to subsidize egg-freezing for their female employees, with the clear intent to enable them to delay family formation and thus avoid (or postpone) the female-specific trade-off between labor market productivity and home production. While this can potentially benefit women, usage of fertility technologies may also have health and welfare costs. 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, and feel pressured to submit themselves to intrusive medical procedures and late parenthood. Thus, it is unclear if expanded access to IVF is the *best* policy to alleviate the one-sided burden of depreciating reproductive capital. What is clear, however, is that this burden plays a crucial role in women's decisions and outcomes.

By testing what happens when the threat of later life infertility is attenuated, this research suggests time-limited fecundity as a key source of asymmetry between men and women. The constraint women face in needing to plan marriage and family timing around their fertility time

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

horizons could be an important "wedge" between men's and women's abilities to invest in careers, helping to explain the gender wage gap and dearth of women in high-powered careers. Policies that relieve the career-family tradeoff, such as access to assisted reproduction, can blunt this effect, and thus promote equality. However, policymakers may also want to consider other options to relieve the stark career-childbearing tradeoff, such as by promoting family leave, work return, and flexible hours. All of these things may help relieve the differential constraint of women's shorter reproductive time horizon.

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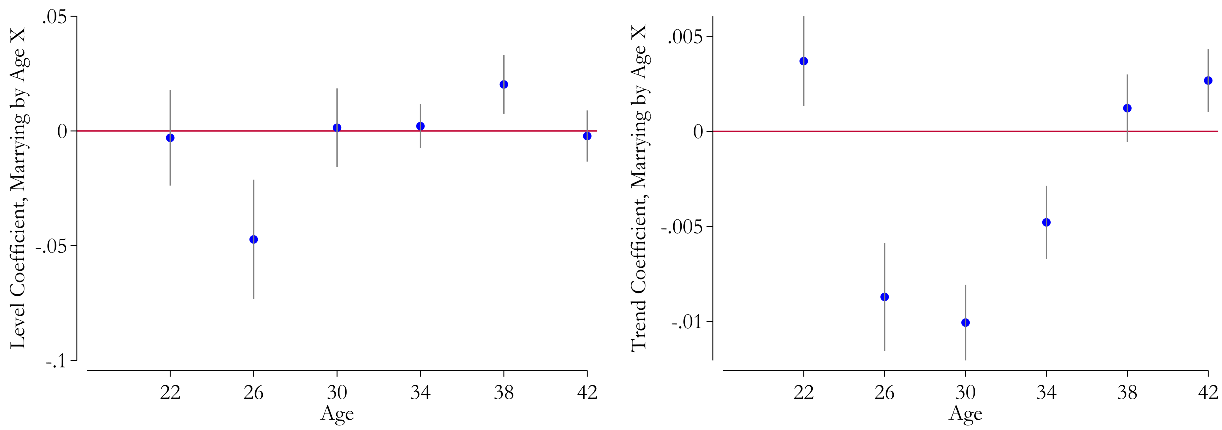
5 Appendix

Figure A1: Regression Coefficients for Effect of IVF Law on Marrying by a Given Age: Men's Ages Shifted

(a) Intercept coefficient

(b) Slope coefficient



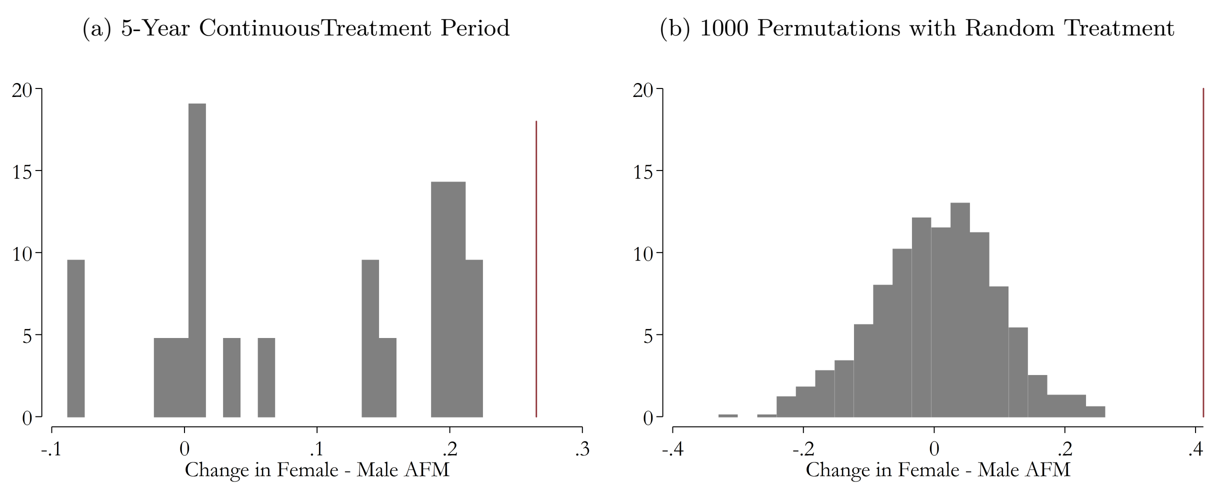
Notes: The figures presents the point estimates and confidence intervals of the coefficients on (a) the interaction term fem×post and (b) the interaction term fem×post×time, for regressions where the outcome is a binary variable indicating whether or not the individual got married at or before a certain age, and the specification is as in column (4) in table 2. In this version of the figure, we compare women's marriage rates by X age to men's marriage rates by X+3, to account for the age gap in average age at marriage between men and women. Data from the 2008 Israeli population census, restricted to Israeli-born Jews.

