Appendix
Out of Labor and Into the Labor Force?
The Role of Abortion Access, Social Stigma, and Financial Constraints

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A Additional Figures and Tables

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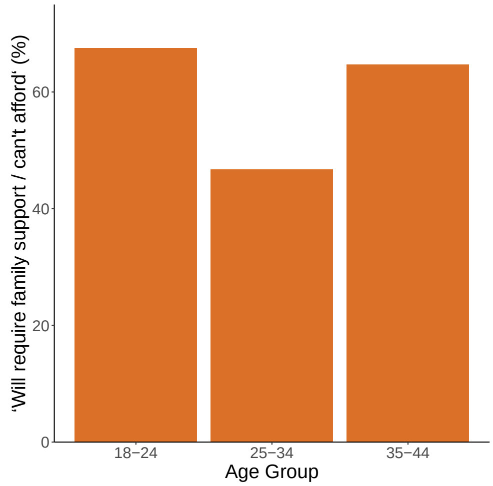
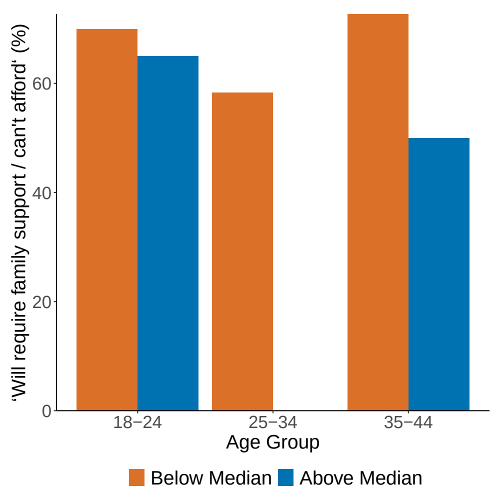


