THE POWER OF TIME: THE IMPACT OF FREE IVF ON WOMEN'S HUMAN CAPITAL INVESTMENTS

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Abstract

Women's time-limited fertility window, compared to men's longer period of fecundity, could be a key constraint in shaping the gender gap in career choices and hence outcomes. Israel's 1994 policy change that made *in-vitro* fertilization free provides a natural experiment for examining how fertility time horizons impact women's investment choices. We find that following the policy change women complete more college and graduate education. We then present evidence suggesting that these larger investments contributed to better labor market outcomes, reducing the gender gap in career achievement. This further implies that persistent labor market inequality may be partly rooted in biological asymmetries.

JEL Codes: J13, J16, J24.

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1 Introduction

The persistence of the gender wage gap has been the subject of much exploration in the economics literature. It is now clear that a significant part of this gap is a direct consequence of women's choices, including over college major, career track, working hours, and workplace amenities. However, it is less well established whether these divergent choices result from inherent differences in women's preferences and abilities,[1] or if they are the product of differing constraints. Literature showing that the introduction of modern contraception increased women's education has demonstrated that control over fertility when young is important for women's ability to invest in their careers (Goldin and Katz, 2002). In this paper, we test the hypothesis that women's loss of fecundity when older is a similarly key constraint shaping their career investment choices, using a policy that extended the older age fertility time horizon.

Women's fertility begins to sharply decline in their mid-thirties, whereas men can successfully reproduce for many years after.[2] Anticipating this decline, women may cut short career investments in order to search for a partner, marry, and have their desired number of children before their fertility window closes. *In-vitro* fertilization (IVF) is a technology that addresses women's age-related infertility and can potentially extend female fecundity beyond menopause. Thus, Israel's unprecedented decision to make IVF free for all citizens provides a natural experiment for examining how anticipating a longer fertility time horizon may impact early life investment decisions.

In 1994, Israel added virtually unlimited IVF treatments to the "basket" of free health services provided by its public insurance system. This policy was (and still is) unique as it provided free access to a relatively new, unknown and very expensive technology. The fact that it was applied equally to all residents regardless of income, marital status, or age, facilitated a sharp increase in IVF usage. In turn, this was reflected in a discontinuous and considerable increase in the number of live IVF-based deliveries (which more than tripled within three years from the policy change). Combined with extensive media coverage, this change motivated a quick, generalized, and substantial shift in beliefs surrounding later-life reproductive possibilities for women, to the extent that it was referred to by scholars as the Israeli "fertility revolution" (Hashiloni-Dolev, 2013). Consequently, Israel became a world leader in IVF availability, as shown, for example, in the 2004 report issued by the International Committee for Monitoring Assisted Reproductive Technologies (ART), which attributes this to the "policy framework and the availability of public or third-party

[1] E.g., taste for competitiveness (Niederle and Vesterlund, 2007) or even distribution of intelligence (Wai et al., 2010).

[2] Women lose 97% of eggs by 40 (Kelsey and Wallace, 2010), while remaining egg quality declines (Toner, 2003). Pregnancies are rarer (Menken et al., 1986), more likely to end in miscarriage (Andersen et al., 2000), and more likely to result in fetal abnormalities (Hook et al., 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%.

payment for ART cycles" (Sullivan et al., 2013).[3]

This unique policy setting provides an opportunity to test how loosening the constraint of the fertility-age profile affects women's choices. In Gershoni and Low (2020), we present evidence that this policy change dramatically increased women's average age at marriage, both by increasing marriage probabilities for older women and by encouraging young women to delay marriage. This finding, that *young* women's expectations and decisions have been altered, even before they were in the position to actually use the technology themselves, indicates that the Israeli setting is well suited to test an emerging theoretical literature that points to women's fertility time horizons as an important determinant of early human capital investments (Low, 2019; Zhang, 2018). The advantage of our setting over previous studies that rely on state-year variation in the timing of insurance mandates within the USA, is that the policy change was comprehensive and influential enough to substantially impact young women's perceptions and thus investment decisions.[4] Moreover, because migration across states is common, existing literature cannot rule out that IVF insurance coverage is merely an attracter of career-focused women, rather than a factor that alters women's investment choices in the first place.

Building on the discontinuous nature of the Israeli policy change, we exploit a difference-in-differences (DID) design to identify the impact of changing fecundity expectations on cohorts of *native-born* Jewish-Israeli women at an age young enough to alter their long-term career decisions. These women are compared to two separate comparison groups: Jewish-Israeli men, whose own fecundity horizons were naturally unaffected by access to IVF,[5] and Arab-Israeli women, who were much less likely to use IVF at the time of the policy change due to religious restrictions. This approach allows us to separate the impact of IVF from other potential changes and general time trends around the policy event, as well as to avoid confounding factors related to changing population structure or endogenous migration.[6]

The main hypothesis that we test with this strategy is that the *option* to use IVF in the future changes the perceived cost of career investment among young women, leading them to increase long-term investments such as college and graduate education. Moreover, having a longer horizon could push women towards career tracks that require longer on-the-job investment, which are also typically considered more prestigious. These augmented choices would, in turn, lead to increased

[3]This report also shows that Israel, in the first place in 2004, had almost *twice* as many treatment cycles per million population as Denmark, in second, and almost 8 times as in the US.

[4]Most of the research using US insurance mandates focuses on the women who themselves use IVF (Velez et al., 2014; Hamilton and McManus, 2012; Bitler and Schmidt, 2012, 2006; Bundorf et al., 2007; Buckles, 2013; Schmidt, 2007, 2005; Machado and Sanz-de Galdeano, 2015). A smaller body of literature looks at the impact on younger women (Ohinata, 2011; Abramowitz, 2017, 2014; Buckles, 2007; Kroeger and La Mattina, 2017). For a complete discussion, see Section 2.

[5]The policy also impacted treatment of male infertility via access to sperm donation and to ICSI (a procedure that allows to use a single sperm-cell to fertilize an egg), this is "primary" rather than "secondary," meaning age-related, infertility, and thus does not affect time horizons. Nevertheless, men did update their beliefs about potential female partner's fecundity as we claim in Gershoni and Low (2020).

[6]Especially due to the mass-immigration from the former Soviet-Union to Israel starting in the late 1980s.

income, and thus to a reduction in gender inequality. Data from the 2008 Israeli population census merged with administrative tax data on income allows us to address a wide range of education and labor market outcomes, in particular, college and graduate education, labor force participation, full-time work, participation in prestigious occupations, and income. We present detailed figures that compare changes over time between the treatment and two comparison groups, and base our causal inference on the apparent and striking differential change in the treatment group at the effective time of the policy change. Our DID regression analysis provides a measure of the magnitude of the impact.

Our first comparison, to Jewish men, enables us to control for other events that may have affected both men and women's educational attainment and labor market prospects over the same time period, as Jewish men have the same cultural background, face similar military requirements, and a similar labor market as Jewish women. Nevertheless, factors that impact only women could potentially confound this analysis, which is addressed by our second comparison to Arab-Israeli women. As previously described, Arab-Israeli women were negligibly impacted by the new funding policy because their access to fertility technology was limited by religious restrictions. Moreover, Arab women in Israel have a relatively early start in higher education and family formation because they are not obligated to mandatory military service as Jewish-Israeli women are. In addition, they usually have more traditional and religious backgrounds which dictate marriage and childbearing at earlier ages. Thus, we expect the limited fertility horizon for women to be less restrictive in this group. Complementary to the first comparison group, this second control group allows us to address confounding factors that impact women in particular, such as worldwide trends towards higher rates of female education and other policies in Israel that may differentially impact women. One such example is the expansion of higher education in Israel, which impacted Arab-Israeli women just as much as their Jewish-Israeli counterparts, if not more.

We find that women's college and graduate education completion increased substantially in the cohorts entering these educational levels at the time of the change. These results are remarkably consistent across the two comparison groups, and also when we use a continuous measure of exposure to treatment to account for the variation in college entrance age. Importantly, the rate of increase in women's graduate education is higher than the equivalent increase in college completion, supporting our hypothesis that the fertility constraint (which is specifically expected to limit long investments) played an active role in these changes. Moreover, we show these investments were advantageous in terms of career outcomes. We find that the gender gap in full-time employment, participation in prestigious occupations, and income all decreased for affected cohorts in the Jewish, but not Arab, population.

To further support our suggested mechanism of impact and to rule out potential alternative explanations, we present multiple robustness and placebo checks. First, we directly control for the educational expansion in Israel with data on the precise years of new academic colleges' approval, allowing this expansion to have a gender and population-group differential impacts. In addition, we show that there was no similar differential increase in high-school completion, a decision which is less likely to be affected by fertility planning. We then use international census data to show that the findings cannot be attributed to some general global change for women, because the change in education rates among women in other, similar countries, including the United States, has been smooth over time compared to men, unlike the trend break demonstrated in Israel. Finally, we use the 1995 Israeli Census to perform a placebo test, indicating that the structural breaks in the data are historically unprecedented, and use a permutation analysis to show that the empirical effects are uniquely large.

Together, our findings indicate that mitigating women's concerns surrounding age-related infertility alters women's education decisions, which in turn, leads to better career outcomes. Importantly, our theory of impact does not rely on actual usage of IVF by the affected women. Rather, we hypothesize that the future availability of IVF provides a form of insurance against age-related infertility. 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 et al., 2012). The evidence we present bolsters the theory that fertility time horizons are an important factor in women's family and career decisions. More broadly, this research highlights the role of biological differences in the divergence of economic outcomes, and the importance of policies in blunting this effect.[7]

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

2 Related Literature

Numerous papers document and aim to explain the evolution of the gender pay gap and its persistence over the last two decades in the US and in other developed economies.[8] A significant part in the lack of convergence between genders is attributed to human capital factors, including choices of college majors (Zafar, 2013; Bronson, 2015), entry into different professions and industries (Bertrand et al., 2010; Goldin, 2014; Buser et al., 2014), as well as cumulative labor force experience and human-capital accumulation, driven by part-time work and time spent out of the labor force (O'Neill and Polachek, 1993; Erosa et al., 2016). In addition, although women's college attainment rates exceed those of men (Goldin et al., 2006), evidence on differential returns on college education due to the absence of high-paying non-college job opportunities for women

[7] For evidence on the prominent role that policy measures have in cross-country differences in the gender pay-gap see Bick and Fuchs-Schündeln (2018); Del Boca and Sauer (2009).