Table A1: Beliefs of Israeli Students about IVF Success Rates, 2009

Age	Natural fertility success rate %	Success rate with IVF %	Improvement from IVF %
20-35	74.6	90.8	21.7
36-39	58.1	75.9	30.5
40-43	46.9	63.9	36.3
44-47	36.8	52.8	43.2
48-52	28.4	41.5	45.8
53-58	17.6	29.5	67.4

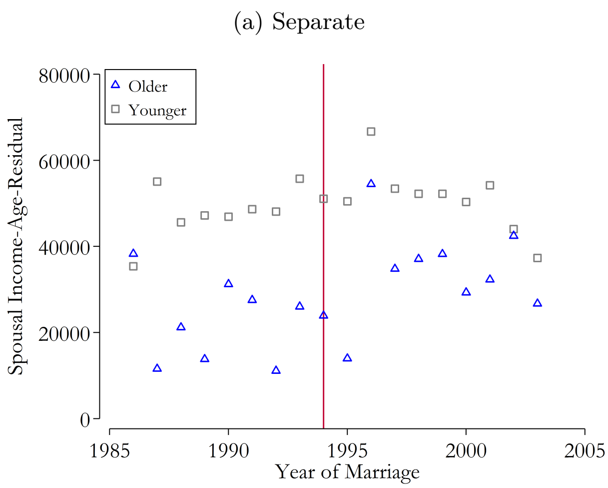
Notes: Imputed estimates from Hashiloni-Dolev, Kaplan and Shkedi-Rafid (2011) survey of Israeli male and female college students. Estimates for natural fertility success rates for given age ranges created by fitting a fifth-order polynomial to survey responses, which were for different age ranges. Total success rates computed by multiplying IVF success rates from the survey by the natural fertility failure rate, then adding to the natural fertility success rate. Percent improvement is the success attributed to IVF divided by baseline success.

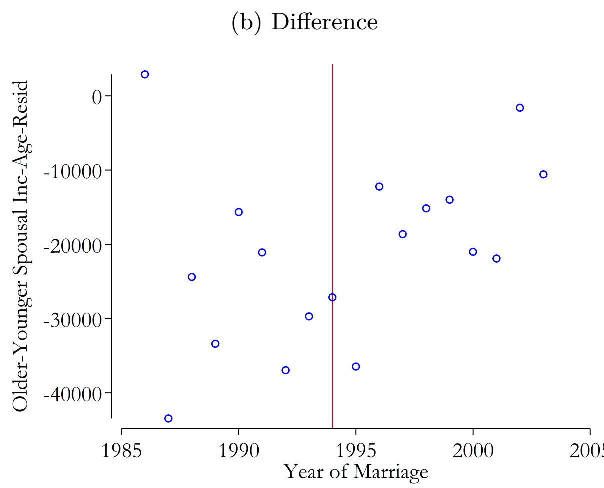
Figure A2: Permutation Analysis of Average Age at First Marriage Effect Size



Notes: The figure on the left is created by running a similar regression as our column 1 specification, except with a ten year data period, with five years control and five years treatment, sequentially, for every possible ten year period in our data range. The red line represents the effect size of the actual treatment year, with this ten-year data period (the ten-year approach allows us to compare our actual treatment to other break points, with the same number of years before and after). The figure at right uses the same number of “treated” years as in the true model, but randomly draws them from the study period (for an example of this approach, see Agarwal et al. (2014)). We perform 1,000 such random draws.

Figure A3: Spousal Income for Older vs. Younger Women, 1986 - 2003





Notes: This figure compares current spousal income for women who married at age 30-34 to women who married at age 25-29. In order to control for age effects, the measure of spousal income used is the residual from a regression of income on a flexible polynomial in age. We use a narrower time range in these graphs to prevent censoring from individuals either still being students, for younger cohorts, or entering retirement, for older cohorts. Figure (a) shows average income-age-residuals separately by age group, by year of marriage. Figure (b) presents the differences in spousal income-age-residual, as well as fitted lines for the pre (1986-1993) and post (1994-2003) periods. Data from the 2008 Israeli population census, restricted to Israeli-born Jews.