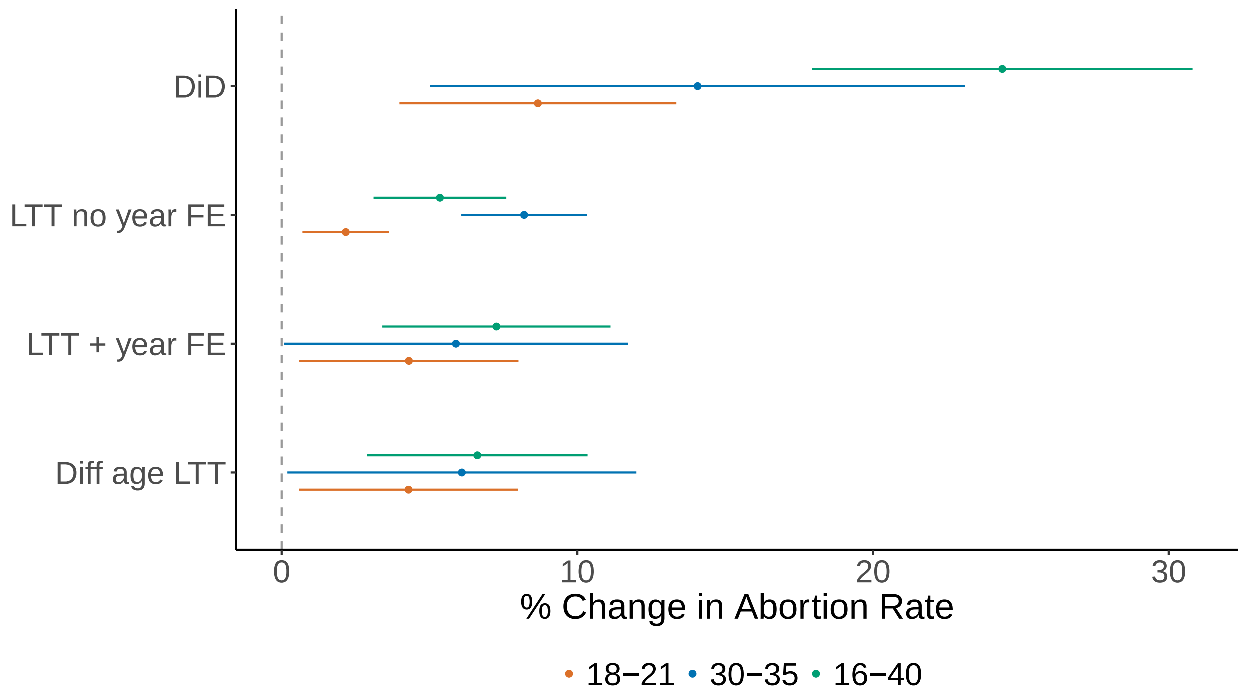
(a) All Unmarried Women



(b) Split by SES

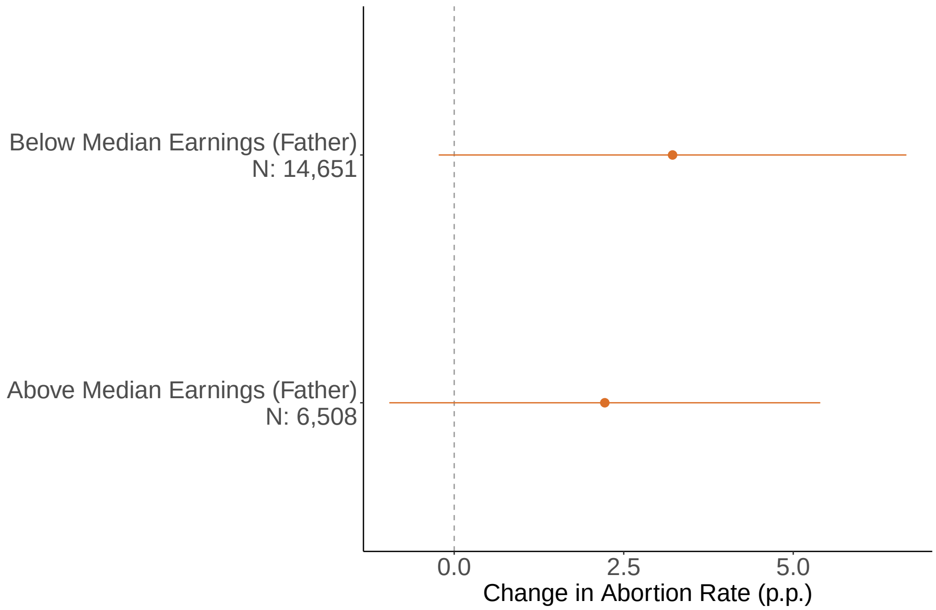
Notes: estimates for unmarried women. Source: Calcalist, 2014

Figure A2: Effect of the 2014 Policy on Abortion Rates

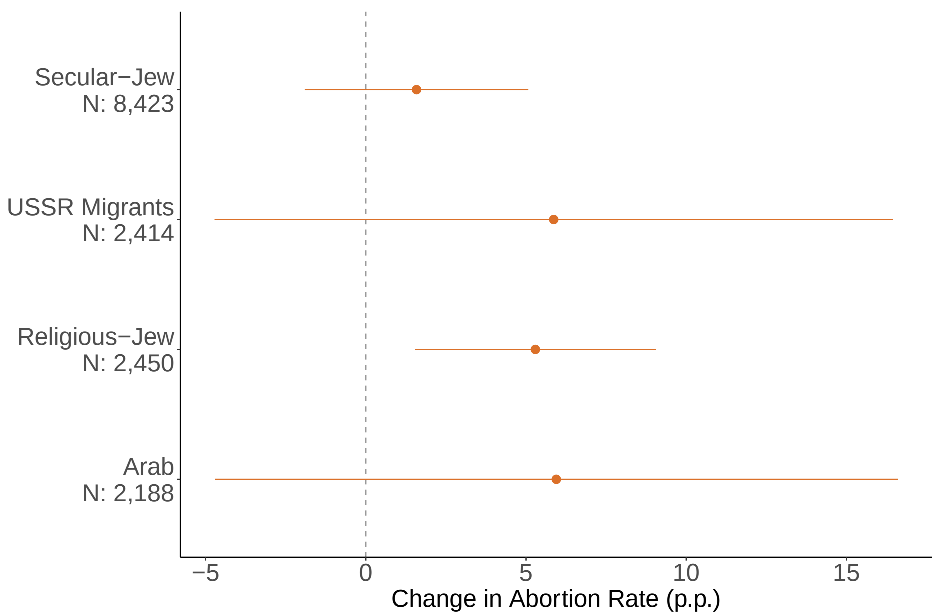


Notes: This figure presents difference-in-difference results for the effect of the 2014 policy on abortion rates. Each row presents the results from a different specification. In each row, the dot represents the treatment effect ($\delta \cdot Post_t \times T_{it}$) and the lines mark the 95% confidence interval around the point estimate. The dashed vertical line is at 0, indicating an insignificant result. The effect is estimated separately in three samples based on womens' age groups: 16-40 (green), 18-21 (orange) and 30-35 (blue). The estimates are divided by the mean abortion rate of each group prior to the policy change and represent the percentage change relative to the baseline mean.

Figure A3: Effect of the Policy by SES and Ethnicity



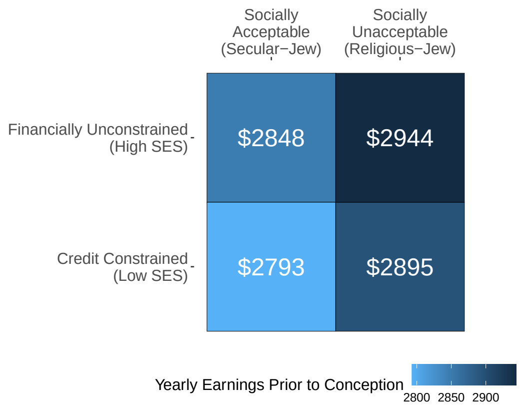
(a) Stronger Effect for Women From Low SES Households(?)