[8] See Blau and Kahn (2017) for evidence on the US and an extensive discussion and review on the possible sources of the phenomenon.

could imply that women's true "steady state" level of college attendance would be even higher if post-college barriers to career are removed (Chuan, 2019).

Previous literature establishes that control over fertility among young women, namely the ability to avoid unwanted pregnancies, plays an important role in women's career choices. Goldin and Katz (2002) (and later Bailey (2006); Bailey et al. (2012); Myers (2017)) use the expansion of access to oral contraception (or abortions) to demonstrate that the ability to delay motherhood enabled women to make greater educational and labor market investments. Numerous additional studies support these findings, and use various methods to establish and quantify the tradeoff between family and career for women (Loughran and Zissimopoulos, 2009; Buckles, 2008; Blackburn et al., 1993; Taniguchi, 1999; Gustafsson, 2003; Miller, 2011; Avellar and Smock, 2003; Wilde et al., 2010). Moreover, recent work directly connects child rearing with substantial wage declines for women (Adda et al., 2017; Kleven et al., 2019; Angelov et al., 2016), especially in high-earning professional careers with a non-linear wage structure (Bütikofer et al., 2018). Lundborg et al. (2017) also show a substantial "motherhood penalty," using quasi-random successful IVF treatments.

On the other hand, women who choose to delay motherhood in favor of career investments risk not achieving their desired family size, in addition to expected difficulties on the marriage market (Low, 2019; Siow, 1998; Dessy and Djebbari, 2010; Bronson and Mazzocco, 2015). In Gershoni and Low (2020), we show that increased access to IVF enables women to postpone marriage and childbearing. These findings are in line with studies that exploit variation in mandated ART (including IVF) insurance coverage across US states and over time to determine how more coverage affects marriage and birth timing (Ohinata, 2011; Abramowitz, 2017, 2014).

Existing evidence of the impact of IVF provision on human capital investments and career outcomes is suggestive, but not conclusive. Unpublished work by Buckles (2007) presents some evidence evidence that infertility insurance mandates led to increased labor force participation for women under the age of 35 (but a decrease for older women). Kroeger and La Mattina (2017) find an increase in women's participation in "professional" occupations (those requiring a JD, MD, DDS, etc.) in states that passed IVF mandates. Although this is highly suggestive that IVF access is valuable for pursuing certain types of careers, their framework cannot rule out migration into states with more favorable policies, rather than young women actually altering their investments.[9]. In addition, it is impossible to establish that the timing of mandates was exogenous and that pre-trends were parallel.

More generally, we might expect these localized policy changes to be less effective at creating generalized shifts in investment behavior, given that awareness may not be widespread among young women who may not even be managing their own insurance yet. Even if they are aware, since inter-state migration is common, women might expect they will be able to move into a state

[9]Kroeger and La Mattina (2017) attempt to address this with a specification that uses women older than 35 as an additional control group, but this group may also be treated by their increased fertility, as shown in Buckles (2007)

with better coverage if it becomes relevant to them. There is also some evidence that mandates may be a double-edged sword, as increasing insurance costs may increase statistical discrimination against women (Lahey, 2012).[10]

In light of this, the 1994 Israeli policy change provides a unique opportunity to study a widespread, discrete policy that would have immediately altered women's planning horizons. Moreover, because the coverage was publicly funded, there should be no general equilibrium effects on the costs of employing women. By using a cohort-level approach to treatment, and thus comparing women who were young enough to alter their investment decisions to those who already completed college, we are able to pinpoint the effect on young women's career investments, rather than capturing other shifts. Moreover, the nation-wide impact of the policy and restricting to an Israeli-born population rules out endogenous migration as a driver. Thus, we are able to provide the first evidence that the long-term expectation of IVF access can dramatically alter young women's human capital investments and their later career outcomes, including the gender wage gap.

3 Setting and Empirical Approach

3.1 IVF in Israel

In 1982, the first Israeli and fifth worldwide "test tube baby" was born in Israel, placing Israel as one of the world's leaders in IVF technology. During the 1980s, research on this innovative technology highlighted the potential of IVF in addressing age-related infertility among women, especially with the development of egg donation and egg and embryo freezing technologies. Although other ARTs existed and effectively assisted infertile couples prior to IVF, none of the alternative technologies offered such a revolutionary change for older women's fecundity prospects. Nevertheless, in the early 1990s IVF technology was still not prevalent worldwide and involved high levels of uncertainty with regards to outcomes and impacts on mothers' and children's health.

Nevertheless, in the early 1990s, IVF technology was still not prevalent worldwide and involved high levels of uncertainty with regards to outcomes and impacts on mothers and children's health. In addition, the technology was very expensive both in absolute terms and relative to other ARTs, especially among older women who require a relatively high number of treatment cycles for conceiving and delivering a live baby. Although IVF treatments were covered, at least to some extent, by Israel's four main health plans,[11] in practice, couples often had to pay substantial fees out-of-pocket to access services.[12] Moreover, the terms for eligibility and extent of coverage varied between health

[10]With any employer-provided insurance benefit, wages may fall to reflect the presence of the benefit to employees, and cost to employers. Lahey (2012) presents evidence on infertility mandates suggesting that in addition to these falling wages, because wage changes will not fully offset the increased premium costs for women in affected age groups, employment opportunities (and thus labor force participation) for this group decreases.

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

[12]See, for example "We will have to forgo having a child since we cannot afford fertility treatments," *Yedioth*

plans and, in many cases, were vague or a priori undetermined.[13] The variation in terms could not be exploited by patients because membership was mostly based on political affiliation, labor unions and parents' membership, and switching between health plans was difficult (especially for individuals with pre-existing medical conditions or of older-age).

In 1994, the Israeli parliament (the Knesset) decided that IVF treatments, including all associated tests and medications, will be fully covered by all health plans, for up to two children per couple. This type of coverage set a worldwide precedent for two reasons; first, it defined eligibility by treatment outcomes (as opposed to the extent of treatment, e.g. limited number of treatment cycles) and second, eligibility is determined per couple and not per individual, namely with each new spouse, individual entitlement is reset. Additionally, the law grants single childless women the same rights. Importantly, no limit was placed on IVF patient's age. This new legislation is in stark contrast to most Israeli HMO policies prior to 1994 and to other public funding policies around the world.[14]

This policy was enacted as part of a major public healthcare reform in Israel, the National Health Insurance Law (NHI) of 1994, which received considerable attention and provoked public debates on many issues, including IVF use. Overall, this reform standardized and equalized the extent of health coverage for all Israelis and regulated the structure and budget of the public health system. More specifically, it enforced a health tax that replaced membership fees that were paid directly to the HMOs and consequently changed the way money flowed between the government, HMOs and healthcare service providers. However, it did not change the ratio of private to public health expenditures in Israel.

The new policy and the attention it received naturally increased the general awareness to this innovative technology and to its potential in boosting fecundity among older women. Advocates of the health reform, including the Minister of Health, publicly touted the benefits of IVF funding in interviews in support of the policy change.[15] Importantly, the passing of this law was driven by a pro-natalist agenda, rooted in the Jewish tradition of familism, rather than in pro-women or "feminist" sentiments which may have carried other effects.[16] This is also mirrored in the Israeli Supreme Court's ruling that provision of IVF by the state is justified by the reason that having a

Ahronoth, June 14, 1992

[13] The most generous coverage was offered by the largest health plan ("Clalit"), which placed almost no limitations on usage. However, this was not announced as an official policy but rather a consequence of difficulty in tracking treatments (Birenbaum-Carmeli, 2004). Other health plans offered a limited number of treatment cycles and placed substantial age restrictions on its use, in addition to long required qualification periods. For example, in "Leumit" health plan the number of treatment cycles was limited to six and the maximal age was 40.

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

[15] 12 See for example an interview with the minister of health Dr. Ephraim Sneh, *Yedioth Ahronoth*, December 15, 1994.

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

biological child is a fundamental human right.[17]

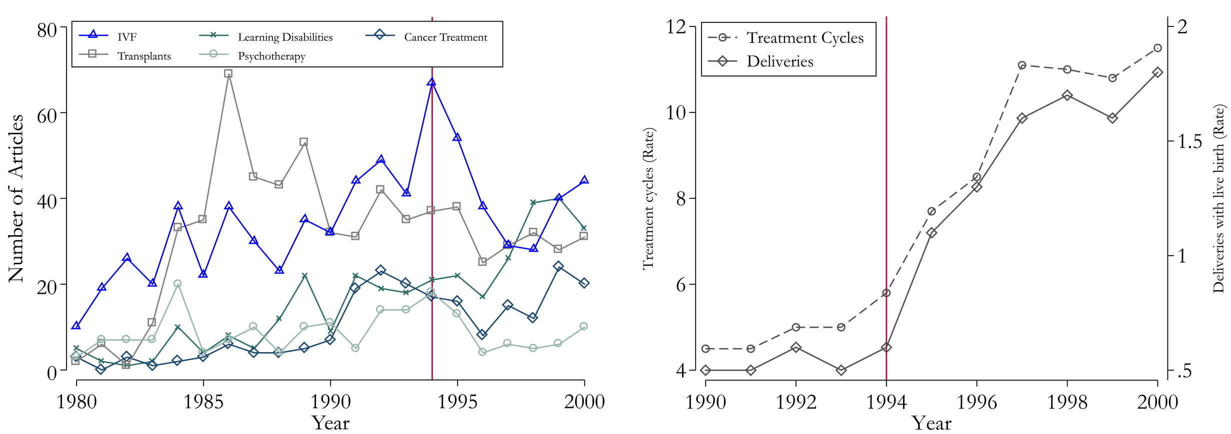
At the onset of this change, the Israeli media was flooded with IVF success stories, such as extreme cases of women bearing children at advanced ages, further raising awareness to the new technology.[18] Figure 1(a) shows a spike in newspaper coverage of IVF and its surrounding funding issues in 1994. This spike is of comparable magnitude to the one observed in the organ transplants news coverage on organ transplants when this technology was first introduced in Israel in 1986. Figure 1(a) also shows that other medical issues which similarly received additional funding in 1994, did not raise media attention as much as IVF, thus supporting the hypothesis that awareness to IVF technology and its potential, substantially and discontinuously increased in 1994.