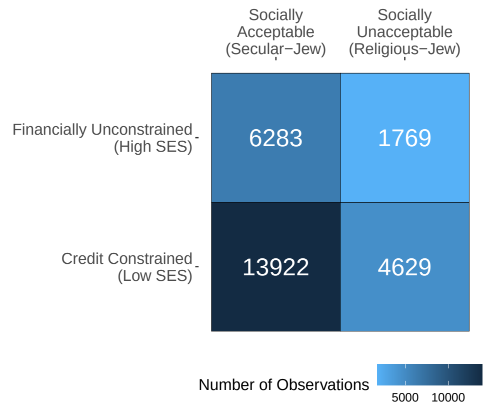
(b) Stronger Effect for Women From More Religious Background

Notes: The figure presents the heterogeneous difference-in-difference results, where we split the sample by population groups. Panel (a) is split by yearly earnings of the father of the woman who concieved (our proxy for household SES level); whereas Panel (b) is split by ethnic groups. In each row, the dot represents the percentage change of the treatment effect ($\delta \cdot Post_t \times T_i$), and the lines mark the 95% confidence interval around the point estimate. The dashed vertical line is at 0, indicating an insignificant result. The sample includes all unmarried women in the country aged 18-21 from 2009-2016 who conceived. Treated women are those aged 20-21.

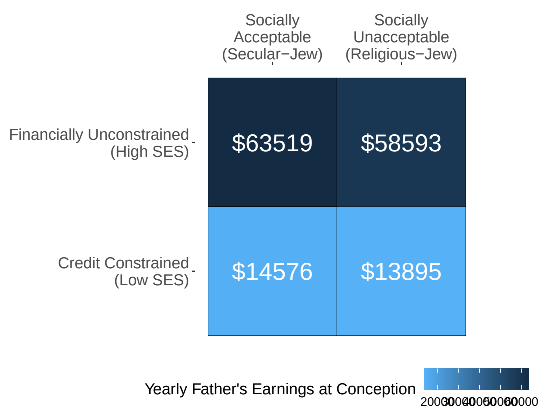
Figure A4: Baseline Yearly Earning by Religious Level and SES



(a) Woman's Earnings



(b) Number of Observations



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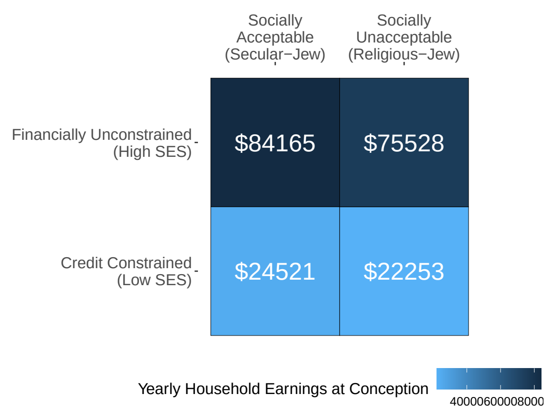

(d) Household's Earnings

Notes: This figure presents the baseline yearly earning while splitting the population across two dimensions: religiosity level and SES background (based on parental earnings). Panel (a) presents the mean yearly earnings of the woman who conceived, one year prior to conception; Panel (b) presents the number of observations in each cell; Panel (c) presents the mean yearly earnings of the woman's father at the year of conception; Panel (d) presents the mean yearly earnings of the woman's household (sum of her parents earnings) at the year of conception. Darker blue correspond to higher levels.

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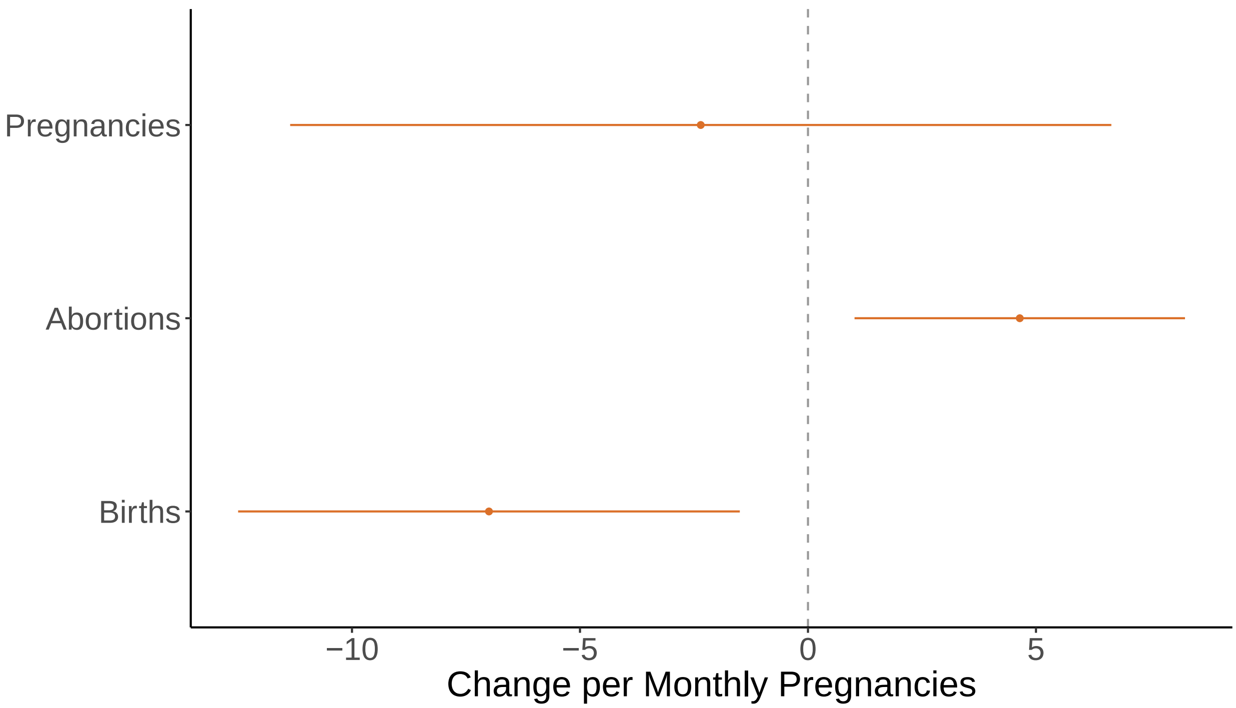
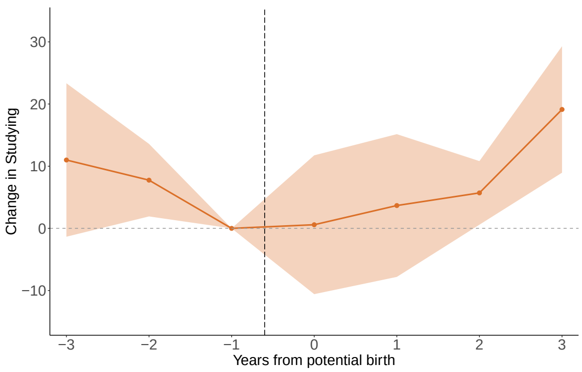
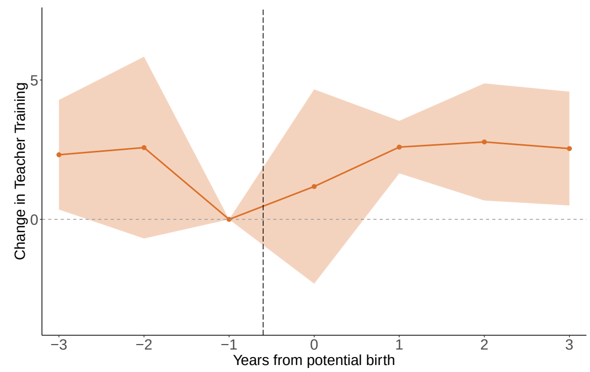


Notes: This figure presents difference-in-difference results for the effect of the 2014 policy on abortion vs birth levels. Each row presents the results from a different specification. In each row, the dot represents the treatment effect ($\delta \cdot Post_t \times T_i$) and the lines mark the 95% confidence interval around the point estimate. The dashed vertical line is at 0, indicating an insignificant result. The sample includes all unmarried women in the country aged 18-21 from 2009-2016. Treated women are those aged 20-21. The estimates are percentage point changes that can be interpreted as the relative change per 100 pregnancies.

Figure A6: Abortion Access Increased Human Capital Investment



(a) Studying - Event-Study x DiD



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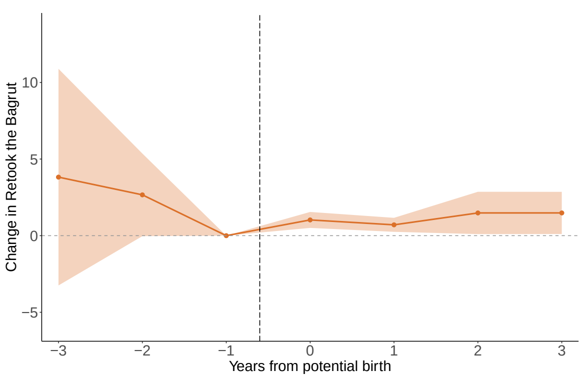

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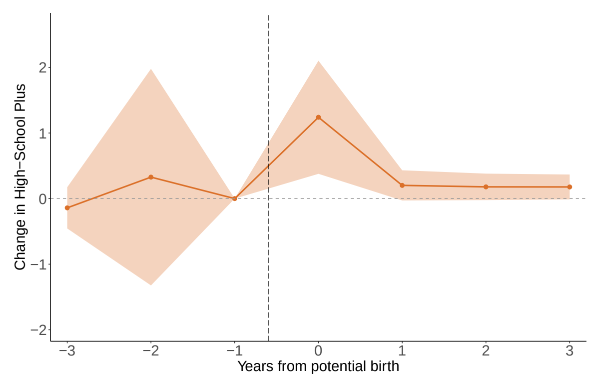
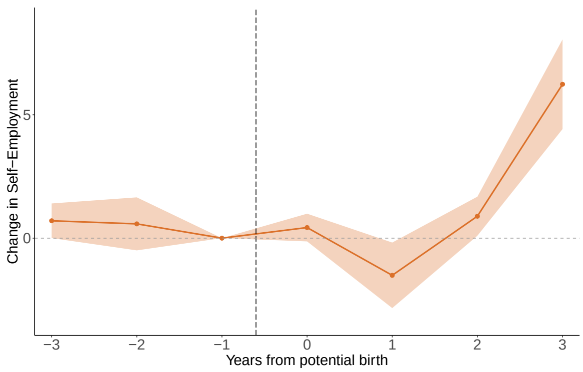