Consequently, an immediate spike in usage of IVF technology was registered, as well as a doubling in the rate of live IVF deliveries within just one year, as shown in Figure 1(b). The figure plots the number of IVF treatment cycles and the number of live births following IVF treatments relative to the population of fertile women (aged 15-49) before and after the policy change. In 2012, the Israeli parliament issued a report that described this dramatic change and attributed it to the regularization and expansion of IVF funding under the NHI law.

Figure 1: IVF Usage and Awareness

(a) IVF News Coverage

(b) IVF Usage



Notes: Figure (a) presents the number of articles found in a search of IVF related terms (e.g., IVF, infertility, ART, sperm and egg donation and surrogacy) and other health conditions and treatments that appeared each year between 1980-2000 in the headline section of Israel's leading daily newspaper at the time (*Yedioth Ahronoth*). Figure (b) (b) shows the rate of IVF treatment cycles and deliveries per 1,000 women aged 15-49, based on administrative data from the Israeli Ministry of Health, covering all women in Israel. Note that a variation of these figures was previously published in Gershoni and Low (2020).

In Gershoni and Low (2020) we show an increase of approximately 30% in the rate of women

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

[18] For example, "World record: woman aged 60 gave birth to girl," *Yedioth Ahronoth*, February 22, 1994. In addition, local success stories were celebrated as "national accomplishments and symbols of local scientific excellence" (Birenbaum-Carmeli, 2004).

aged 41-47 who gave birth immediately after the policy change.[19] The fact that this impact can be easily observed by the public, has the potential to shift young women's beliefs regarding future fecundity, even without direct knowledge about IVF funding policy.

In Appendix table A1 presents evidence on the beliefs of young Israelis about natural and IVF assisted fertility at different ages. These estimates are based on a survey of undergraduate students as reported by Hashiloni-Dolev et al. (2011) and demonstrate that individuals, at the age in which one typically make decisions on education and career tracks, are well aware of the decline in female fecundity with age.[20] Moreover, according to the perceptions of these young Israelis, IVF technology enabled a substantial delay in commencing childbearing without a decrease in expected fertility, making women in their late thirties as fertile as 20-35 years old women. A comparison of medical data on success rates shows a respondents' tendency towards over-optimism about IVF success rates among older women.[21] This type of perceptions has the potential to significantly alter decisions young women make when weighing whether or not to make time-consuming human capital investments, especially in long-term investments.

3.2 Empirical Design and Data

We aim to study how knowledge of IVF access, and consequently a longer expected fertility time horizon, affected higher education attainment and career outcomes among women. Additional human capital investments might come at the cost of delaying marriage and childbearing, and therefore lower the expected fertility. Thus, knowledge of IVF availability later in life may encourage making further human capital investments before commencing with childbearing. We expect to find these impacts regardless of whether these women actually end up using the technology, as IVF, in a sense, provides insurance against age-related infertility.

In Israel, due to a 2-3 years mandatory military service (for Jewish men and women), even early educational and career investments (such as completing college) may infringe on a woman's planned reproductive years, and potentially limit family size.[22] Therefore, we examine both college and graduate education as outcomes, in addition to full-time work, career field, and earnings.

[19] This increase is measured between 1993 and 1995, since pregnancies that started at 1994 were expected to end during 1994-1995 (therefore 1994 could be partially "treated").

[20] The study was conducted in 2009. There is no similar study prior to 2009, therefore, we are unable to offer direct evidence that beliefs evolved over time.

[21] 18 For example, students believed that IVF would be 32% effective for women 40-43, whereas the actual rate is around 20%, and the gaps are even larger for older ages.

[22] While in the USA or in European countries women around age 24 (women's average age at first marriage in Israel at the time) would mostly be "too old" to make decisions on college attendance, Israel's mandatory military service requirement substantially postpones such decisions. The median age at college entry for Jewish women at the time was 22.5, as reported by the Israeli Central Bureau of Statistics. Mandatory military service following high school takes between two to three years (including pre-service training or waiting periods between high-school completion and enlistment). Typically, an additional year after military service is required for entry exams and preparation of applications, in addition to the fact that military service may start and end "off cycle" with the academic year, further delaying college entry.

As previously mentioned, the 1994 Israeli IVF policy applied to all Israeli citizens. This provides the advantage of quickly shifting widespread beliefs about reproductive time horizons, but also entails the disadvantage of not providing statutory variation for identification. We, thus, employ a DID strategy, comparing groups within the country which are expected to be more rather than less affected by the policy. We use multiple control groups, detailed figures of the raw data, and a number of robustness and placebo checks to present a collage of evidence that the policy indeed causally impacted the outcomes we examine.

Our data come from the 2008 Israeli population census, which sampled approximately 20% of Israeli households. These data are combined with administrative tax data, to accurately measure income. Our analysis is restricted to native-born Israelis, to avoid potential bias from large immigrant inflows over time, especially the mass migration of Jews from the former Soviet Union that was at its height from 1989 to 1992 (Goldner et al., 2014). Our "treated" group is Jewish-Israeli women, who are most likely to be responsive to a change in IVF access.

We use Jewish-Israeli men as a comparison group to absorb changes that could have affected the overall educational market among Jewish Israelis, such as changing life expectancies, economic shifts or changes in labor demand, and demographic changes. Given that men do not experience the same drop in fertility with age as women, IVF funding is unlikely to affect their expectations regarding age-related levels of fecundity.[23] They may, however, be affected via the choices of their female partner or by equilibrium effects, but these indirect effects are not expected to exceed the initial impact on women.

We use Arab-Israeli women to absorb changes that could have specifically affected women in Israel, distinctly from men. One important example is an ongoing reform in Israel that increased access to higher education, especially for disadvantaged populations in peripheral areas. This reform may have differentially impacted women, if, for instance, moving away from home to acquire education is more costly for women than for men, or if it helped remove cultural barriers to women's education. When it comes to IVF availability, however, Arab-Israeli women are not expected to be affected as much as Jewish women. First and foremost, they were much less likely to use IVF at the time of its introduction for religious reasons (Remennick, 2010). Whereas Judaism essentially allows all ART practices, Islam places significant restrictions on their use (especially on surrogacy and egg and sperm donations (Birenbaum-Carmeli, 2003)). Secondly, Arab women are exempt from military service and on average begin college 3-4 years younger than Jewish women in Israel (Danziger and Neuman, 1999). This generates a lower concern regarding the impact of college education on future fertility.[24] While this strategy may have its own potential confounding factors,

[23]Similarly, in Jayachandran and Lleras-Muney (2009), women's life expectancy was updated by declines in maternal mortality, while men's remained unaffected.

[24]See for example CBS report "Arabs in Higher Education in Israel - First Year Students for First Degree in 2011/12" issued October 21st, 2014. It should also be noted that the variation in the age of college applicants is much larger for the Jewish population (based on CBS data processed and presented by Mr. Aviel Kranzler, Higher

they should be orthogonal to any issue presented by the male control group. Thus, if we estimate similar effects using the two strategies, it is unlikely that they are both caused by a single omitted factor.

The main identifying assumption in the DID framework is that pre-trends are parallel for the treatment and control groups. Although in many areas in the world women and men were exhibiting substantially different trends as educational gaps were narrowing during the last decades, as we show below in Figure 2, , for the cohorts considered here, native-born Jewish women had already caught up with men in levels of graduate education and exceeded men in levels of college education.[25] Therefore, when we compare these two groups, pre-trends are quite similar. Unfortunately, this is not the case for the groups of Arab and Jewish women, in which levels are extremely different and college completion pre-trends present some differences (with Jewish women increasing their college attainment faster than Arab women prior to 1994).

To address this limitation, we base our causal interpretation of the differential changes on the detailed figures of the raw data we present, which shows a discontinuous change for the 1994 college cohort. Our regression analysis supplements these figures by demonstrating the magnitude of the impact and statistical significance levels. Our regression results are robust to the inclusion of group-specific time trends, and we show specification both with and without to confirm that the results are not sensitive to this choice.

Given that our data is from a single 2008 cross-section, to avoid censoring, we limit the sample to Jewish women born between 1954 and 1975.[26] We identify treated individuals as those from the cohorts that were still at the relevant age for educational decisions at the time they learned about the increased access to IVF. In our main specification, we use the median age at college or graduate school entry (based on Macro-level data) in the relevant period to determine which cohort is the first to be affected by the policy change with regards to the outcome variable. Because there are substantial differences between the treatment and control groups, the treated cohort is calculated separately for each group, based on its specific median age. The median Jewish-Israeli man enters college and graduate school one year later than the median Jewish-Israeli woman, and the median Arab-Israeli woman enters three years earlier. We thus align the groups by college (or graduate school) cohort rather than by year of birth. Therefore, we use Jewish men that where born one year earlier than our treated women as control, and Arab women that were born three years later.

The definition of treatment by cohort is binary for most of the analysis, and simply indicating whether or not an individual belonged to the cohort most relevant for college (or graduate school)

Education and Science department at CBS.)

[25] This early catch-up relative to other countries could be the result of multiple reasons, including men's longer period of military service or the low educational rates among ultra-Orthodox men.

[26] 1954 was chosen as the starting point of the range in order to have a reasonable number of observations for Israeli-born Jews (the state of Israel was founded in 1948). The end point of our data range was chosen to avoid censoring in educational outcomes among individuals who may not have completed their education at the time of the 2008 Census. Thus, we analyze Jewish women not younger than 33 years old.

entry in 1994 or after. For college education, we use a secondary analysis with a continuous definition of treatment, based on the pre-treatment age distribution at college entry for each individual's gender and population group. This distribution is calculated using the 1995 census data which indicates for each individual whether they were enrolled in college at that time and the year of study. Then, by year of birth, we take the share of individuals that were not yet enrolled in college and could have therefore altered their college enrollment decisions in 1994 (or after). Note that this is imputed based on the distribution for 1993, the last pre-treatment year and in a different sample than the one we use for our analysis (the 1995 census sample versus the 2008 census sample). For each group, the intensity of treatment naturally increases by year of birth, as younger individuals were more likely to have not entered school yet, and thus able to respond to the introduction of IVF and potentially change their human capital investments accordingly. This approach also takes into account the fact that the variation in college age is larger for men compared to women and for Jewish-Israelis compared to Arab-Israelis.