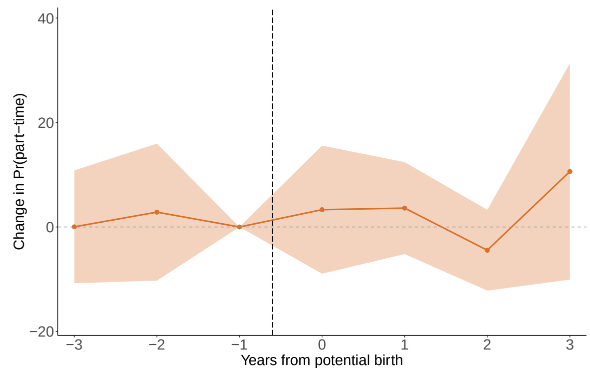
(d) Non-academic Training - Event-Study x DiD

Notes: This Figure presents the results on four sets of outcomes: Panels (a) present the results on the probability a woman will be be enrolled in either: teacher training, take the matriculation exam, academic or non academic training; Panels (b) present the results on the probability a woman will be enrolled in teacher training; Panels (c) present the results on the probability a woman will take the matriculation exam; Panels (d) present the results on the probability a women will be enrolled in non-academic training. Each panel presents results for the reduced form effect of the 2014 policy relative to year of potential birth as described in Equation 4. Each orange circle represents the treatment effect for the reduced form estimated, from three years prior until three years post potential birth timing, relative to one year prior to potential birth (the dropped category). The shaded region mark the 95% confidence interval around the point estimate. The dashed horizontal line is at 0, indicating an insignificant result. The sample consist of unmarried women 18-21 years old who conceived between 2009-2016, and are financially and socially constrained (see Section 5.2).

Figure A7: Abortion Access Effect on Employment and Earnings



(a) Self Employment - Event-Study x DiD



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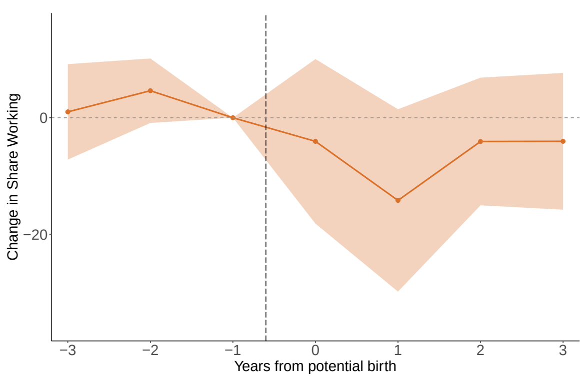

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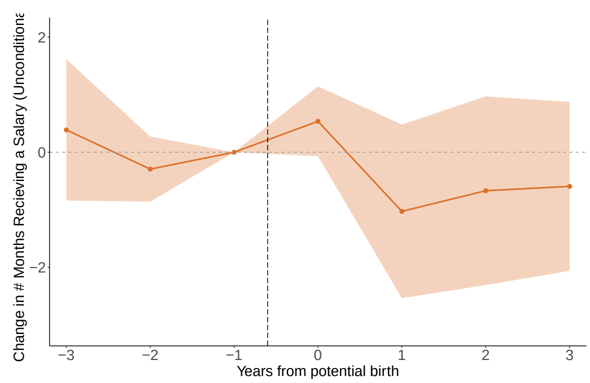
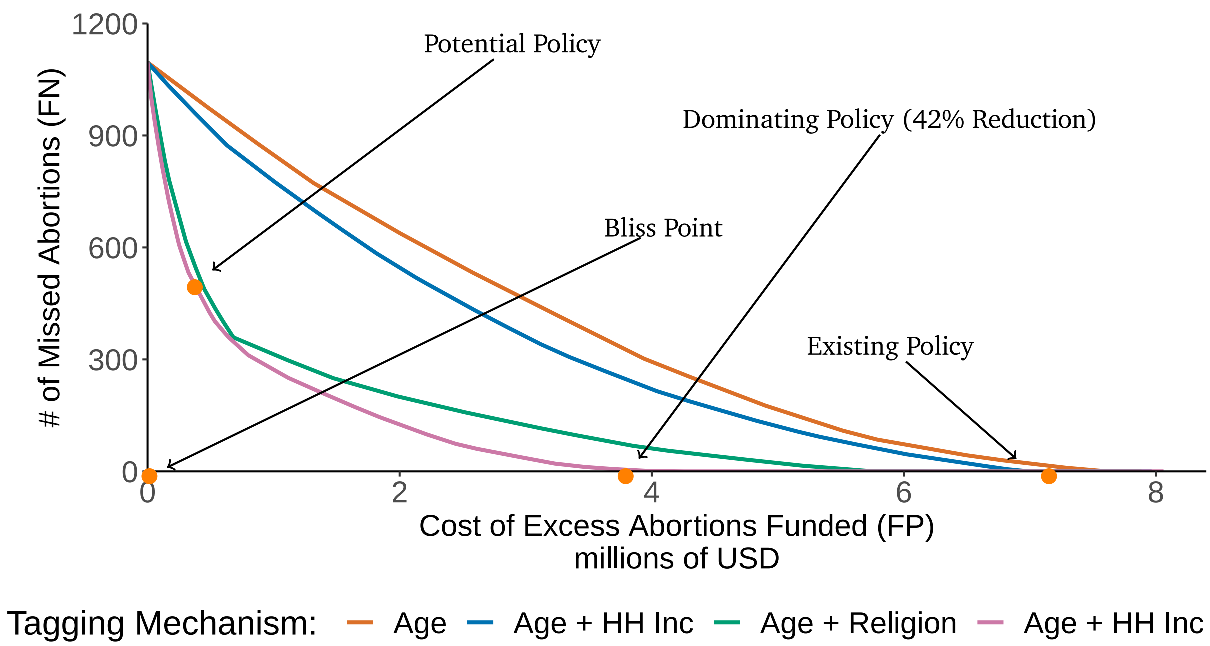


(d) Total Months Worked a Year - Event-Study x DiD

Notes: This Figure presents the results on four sets of outcomes: Panels (a) present the results on the probability a woman will be self-employed; Panels (b) present the results on the probability a woman will be employed part-time; Panels (c) present the results on the probability a woman will be employed; Panels (d) present the results on the total number of months worked a year. Each panel presents results for the reduced form effect of the 2014 policy relative to year of potential birth as described in Equation 4. Each orange circle represents the treatment effect for the reduced form estimated, from three years prior until three years post potential birth timing, relative to one year prior to potential birth (the dropped category). The shaded region mark the 95% confidence interval around the point estimate. The dashed horizontal line is at 0, indicating an insignificant result. The sample consist of unmarried women 18-21 years old who conceived between 2009-2016, and are financially and socially constrained (see Section 5.2).

Figure A8: # Abortions Missed vs. Excess Cost by Different Policies (All Variations)



Notes: This figure presents a framework to assess how efficient a given funding scheme is in targeting financially constrained women and to compare across different policies. It presents the efficient frontier analogus to Figure IXb with added exercises using alternative tagging device. The blue, green, and purple lines corresponds to similar exercises where we use both the women's age coupled with: parental earnings, religious background, and both. The y-axis represents the number of abortions we 'missed' (i.e., false negatives). We scaled the x-axis by the cost of each abortion (600$) to convey the cost of "excess" abortions (FP) funded in millions of dollars.

Table A1: Eligibility for Abortions and Subsidies Pre-2014

Eligibility for Abortion	**% of Approved Applications**	**Subsidized**
Out of Marriage	50.3%	X
or illegal Act		✓
Risk for Woman	40.6%	✓
or Fetus		✓
Age $<$ 18 or	9%	✓
Age $\geq$ 40		X

Notes: This table shows both the eligibility criteria (column 1) to obtain a legal abortion in Israel and the proportion of applications that are approved by the committee for each criteria (column 2). In the third column, we show the eligibility criteria for a subsidized abortion pre-2014.

Table A2: Selection Into Abortion

	Abortion			Birth			Difference	P-value	Full Sample		
	Mean	SD	N	Mean	SD	N			Mean	SD	N
Age	28.0	6.7	241,625	28.5	5.3	2,276,942	0.5	0.000	28.4	5.5	2,518,567
Married	0.41	0.49	241,625	0.90	0.30	2,276,942	0.49	0.000	0.86	0.35	2,518,567
Total Kids	1.1	1.2	241,625	1.3	1.1	2,276,942	0.2	0.000	1.3	1.1	2,518,567
Months Worked Pre	5.0	4.5	241,625	5.4	4.7	2,276,942	0.4	0.000	5.4	4.7	2,518,567
Yearly Earnings Pre	32,327	49,502	241,625	37,369	52,356	2,276,942	5,042	0.000	36,885	52,110	2,518,567
Monthly Earnings Pre	3,513	4,872	241,625	3,904	5,381	2,276,942	391	0.000	3,867	5,336	2,518,567
USSR Descendent	0.23	0.42	241,625	0.10	0.30	2,276,942	-0.13	0.000	0.11	0.32	2,518,567
Religious-Jew	0.07	0.26	241,625	0.28	0.45	2,276,942	0.21	0.000	0.26	0.44	2,518,567
Arab	0.10	0.30	241,625	0.22	0.41	2,276,942	0.11	0.000	0.21	0.40	2,518,567
Years of Education	12.0	2.9	241,625	12.4	2.9	2,276,942	0.5	0.000	12.4	2.9	2,518,567
less than HS	0.72	0.45	241,625	0.63	0.48	2,276,942	-0.08	0.000	0.64	0.48	2,518,567
HS + Vocational	0.10	0.31	241,625	0.10	0.30	2,276,942	-0.01	0.000	0.10	0.30	2,518,567
BA+	0.18	0.38	241,625	0.27	0.44	2,276,942	0.09	0.000	0.26	0.44	2,518,567
High-earnings	0.35	0.48	241,625	0.40	0.49	2,276,942	0.06	0.000	0.40	0.49	2,518,567
Low-earnings	0.04	0.20	241,625	0.04	0.18	2,276,942	-0.00	0.000	0.04	0.19	2,518,567
Nonemployed	0.36	0.48	241,625	0.35	0.48	2,276,942	-0.01	0.000	0.36	0.48	2,518,567