In our main DID specification we estimate the following equation, separately for each of the two control groups:

$$Education_{i,c} = \beta_1 treated_i + \beta_2 treated_i \times post_c + X_i' \gamma + \delta_c + \epsilon_i$$

where $Education_{i,c}$ is an indicator for individual i from college (graduate school) cohort c completing a college (graduate) degree. $treated_i$ indicates that the individual is a Jewish-female, and $post_c$ indicates whether the cohort was young enough to enter college (graduate school) after the policy change, X_i is a vector of individual level characteristics including parents' origin and religiosity level, and δ_c is a cohort fixed effect. In addition, we include linear group-specific time trends to account for the differences in pre-trends that specifically exist among the Arab-women control. However, because, in some settings, adding such trends could actually bias the estimated effects, we also estimate the same specification without these time trends to show that the results are not sensitive to this choice. As mentioned above, for college, we estimate the same equation using a continuous measure of exposure to treatment (or gradual exposure).

We cluster our standard errors at the $group \times cohort$ level. Then, to address concerns for serial correlation, we also collapse the data to group-cohort cells and run Generalized Least Squares (GLS) regressions with an explicit AR(1) error structure that allows for correlation both *across* and *within* panels, (as in Chandra et al. (2010)).[27] In addition, we show permutation tests for each of our main results, demonstrating that our effects are "large" relative to the actual variation in the data.

In light of other potential long-term societal trends that could have divergent effects on different

[27] The within-panel correlation factor accounts for serial correlation, assuming an AR(1) process with a unique autocorrelation parameter for each panel (i.e., gender).

population groups, we perform several analyses to provide further evidence that the 1994 IVF policy change drives our results. We first show that high-school graduation, which would not be affected by fertility concerns, is not affected by the 1994 IVF reform. Having already shown that Arab women were not equally affected, we then examine whether there is any possibility that a broader expansion of higher education that occurs over time had a differential effect on Jewish women. To accomplish this, we run the main regressions with explicit controls for the number of colleges and the number of teacher-training colleges that were open each year, allowing for a differential impact by population group.

Next, to rule out the possibility that broader international trends, such as the global trend of women seeking more education, are responsible for our results, we conduct placebo analyses on data from several other countries, showing that no country registered a similar discontinuous increase in education for the 1994 cohort. To verify that censoring due to the retrospective nature of the data does not bias the results, we compare our main findings to a placebo analysis using the exact same techniques used in the 1995 Census. All of these exercises confirm that our effect is not driven by educational trends in Israel, nor by broader global trends, thus attributing the upsurge in education to the expected future IVF availability.

We then look at women's career outcomes and income as dependent variables and compare them to men to examine whether these "treated" cohorts of women actually benefited in labor markets. Our primary goal is to assess whether this change contributed to narrowing the gender gap. In this case, the Arab control group is not appropriate. However, to show that what we capture is not just a pre-existing gender differential trend in labor markets, we compare the results for the Jewish population to a placebo analysis on the Arab population.

Table 1 shows summary statistics for our sample, comparing Jewish women to Jewish men and Arab women in the pre-period. First, it is apparent that the means for educational outcomes as well as income differ substantially between the Jewish and the Arab populations. Jewish men and women are more similar in education levels, but not in income. Although Jewish women exceed men in higher education levels even in the pre-change period, this should not be mistakenly interpreted as a sign for lack of gender-divergent constraints. As discussed in section 2, women, nowadays, exceed men in college education, but still earn substantially less in the USA and in many developed countries. Since women may experience differential returns on college education, it is possible that without reproductive limits, and the perception of these limits as a barrier to future career investment, women's "steady state" level of attending college would be even higher in the counterfactual.

In addition, the last two rows of the table, present the pre-trends in education and compare them across the three groups. These pre-trends are relatively small for all groups. However, while our treatment group exhibits a slightly stronger pre-trend for college, the opposite is true for graduate education. As mentioned above, we control for these differential pre-trends in our regressions.

Table 1: Summary Statistics

Pre-1994 college cohorts:	Jewish women N=53,014		Jewish men N=49,657		Arab women N=20,152	
	Mean	SD	Mean	SD	Mean	SD
Ultra-Orthodox	0.06	0.23	0.06	0.24	N/A	N/A
European-born mother	0.25	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
European-born father	0.29	0.43	0.28	0.45	N/A	N/A
Asian/African-born father	0.53	0.50	0.52	0.50	N/A	N/A
Income in Shekels	98,167	92,393	186,757	173,543	53,299	49,999
Age	45.42	5.39	47.46	5.34	42.24	5.66
Age at first marriage	23.57	3.91	25.86	3.86	21.03	4.11
Highschool completion	0.9	0.30	0.84	0.37	0.44	0.5
College Degree	0.33	0.47	0.28	0.45	0.09	0.29
Graduate Degree	0.12	0.33	0.11	0.31	0.02	0.12
College Degree pre-trend (SE)	0.006	(0.0004)	0.005	(0.0004)	0.003	(0.0004)
Graduate Degree pre-trend (SE)	0.0003	(0.0005)	0.0018	(0.0005)	0.0004	(0.0003)

Notes: Data from the 2008 Israeli population census (20% sample), restricted to Israeli-born. The sample is pre-1994 college cohorts, namely individuals below the median age at college entry for their population group (at the time). Thus, for Jewish women the sample is those born 1954 - 1970, for Jewish men 1953 - 1969 and for Arab women 1957 - 1973.

Nonetheless, in the next section, we proceed by examining in more detail in figure 2 how these outcomes evolve over time.

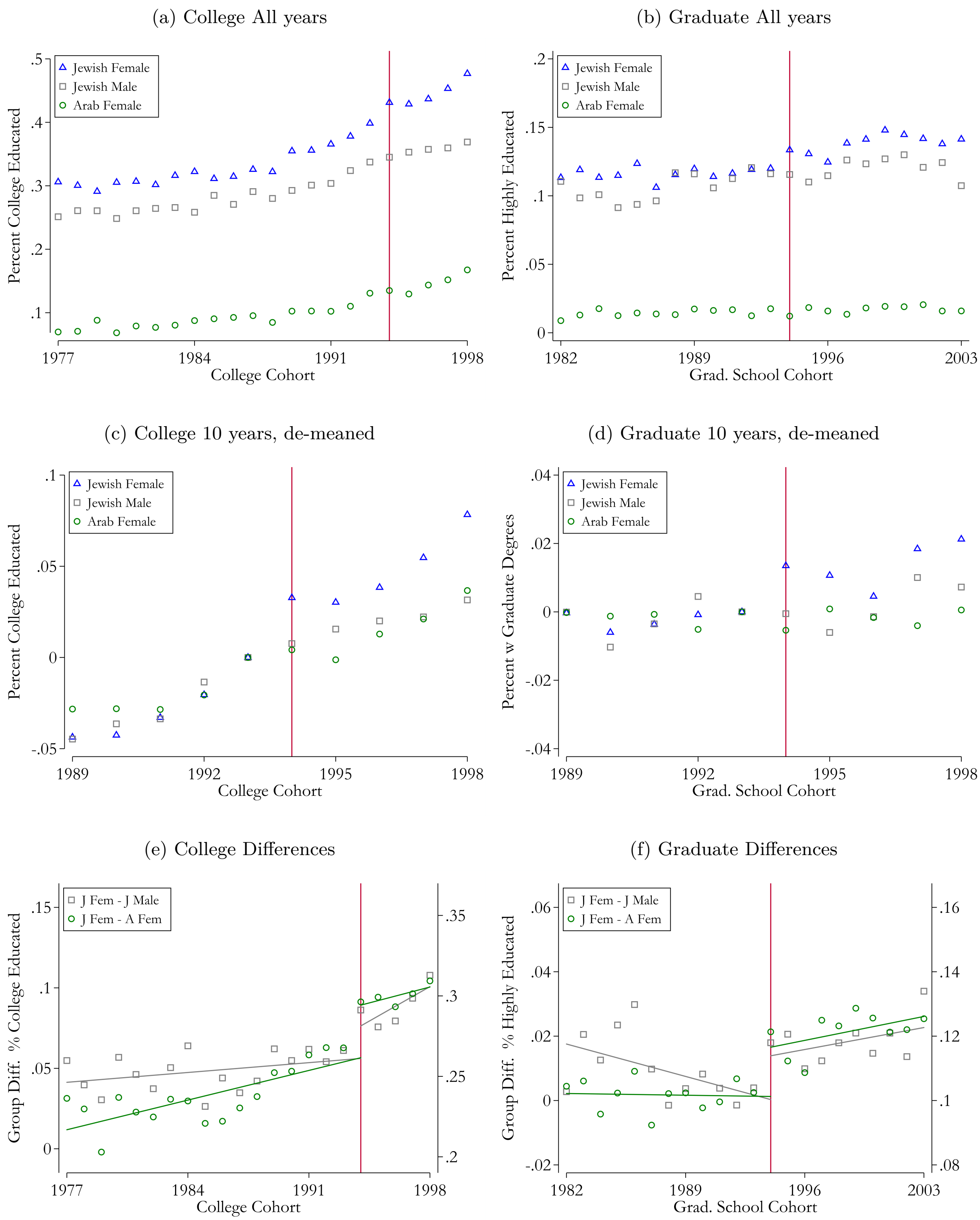
4 The Impact of Extended Fecundity on Women's Education

4.1 Main Results

As explained above, we expect decisions on both college and graduate education made by Jewish women to be affected by the change in expected fecundity, in light of the fact that in our high-fertility, late college-entry setting, even decisions on college-attendance may potentially be constrained by the expected age-related decrease in fecundity.

We first plot the raw data on these outcomes for the treatment and two control groups, to examine if there was a shift in higher education attainment for the treatment group that stands out relative to the trends in both control groups. We focus mainly on the analysis of these plots since both of our control groups have limitations (discussed above) and in light of the fact that pre-trends are not always parallel. Therefore, regression result cannot be interpreted as causal without a careful inspection of the patterns in the raw data.

Figure 2: Higher Education Rates - Jewish Women vs. Comparison Groups



Notes: Figure (a) shows average college attainment for Jewish women, Jewish men and Arab women by college attendance cohort, based on the median age at college entry for each group. Figure (c) presents the same averages de-meaned (using the pre-treatment mean) for a period of 10 years around the year of change (1994). Figure (e) plots the differences in average college attainment between Jewish women and men, and between Jewish and Arab women. Figures (b), (d) and (f) present the same measures for graduate education by graduate school cohort. Data from the 2008 Israeli population census, restricted to Israeli-born.

In the top panel (a) – (b), we show average education level by college (or graduate) cohort, which is defined according to the median age at college (or graduate school) entry for each group separately. For example, for Jewish women the median age for college just before 1994 was approximately 23, and therefore the women considered as the 1994 college cohort are the ones that turned 23 on that year (i.e., born in 1971). Panel (a) clearly shows an increase in college completion rates for Jewish women after the policy change, which is not matched by either control group. This is even more apparent if, in panel (c), we zoom in on the five-year periods immediately before and after the change, and de-mean the figures in order to bring the plots closer to each other. In panel (e) we plot the differences between the treatment and each control group. We use the same scale over a different range in order to overlay the two plots, since baseline differences are not the same between the two control groups. Despite an apparently stronger differential pre-trend for the Arab control group, this figure shows a remarkably identical discontinuous change in differences for both control groups after the introduction of the new policy.

Panels (b), (d) and (f) present the same plots for graduate level education, and lead to similar conclusions. Once again, we see here a strikingly similar discontinuous increase for Jewish women relative to both control groups. The pre-trends for this outcome are much flatter for all groups, however, we observe an irregularity in the trend for the graduate school cohorts of Jewish men between 1983 and 1986. These cohorts appear to have lower shares of graduate degrees relative to the general trend. This could be attributed either to the events of the 1982 Lebanon war or to the period of hyper-inflation in Israel between 1980-1985.[28] Nevertheless, the post-period increase for Jewish women relative to Jewish men and Arab women in the years closer to the 1994 cutoff is very clear. Panels (d) and (f) show that the magnitude of the increase is strikingly similar relative to both control groups.

In addition, a pattern of a steady post-treatment increase in differences for both outcomes and for both control groups is apparent in panels (e) and (f). This could have two plausible explanations. First, because we define treatment by the median, later cohorts potentially have more undecided individuals that could be affected by the treatment. Second, to be effective, the new IVF possibilities must become widely known, and, naturally, over time, more and more people become aware of the innovative technology (possibly by observing others successfully becoming parents with IVF). Therefore, the impact of the policy change keeps increasing after 1994.

Tables 2 and 3 present regression results that allow us to quantify the size of the effect more precisely, when individual characteristics are controlled for. We show the results for three different specifications using the two alternative control groups. Columns (1) and (2) in Table 2 show that the differential impact on college graduation for Jewish women relative to either control group is 2.7 percentage points (approximately 6.75% increase relative to the 1993 level) when group specific

[28] These years also stand out relative to cohorts earlier than 1982, which are not shown in the figures here. If anything, this irregularity would bias our results downwards if we ignore pre-trends in the regression analysis.

linear time trends are accounted for. The estimated effects are slightly larger for the male control group and more than double for the Arab control group when these trends are not included in the regression. Based on Figure 2, we assume that these later estimates are less reliable, especially for the Arab control group, however, we show both specifications to demonstrate that the positive impact is not driven by the choice of specification.

To account for variation in college entry age, and thus treatment status, we present a regression in columns (5) – (6) that classifies a portion of each cohort in each group as potentially exposed to treatment which is based on the percentage of individuals that would have not yet entered college at that time point, according to data from the 1995 Census on college entry ages. We allow for different percentages in each gender and population group which account for the varied college entry age distribution among these groups.[29] Again, results show a significant positive impact in the affected cohorts for Jewish women.

Table 2: College Attainment Rates

	Binary Treatment Timing				Gradual Exposure	
	(1) Jewish Men	(2) Arab Women	(3) Jewish Men	(4) Arab Women	(5) Jewish Men	(6) Arab Women
Treated × Post	0.026***	0.027***	0.038***	0.059***		
	(0.005)	(0.006)	(0.003)	(0.005)		
Treated × Exposure					0.021*	0.086***
					(0.011)	(0.029)
Group time trend	√	√				
Cohort FEs	√	√	√	√	√	√
Observation	144889	102829	144889	102829	144889	102829

Notes: All columns present estimates based on an OLS difference-in-differences regression with cohort fixed effects, using either Jewish men (in the odd numbered columns) or Arab women (in the even numbered columns) as a control group. In columns 1—4 the cohort is defined by college-entry year for the median student in the group, and in columns 5—6 the cohort is defined by year of birth. In columns 1—4 "post" indicates that the individual belongs to a cohort young enough to change decisions on college education. This is determined on the basis of the median age at college entry at the time of the IVF policy change announcement. In columns 5—6 we use a measure for the level of exposure for each cohort, which is calculated on the basis of the entire age at college entry distribution for each specific group (Arab women, Jewish women, Jewish men). In columns 1,3, & 5 we also control for religiosity and parents' origin (these controls are irrelevant for the Arab population). Robust standard errors clustered at the group × cohort level in parentheses. Data from the 2008 Israeli population census, restricted to Israeli-born individuals.
*** p<0.01, ** p<0.05, * p<0.1

Proceeding with our analysis to graduate education, we report in Table 3an impact of 1 to 1.8 percentage points, for the male and Arab control groups, respectively. This implies an increase of 8.3 to 15 percent, relative to the 12% share of Jewish women with graduate degrees in the 1993 cohort. In the case of graduate education, due to the irregularity for men in the earlier years of the

[29]The differential and gradual exposure to treatment specification captures a separate non-linear time trend for each group. For this reason, we do not include the group specific time trends in this specification. In addition, we use year of birth rather than cohort fixed effects.

pre-period, the Arab control group may be more reliable. However, in both cases, the percentage increase in graduate education is larger than in college education. This may be intuitively explained by the fact that fertility constraints are more of a concern for educational investments that occur later in life.

In columns (5) and (6), we estimate the effect on graduate education conditional on completing college education. We use this last specification to understand whether graduate education has increased as a natural consequence of the increase in the rate of college education, or whether there has been an increase in graduate education over and above the mechanical impact of increasing the pool of college graduates. Some evidence to the latter may be the that the timing of the effect is for the 1994 graduate cohort, which was not yet "treated" in their college-attendance. Nonetheless, in columns (5) and (6) we directly observe an impact on graduate school even when conditioning on college completion (although the estimate is not significant for the male control group). Obviously, since we are conditioning on an endogenous variable (college education is also impacted by the treatment) the magnitude of this effect should be interpreted with caution. Nonetheless, both the larger results for graduate education, and the further increase conditional on college education, support our main hypothesis that extended later-life fertility for women drives the observed shifts, since decisions on graduate education are made at an older age, in which time, expected fertility plays a much more important role and portrays the type of long investments that we believe to be constrained by fecundity horizons.

Table 3: Graduate Education Rates

					Conditional on College	
	(1) Jewish Men	(2) Arab Women	(3) Jewish Men	(4) Arab Women	(5) Jewish Men	(6) Arab Women
Treated × Post	0.010**	0.018***	0.006**	0.023***	0.023	0.037**
	(0.004)	(0.003)	(0.003)	(0.002)	(0.018)	(0.018)
Group time trend	√	√				
Cohort FEs	√	√	√	√	√	√
Observation	144889	102829	144889	102829	48614	30321
R-Squared	0.0456	0.0278	0.0456	0.0278	0.0159	0.0210

Notes: All columns present estimates based on an OLS difference-in-differences regression with cohort fixed effect, using either Jewish men (in the odd numbered columns) or Arab women (in the even numbered columns) as a control group. The cohort is defined by graduate-school entry year for the median student in the group. "Post" indicates that the individual belongs to a cohort which is young enough to change decisions on graduate education. This is determined based on the median age at graduate-school entry when the IVF policy change was announced. In columns 5—6 we restrict the sample to college graduates. In columns 1,3, & 5 we also control for religiosity and parents' origin (These controls are not relevant for the Arab population). Robust standard errors clustered at the group × cohort level in parentheses. Data from the 2008 Israeli population census, restricted to Israeli-born individuals.

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

4.2 Robustness Checks and Alternative Explanations

In this section, we first discuss potential data and estimation issues and then describe different tests conducted to establish that our reported effects are most likely driven by the IVF policy rather than other events or policy reforms.

Alternative approaches to standard-errors calculations In micro-data DID studies, one may be concerned that a high degree of intra-group correlation or correlation across time periods is driving the results' significance . We first address this issue by providing GLS estimates that allow for correlations within groups and across years. This strategy is explained in further detail in section 3.2 above. The results are reported in table 4, which presents estimates for college and graduate education, both with and without group specific time trends. All estimates are similar in magnitude and significant at conventional levels, except for the coefficient in column 5, for graduate education with the male control group which is of similar magnitude but not statistically significant. As mentioned above, the time series for men has several unusual values in the beginning of the pre-period, which may decrease the precision of our estimates, especially when the data is collapsed to group-cohort cells.

Table 4: GLS Regressions for Education Outcomes

	College				Graduate			
	(1) Jewish Men	(2) Arab Women	(3) Jewish Men	(4) Arab Women	(5) Jewish Men	(6) Arab Women	(7) Jewish Men	(8) Arab Women
Treated × Post	0.025***	0.027***	0.045***	0.050***	0.009	0.017***	0.010**	0.018***
	(0.006)	(0.009)	(0.007)	(0.011)	(0.008)	(0.003)	(0.004)	(0.005)
Group time trend	√	√			√	√		
Cohort FEs	√	√	√	√	√	√	√	√
Observation	44	44	44	44	44	44	44	44

Notes: All columns present estimates based on a Generalized least squares difference-in-differences regression with data collapsed to the gender—population-group—cohort level. All specifications include cohort fixed effects, and use either Jewish men (in the odd numbered columns) or Arab women (in the even numbered columns) as a control group. In columns 1—4 the dependent variable is college completion and cohort is defined by college entry year for the median student in the group. In columns 5—8 the dependent variable is graduate-school completion and cohort is defined by graduate-school entry year for the median student in the group. "Post" indicates that the individual belongs to a cohort which is young enough to change decisions on college education in columns 1—4 or on graduate education in columns 5—8. This is determined based on the median age at college or graduate-school entry in 1994. 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 individuals.
*** p<0.01, ** p<0.05, * p<0.1

As an additional check that our estimates are large relative to the variation in the data, we also perform a permutation analysis on the treatment effect coefficient for our main outcomes. The results of this analysis are presented in Figures A2. To plot these distributions of effect sizes, we randomly draw from the entire study period a number of years equal to our true number of treated years, and run our baseline regression (including group specific time trends) on these randomly selected years as the "treatment" period (for an example of this approach, see Agarwal et al. (2014)). This approach accounts for intra-group correlation and other non-standard error structures. We perform 1,000 such random draws, and compare our true treatment coefficient to the resulting distributions. Results show that our true effect is outside of the curve or in the far-right tail, with less than 5% of the values above it, for every outcome measure and for each comparison group.