Notes: This table presents summary statistics of key variables separately for the samples of women who gave birth and aborted, as well as the overall population. The first three columns present the mean, standard deviation, and sample size of characteristics for women who had an abortion. The next three columns present the same for women who gave birth. Next, we report the difference in means and the p-value for a t-test of the difference. Finally, in the last three columns we report the mean, standard deviation, and sample size for the full sample of women.

Table A3: Pre-policy characteristics by subsidy eligibility

	Not Treated		Treated		ND	P-Value
	Mean	SD	Mean	SD		
Secular-Jew	0.40	0.49	0.41	0.49	-0.03	0.03
USSR Descendent	0.14	0.35	0.16	0.36	-0.04	0.00
Religious-Jew	0.09	0.29	0.09	0.29	0.00	0.83
Arab	0.06	0.24	0.07	0.25	-0.02	0.15
Enrolled in College	0.01	0.02	0.04	0.05	-0.92	0.00
N. Children	0.001	0.003	0.001	0.007	-0.088	0.000
Months Worked a Year	5.8	0.6	6.9	0.9	-1.3	0.0
Monthly Wages	1,881	330	2,628	456	-1.88	0.00
N	12,303		13,756			

Notes: This table presents summary statistics of key variables separately by treatment status for the sample of unmarried women aged 18-21. Treatment is defined as the women eligible for the subsidy after the 2014 policy change (20-21 year olds). The first two columns present the mean and standard deviation for the ineligible women. The next two columns present the same for women who were eligible. Next, we report the normalized difference in each variable by treatment status and the p-value for the difference in means.

Table A4: Constructing efficiency frontier

(a) Calculating Compliers and Always Takers (AT) by Age

Age	Compliers	AT	Compliers/AT
20	113	1,116	0.10
21	42	1,179	0.04
22	118	1,150	0.10
23	135	1,146	0.12
24	77	1,004	0.08
25	105	958	0.11
26	122	804	0.15
27	106	738	0.14
28	96	652	0.15
29	48	600	0.08
30	10	540	0.02
31	13	471	0.03
32	24	451	0.05
33	0	447	0.00
34	0	434	0.00

(b) Re-ordering by Ratio between Compliers and AT

Age	FN	FP	Cumul. Compliers	Cumul. AT	Compliers/AT
No Funding	1,096	0	0	0	
26	974	804	122	804	0.15
28	878	1,456	96	652	0.15
27	773	2,195	106	738	0.14
23	638	3,341	135	1,146	0.12
25	533	4,299	105	958	0.11
22	415	5,449	118	1,150	0.10
20	302	6,565	113	1,116	0.10
29	254	7,165	48	600	0.08
24	176	8,169	77	1,004	0.08
19	109	9,195	67	1,026	0.07
32	85	9,646	24	451	0.05
21	43	10,826	42	1,179	0.04
31	30	11,297	13	471	0.03
18	10	12,151	20	854	0.02
30	0	12,691	10	540	0.02

Notes: This table demonstrates the calculations used to construct the efficiency frontier. Panel (a) calculates the always takers (AT) and compliers under the existing age-based policy. We calculate these for each age group that was affected by the policy (except 18 and 19 year olds as they were previously funded before the 2014 policy change). For example, when 20 year olds are funded, there are 113 compliers (women who are able to have the abortion *only* because of the funding) and 1,116 always takers (women who would have had the abortion regardless of the funding). Panel (b) re-orders the age groups according to the ratio between compliers and always takers, which is used to trace out the efficiency frontier. For example, we can see that funding 26 year olds is the most efficient, according to this criteria. By moving from a "no funding" scenario to funding only 26 year olds, the number of false negatives is reduced by 122, which corresponds to the number of compliers in that age group.