Censoring or other data issues Another possibility is that the effect we observe is an artifact created by looking at outcomes retrospectively in cross-sectional data. This concern is minimized as we carefully choose the years and cohorts which constitute the sample for each outcome, and compare the groups by college (or graduate school) cohort. The latter helps overcome possible differential censoring across groups. However, to further verify that the retrospective nature of the analysis could not create similar breaks in the data *without* a real policy effect, we use the 1995 Israeli Population Census to conduct a placebo test. We replicate our analysis with a fake "policy change" in 1981 (14 years prior to the Census year, as the real 1994 policy change is 14 years before the 2008 Census) and find no evidence of a break in college or graduate education, as shown in Figure 3(a).

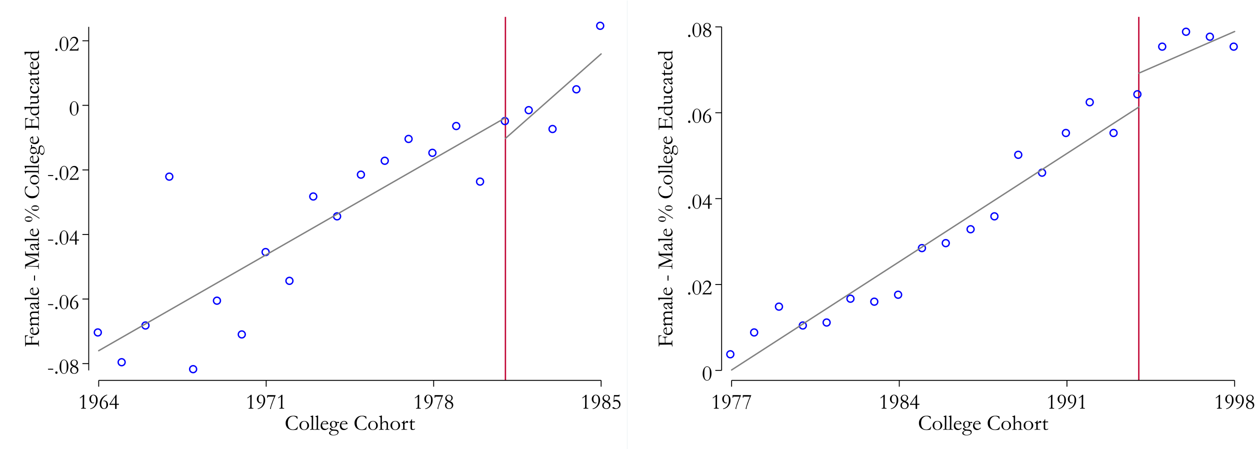
Global shock After establishing that the break we observe is genuine and significant, we turn to explore the possibility that we have misattributed the source of this dramatic change and explore several alternative explanations to these findings.

To verify that broader international trends during the nineties are not responsible for our effects, we conduct placebo tests in the United States as well as four other OECD countries with GDP per capita similar to Israel, and Census data availability: Greece, Spain, France, and Portugal. Results for college education in the four "comparable" countries are shown in Appendix figure A3. We also look at the United States American Community Survey in Figure 3(b). None of these placebo tests show a positive differential change for women compared to men as we see in Israel, indicating that there was no broader global shock affecting those entering college in 1994.

Other health expansions Another possible explanation to the observed changes in women's outcomes specific to Israel could be the general change in health services provided by the NHI law. Although improved health services could have some effect on education (due to an increase in life expectancy for instance) or on marital outcomes (if we believe that age serves as a proxy for spousal health in general, rather than just fertility), it seems less likely to explain the gender differential change that we identify. Moreover, only a very dramatic change in access to health services would have an impact on either life expectancy or age-related health perceptions. In practice, only 5%

Figure 3: Historical and US Placebo Tests

(a) 1995 Census (b) United States



Notes: Figure (a) shows a placebo test for a global 1994 shift by plotting the difference in college attainment between women and men in the US by college cohort (based on the median age for college entry in the US). Data from the 2008 American Community Survey. Figure (b) shows the difference in college attainment between women and men in Israel using the 1995 census with a placebo treatment timing in 1981 (14 years before the census year).

of the population had no health insurance prior to the reform. The remaining 95% were covered by one of the four HMOs, and experienced a very moderate change in coverage, except for very specific services such as IVF.[30] In addition, in Gershoni and Low (2020) we show that the change in total health expenditures, both privately and from the government, was smooth around the time of the policy change.

It should also be noted that during 1994, prior to the actual execution of the NHI reform, many believed that it was bound to increase the cost of health care, and decrease the quality of the services.[31] Due to the complexity of the reform, which was aimed mainly at changing the institutional structure of the health system in Israel, the uncertainty surrounding the actual results of the reform lingered for decades, and even nowadays the debate on its consequences continues.[32] Therefore, it is plausible to assume that the 1994 health reform generated a certain degree of uncertainty or confusion regarding its costs and benefits, which did not have a systematic effect on young people's health expectations.

Higher-education reform To rule out a general change in education-seeking, either by the treated cohort or based on other things that occurred in 1994, we perform a placebo test using high-school completion data. Individuals on the margin of completing high-school are unlikely to

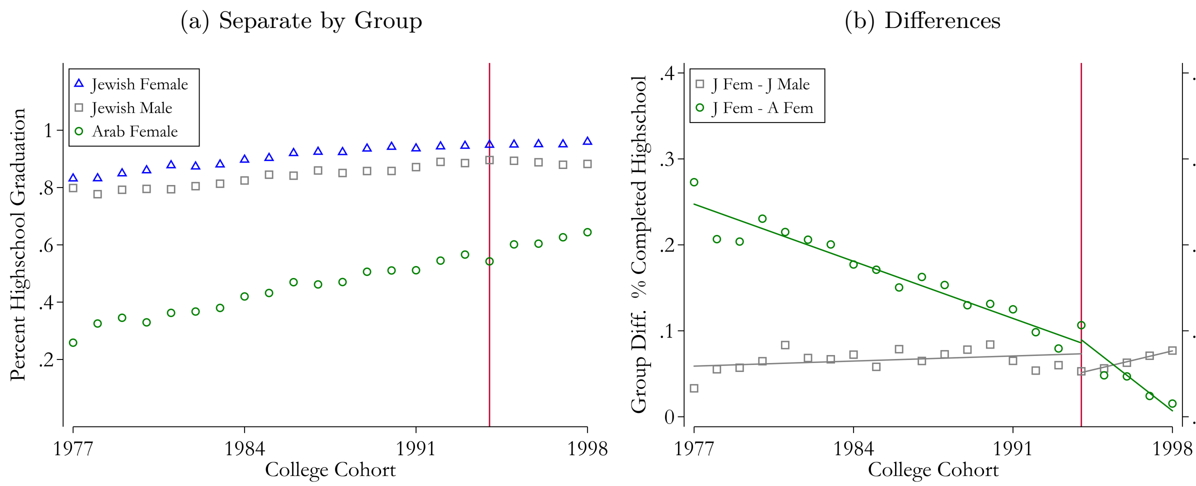
[30] Additional significant changes were made in coverage of learning disabilities treatment and psychotherapy, as well as in tests for early detection of cancer.

[31] The slogan of the anti-reform campaign was "pay more, get less", see *Yedioth Ahronoth*, December 15, 1994.

[32] See for example reports issued by the Ministry of Health: "20 Years to the NHI Law" (May 2015) "Light and Shadow in the evolvement of the NHI Law" (February 2010).

make high-school completion decisions based on older-age fertility prospects. Figure 4demonstrates that, as expected, there is no impact on this outcome for the 1994 college cohort.[33]

Figure 4: High-school Graduation Placebo



Notes: Figure (a) shows average high-school completion rates for Jewish women, Jewish men and Arab women by college cohort, based on the median age at college entry for each group. Figure (b) plots the differences in average highs-chool completion rates between Jewish women and men, and between Jewish and Arab women. Data from the 2008 Israeli population census, restricted to Israeli-born.

We then consider the higher education reform in Israel, which materialized throughout the eighties and nineties, as an alternative explanation. Prior to this reform, only universities could grant academic degrees. Starting in the seventies, colleges began to gradually receive permission to grant academic degrees equivalent to those given by universities. This process accelerated during the eighties and early nineties, culminating in an official and comprehensive plan for the development of academic colleges. In the decade between 1992 and 2002, the number of students in academic programs nearly doubled (the effect of the reform was already noticeable in the early nineties, but started to actually build up in 1997-1998 (Volanski, 2005; Bernstein, 2002)). It is important to note that despite the substantial changes, this reform was gradual. Thus, it could be ruled out as an alternative explanation to the discontinuous change in women's education attainment (as shown in Figure 2).

Nonetheless, to directly rule out this alternative explanation, in Table 5 we implement regressions controlling for a group-specific impact of the expanded educational access. We include

[33]This result is also supported by macro-level data on the number of high-school students passing matriculation exams ("Bagrut"). According to this data, while the number of students passing the test increases for both genders, the ratio of women to men remains stable for the Jewish population of Israel, while there is an increase in this ratio for the Arab population of Israel.

controls for the number of academic colleges and teacher-training colleges by year,[34] also interacted with group, to allow the additional educational access to have a differential effect for each group. Even with these additional controls for the direct impact of the educational reform, the 1994 break remains positive and significant for all outcomes, and point estimates are similar in magnitude (or even slightly larger).

Table 5: Higher Education Rates Controlling for College Expansion

	College		Graduate		Graduate|College	
	(1) Jewish Men	(2) Arab Women	(3) Jewish Men	(4) Arab Women	(5) Jewish Men	(6) Arab Women
Treated × Post	0.032***	0.046***	0.016***	0.022***	0.040**	0.047**
	(0.007)	(0.008)	(0.003)	(0.003)	(0.016)	(0.022)
Group time trend	√	√	√	√	√	√
Cohort FEs	√	√	√	√	√	√
Observation	144889	102829	144889	102829	48614	30321

Notes: All columns present estimates based on an OLS difference-in-differences regression with cohort fixed effect, using either Jewish men (in the odd numbered columns) or Arab women (in the even numbered columns) as a control group. In columns 1—2 the cohort is defined by college-entry year for the median student in each group, and in columns 3—6 the cohort is defined by graduate-school entry year for the median student in each group. "Post" indicates whether the individual belongs to a cohort which is young enough to change decisions on college education in columns 1—2, or on graduate education in columns 3—6. This is determined based on the median age at college or graduate school entry in 1994. In columns 1,3, & 5 we also control for religiosity and parents' origin (these controls are not relevant for the Arab population). Robust standard errors clustered at the group × cohort level in parentheses. Data from the 2008 Israeli population census, restricted to Israeli-born individuals.
*** p<0.01, ** p<0.05, * p<0.1

In addition, given the higher education reform's aims and target population, it is highly unlikely that our main results stem from it. During this reform, women already constituted more than 50% of undergraduate students. The main purpose of the reform was to make higher education institutions more accessible to a lower socioeconomic status population, mostly concentrated in peripheral regions (Volanski, 2005; Shavit et al., 2007), and to increase higher education supply to match the rapidly increasing demand.[35] Numerous studies were conducted to document the reform's consequences, none of which report a distinctive effect on women's participation in higher education (see for example Volanski (2005)). In fact, the percentage of female students in colleges (previously "non-academic" institutions) was actually lower than in universities.[36] On the other hand, based on these studies, Arab-Israelis were much more likely than Jewish-Israelis to be affected by the higher-education reform, because they have relatively low high-school achievements and are more concentrated in peripheral areas (Volanski, 2005). Thus, the increase we observe for Jewish

[34] Although the number of students varies by college, controlling for the number of students would be endogenous, and thus the number of institutions provides a better measure of the exogenous change.

[35] The demand increase stems from the growing rate of high-school graduates who received matriculation certificates (a pre-requisite for college application) (Shavit et al., 2007).

[36] The only exception is teacher's training colleges, where there is a vast majority of female students. However, academization process in these colleges took place in the early eighties. In addition, the students in these institutions constitute only a small share of the overall number of college students.

women relative to both Jewish men and especially relative to Arab women cannot be plausibly attributed to the impact of the higher education reform.

Finally, Similar reforms in other countries were not found to affect women differently than men. One example is the higher education reform in Spain, which was enacted at approximately the same years as in Israel, and did not change the trend of women's education or their marriage decisions (Mora, 1996).[37]

5 Gender Gap in Labor Market Outcomes

Our results illustrate that the cohorts of Jewish women that experienced a change in their fertility "deadline" due to the introduction of IVF, invested this time extension in higher-education, yielding both college and graduate degrees. In this section we present evidence that this additional investment in human capital translated into improved labor market outcomes, and played a role in narrowing the gender gap in the Jewish population of Israel, but not in the Arab population, where women were less likely affected by the new technology. If the educational effects of the policy translate into better career outcomes (rather than women achieving high-power education, but then dropping out of professional occupations, as in Bertrand et al. (2010)) it would suggest that differential fertility time horizons can meaningfully contribute to disparities in labor market outcomes.

Unlike the availability of contraception methods (or abortions) which impacts very early decisions and constraints among women and thus may determine whether a woman works at all, the availability of fecundity-extending technology would alleviates constraints for women who consider longer or more demanding career investments. Therefore, it is less likely to impact the decision whether *to* work, but rather allow women that already aim to work to prolong their career investments and to make more ambitious choices rather than to settle for lower-level or part-time jobs.

Table 6 presents DID estimates that measure the gender differential impact on several labor market outcomes for cohorts affected by the IVF policy change. The top panel presents the change in the gender gap for the Jewish population and the bottom panel shows the same for the Arab population, as a placebo test where an impact is not expected. It should be noted that although, on average, the Arab population is much more traditional this population also experienced education and employment improvements among women versus men for the relevant cohorts. Therefore, this placebo test can refute concerns that the effects shown for the Jewish population are driven by these more general trends for women versus men.

In light of the fact that we use outcomes reported on the census year (2008), we restrict this

[37] The lack of a differential shift for women in Spain is also demonstrated by our placebo analysis in Appendix figure A3.

analysis to a ten-cohort interval for each of the populations to minimize variation by age. We align these cohorts (as before) by the college entry cohort, which we regard as the treatment timing. For Jewish women, the sample includes women born between 1964 and 1973, in which case the youngest woman is 35 years old in 2008 (time of outcomes measurement) and the oldest is 44. Jewish men are one-year older, Arab women are 3 years younger and Arab men are 2 years younger.

As expected, in the top panel of the table, in columns (1) – (2), we do not see an impact on labor force participation or on employment for Jewish women. This offers a useful falsification test that no broader changes in gender outcomes over time is being picked up. However, the share of women who are employed full-time (35 hours a week or more), shown in column (3), increased by 2.5 percentage points (an almost 5% increase relative to the pre-change baseline of 55%), and this increase is highly significant.

Additionally, the share of women who are employed in prestigious occupations (column 4), that either require an academic degree (with the exception of schoolteachers) or include managerial duties,[38] increases by 1.6 percentage points and closes nearly one quarter of the 7 percentage points gap pre-change. Notably, these occupations include MBAs and lawyers, which have been shown to be occupations with a uniquely severe "motherhood penalty" that contributes to a large gender pay-gap (Bütikofer et al., 2018).[39]

Lastly, in column (5) we show that employed women's income, measured by tax data, also increases substantially by 6% relative to men, and relative to the time trend, showing a substantial impact on closing the gender pay gap in the treated cohorts.

One may be concerned that these estimates are picking up differential time trends in women's versus men's career outcomes over time, i.e., gender equity progress. This concern was already accounted for by the inclusion of gender-specific time trends, which control for linear progress over time. To further address this, we use the Arab population as a placebo group, to test if a more general trend toward gender equality exists in Israel. Using the same outcomes and specifications for the Arab population, point estimates show very small, inconsistent and insignificant changes for women, except for a substantial *decrease* in women's income relative to men.[40] These results establish that labor market outcomes improved specifically for Jewish women, who were impacted by the IVF reform, in the treated cohorts, rather than women's outcomes improving more generally.

This improvement in labor market outcomes – an increase in full-time employment, employment in prestigious occupations, as well as increased earnings – demonstrates that the increase in human capital investments shown for the 1994 college cohort was directly linked to other later-life career

[38] The latter are classified by the Israeli CBS as "managerial occupations".

[39] Additional examples of occupations that we define as "prestigious" are engineers, university lecturers and professors, medical doctors and government officials.

[40] Because participation and employment rates for Arab women are extremely low (even nowadays) and much of this population work in the informal sector (hence, their income is not included in our data which is based on tax records), this result could well be merely noise. Note that the other outcomes in Table 6 are based on census survey data, and are, hence, less likely to be biased by this informal work.

Table 6: Gender-Gaps in Labor Market Outcomes

	(1) In Labor Force	(2) Employed	(3) Employed Full-Time	(4) Prestigious Occupation	(5) ln(Income) | Employed
Jewish Population -					
Treated × Post	-0.004	-0.003	0.025***	0.016**	0.061**
	(0.006)	(0.005)	(0.008)	(0.007)	(0.023)
Observation	72,272	72,272	72,272	72,272	62,135
Arab Population -					
Treated × Post	-0.001	0.004	0.009	-0.007	-0.114***
	(0.010)	(0.010)	(0.011)	(0.012)	(0.034)
Observation	27,255	27,255	27,255	27,255	13,520
Group time trend	√	√	√	√	√
Cohort FEs	√	√	√	√	√

Notes: All columns present estimates based on an OLS difference-in-differences regression with cohort fixed effect. In the top panel, the sample includes Jewish individuals and in the bottom panel Arab individuals. In all columns and panels women are compared to men, and "post" indicates that the individual belongs to a cohort which is young enough to change decisions on college education (which is determined based on the median age at college entry in 1994). The outcome variables are specified above each column. In the top panel we control for religiosity and parents' origin (these controls are not relevant for the Arab population). Robust standard errors clustered at the group × cohort level in parentheses. Data from the 2008 Israeli population census, restricted to Israeli-born individuals.

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

investments and thus improved outcomes. By relaxing the gender-specific fertility time constraint, free IVF in Israel may have enabled women to delay marriage and childbearing in order to pursue more high-powered careers.

6 Conclusion

Increased access to *in vitro* fertilization offers women security in the form of a second-line option in the event that they cannot naturally achieve their desired level of fertility. Like any other insurance, this guaranteed access to IVF may influence individual behavior: women may be more willing to incur delays to marriage and childbearing in order to make human capital investments that yield future earnings. We show that when Israel introduce free IVF, the cohort entering college on the year of the policy change was substantially more likely to complete college degrees, pursue graduate education, and proceed to higher paying, more prestigious occupations and outcomes.

We use the empirical strategy of comparing Jewish-Israeli women's outcome before and after the policy change to those of both Jewish-Israeli men and Arab-Israeli women. These two control groups were chosen as men's reproductive time horizons are not directly affected by the policy change, whereas, Arab-Israeli women are much less likely to use IVF. Defining the treated cohort as women at a young enough age to be entering college (or graduate school) at the time of the change, we find a 6.75% increase in college completion, an 8–15% increase in graduate education, and a 6% increase in income conditional on employment. Our analysis is robust to including group specific time trends, using a GLS model to explicitly account for serial correlation, and a permutation approach to standard errors. Moreover, we use several placebo tests including the data from the US, previous Israeli census, and other OECD countries to help verify the source of impact.

Our findings demonstrate that the beneficiary population extends far beyond women who actually use IVF or other assisted reproduction technologies. Rather, the guaranteed access acts as insurance in the event that natural conception fails. Therefore, all women considering further educational investments or delayed marriage may benefit from it. The relevance of this information lies in the hefty cost per user entailed by Israel's generous IVF coverage, and Israel's current consideration of measures to limit the policy, which already include age limits, and restricted number of cycles per woman. The potential benefits from the "insurance" effect for younger women are not something policymakers typically consider when weighing IVF funding.

By examining the effects of attenuating later life infertility, this research suggests that time-limited fecundity is a key source of asymmetry between men and women. Women who are better insured against later-life infertility invest more in education and achieve better career outcomes. In the absence of such insurance, the female-specific sharp decline in fertility may contribute to lower human capital investments by women in their reproductive years, and consequently to the gender wage gap. In Israel, changing fertility horizons appears to substantially impact college and graduate education, both because women start families at a fairly young age and have relatively high desired fertility rates, and because obligatory military service delays any decision women make by at least two years. In other OECD countries, however, this investment tradeoff may take place after women have already completed their higher education, when further on-the-job investments are required to climb the corporate ladder: late nights at the law firm, medical residencies, or the tenure sprint. Thus, depreciating reproductive capital may help to explain the absence of women from higher-level management positions as well as the high-skill gender wage gap. A wide range of policies, such as increased support for child-rearing in two-career households as well as access to maternity leave and career re-entry, in addition to access to assisted reproduction technologies, could help alleviate this tradeoff.

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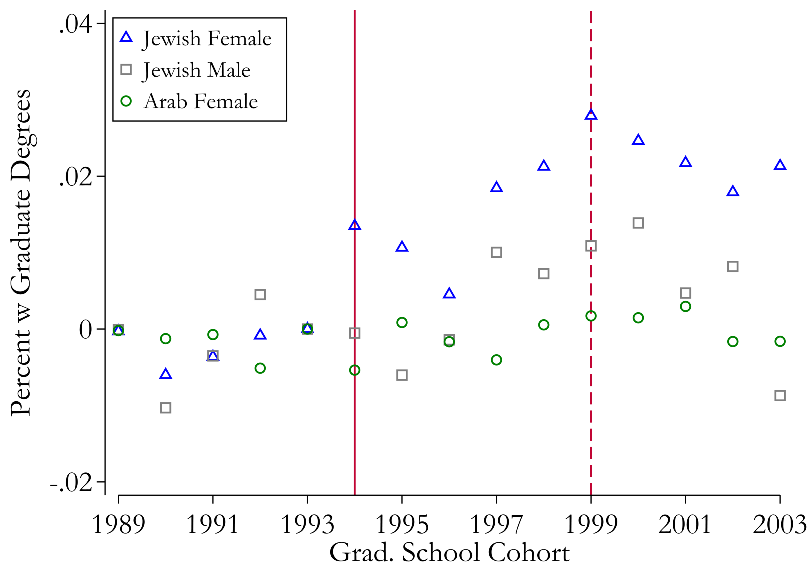
7 Appendix

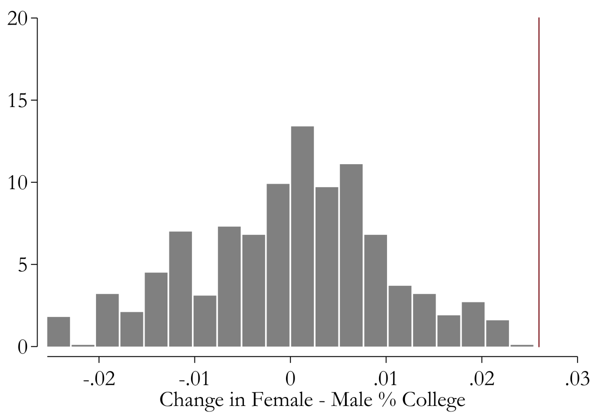
Figure A1: Graduate Education including Treated College Cohorts



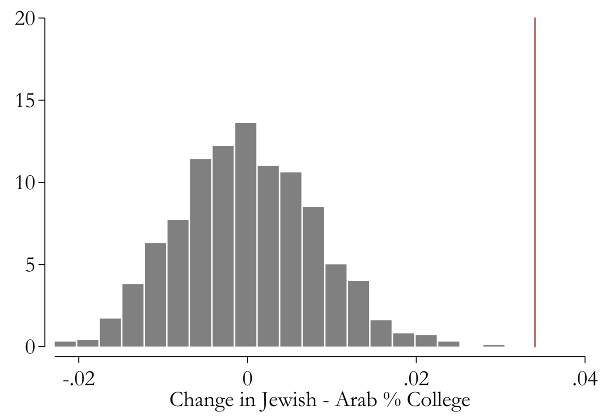
Notes: The figure shows average rates of graduate education for Jewish women, Jewish men and Arab women by graduate-school cohort, based on the median age at graduate-school entry for each group. The solid vertical line indicates the timing of the policy change for cohorts that make decisions on graduate education. The dashed vertical line indicates the timing of the policy change for cohorts that were young enough to change decisions both for college and for graduate education.

Figure A2: Permutation Approach to Significance Levels

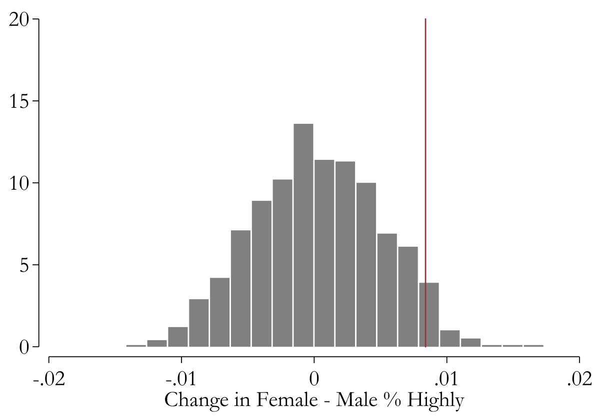
(a) College Education, Male Control



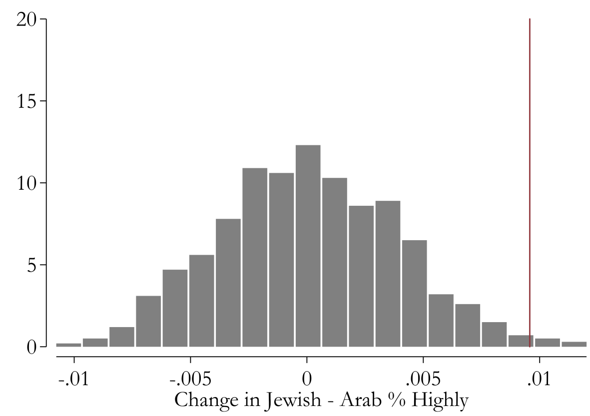
(b) College Education, Arab Control



(c) Graduate Education, Male Control

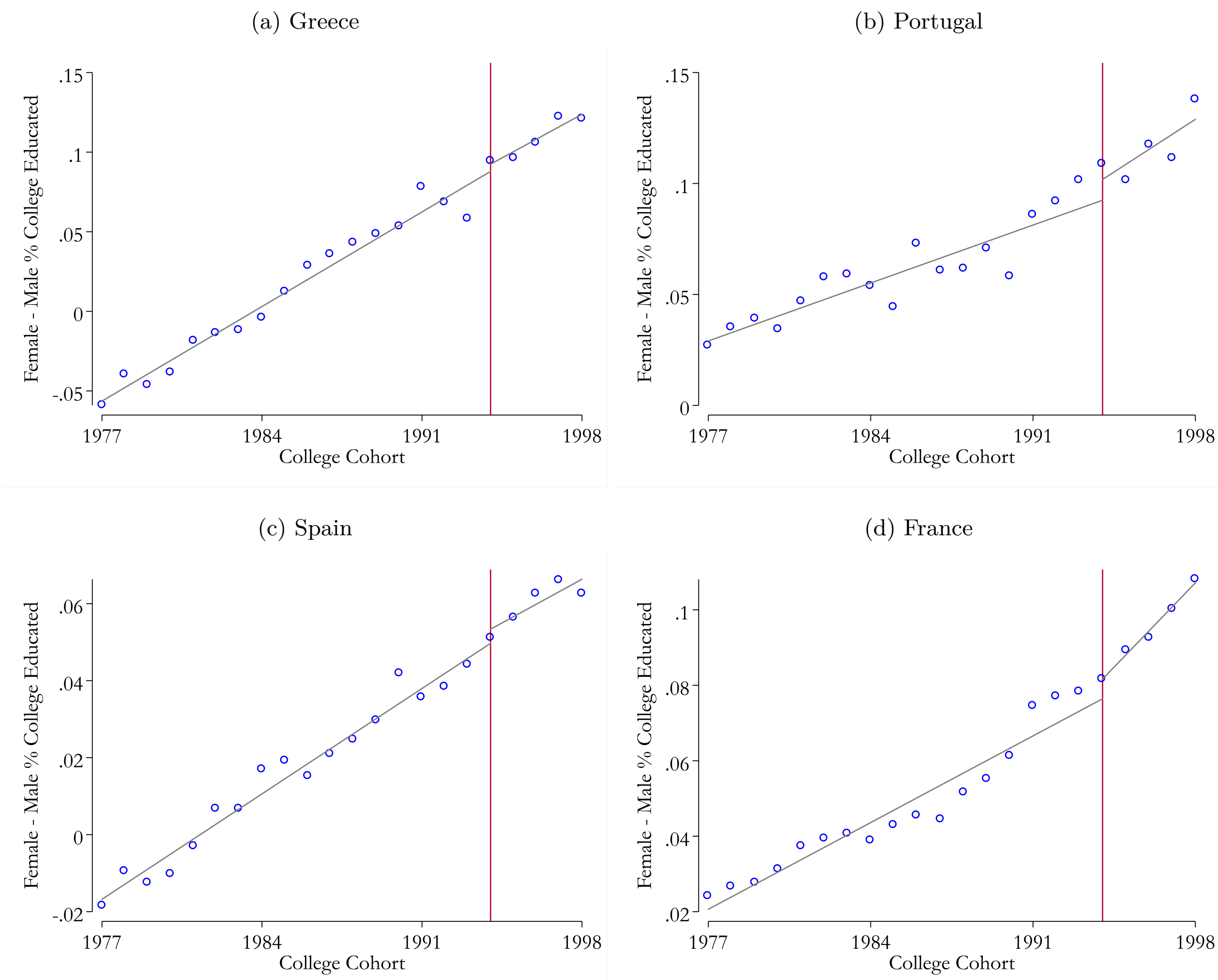


(d) Graduate Education, Arab Control



Notes: The figures plot the distribution of estimated effect sizes for college or graduate education from a regression as in the main specification using the same number of "treated" years as in the true model, but randomly drawing them from the study period (for an example of this approach, see Agarwal et al. (2014)). We perform 1,000 such random draws. The red line marks the effect size of the actual treatment year. The figure on the left hand side performs this analysis using Jewish men as control whereas the figure on the right hand side uses the Arab women control group.

Figure A3: International Placebo Tests



Notes: The figure presents placebo tests for a global 1994 shift by plotting the difference in college attainment between women and men in four different countries — Greece, Portugal, Spain and France — by college cohort (based on the median age for college entry in each country). Data from Integrated Public Use Microdata Series, International.

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

Woman's 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 et al. (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 rate with IVF relative to baseline success.