B Context

B.1 Committee Approval Process and Motivation

The committee process for obtaining an abortion was originally motivated by medical concerns that abortion could affect a woman's future fertility (Amir, 2015). The concern for women's fertility is aligned with Israel's demographic project, which aims to reverse the decrease in the worldwide Jewish population as a result of the Second World War.[1] For this purpose, Israel has an aggressive, pro-natalist policies such as subsidized daycare, monthly child allowances, tax deduction, paid parental leave, national health insurance (including infertility treatment, and freezing eggs). Similarly, contraception is not covered by the national health insurance, and abortions are illegal without prior approval from the committee. As a result, Israel's birth rate is the highest among the developed world.

Israel has a national health-care system; however, people may choose to seek care from private providers. If a woman decides to have an abortion through a private doctor, she is still required by law to go through the committee process for approval. A total of 42 committees are scattered around the country. Each hospital has its committee and the rest operate in smaller clinics (either private or public).[2] Typically, once a woman becomes pregnant and would like an abortion, her doctor will direct her to make an appointment with a committee (usually up to a week).

The committee's social workers serve as the effective gatekeepers to the approval process, while the committee itself serves as a rubber stamp (Oberman, 2020). Upon arrival at the committee, the woman fills out the necessary paperwork and pays the committee fees.[3] Then, she meets the social worker to discuss her decision and assess her eligibility per the criteria. As mentioned above, only one group of women are ineligible for approval - married women between 18 and 40 who have healthy pregnancies. In cases when a woman is ineligible based on the criteria but desires an abortion, the social worker will often help her navigate the system to meet the criteria. Most commonly, the social workers will refer these women to a psychiatrist who can assert that the woman is not adjusting to the pregnancy, which will enable her to obtain approval under the criteria for protecting women's health (Oberman, 2020). [4]

Although the committee process may seem obstructive, given this process effectively, all applications are approved (unsurprisingly). Our data shows that 99% of applications were approved, and 97% are acted upon. The application and committee process is completely confidential for the woman, and neither parental nor partner consent is required. Women past the 24th week of pregnancy (1% of abortions) are referred to a select committee that reviews the request and has stricter standards for approvals (though approval rates are as high as the standard committee), which we exclude from this analysis. Oberman (2020) attributes the high approval rates to the social workers who direct women who would otherwise not be approved to a psychiatrist for sign-off under the women's health criteria.

The approval criteria were motivated by Jewish law (Amir, 2015). Judaism holds relatively liberal views with respect to abortion compared to Islam and Christianity (19% and 2% of the population, respectively). Jewish law emphasizes the mother's life and health, whereas the fetus is part of the mother and does not have its rights before it is born. Based on the religion, a child outside of wedlock is illegitimate and is doomed to bad life outcomes (e.g., cannot get married by Jewish tradition), and should therefore be avoided.

B.2 Illegal Abortion Market

Israeli law puts the liability of performing an illegal abortion on the doctor rather than the woman. Though, in practice, this is rarely ever prosecuted (Amir, 2015). Given the high accessibility, quality, and low, or in some cases free, cost, incentives are low to do illegal abortion in Israel. Why would women want to opt for an illegal procedure over the committee process? There are four suggested reasons: no wait time, no 'guilt' involved with sitting in front of a committee, have a doctor you know, and trust, and the legal liability is on the doctor (which is never enforced) (Oberman, 2020).

Anecdotal evidence argues illegal abortion does exist in Israel but is dominated by 'high-end' (and high cost) providers operating outside the committee process, rather than the unsafe conditions that are more characteristic

[1] "increasing the Jewish birth rate is in dire need..." David Ben-Gurion, Israel's first Prime-minister.

[2] See the full list here

[3] The committee fee is 400 NIS (or $155), these are also funded by the new policy.

[4] Alternatively, some anecdotal evidence suggests women can report taking certain medications before getting pregnant, which puts the fetus at risk.

of illegal abortion in other settings (Newman, 2017; Oberman, 2020). An article in the Israeli newspaper Seven-Days ('Shivaa Yamim') suggested that there are 15,000 illegal abortion a year in Israel (Newman, 2017). However, contacting the reporter and organization quoted in the article, we found no evidence for the source of the survey, nor an individual that was willing to take responsibility for it.

B.3 Abortion Norms and Rates Among Populations of Interest

Abortion is not uncommon in Israel, despite the existence of the committee, as shown in Figure B2. The share of legal abortions out of pregnancies has remained relatively constant between 2002-2016, averaging approximately 8% of pregnancies each year. While this may sound high, Israel's legal abortion rate is actually relatively low compared to global rates and other high income countries (see Figure B1). Twenty-five percent of pregnancies are aborted worldwide, while in Europe the rate is 30% and in the U.S. the rate is 17% (Institute, 2018).

Abortion is not a politically charged topic in the Israeli parliament (*Knesset*) relative to other settings, such as the United States, and is considered to be a "silenced phenomena" (Amir, 2015). While the liberal parties apposes the existence of the abortion committee, they know that opening the topic for discussion might result in a more restrictive abortion laws (Oberman, 2020; Rimaltt, 2017). On two rare occasions that a challenging bill was introduced it did not pass.[5] On the other side of the debate, the religious parties (both Orthodox and Arab parties) tried twice to challenge the status-quo of the current abortion law but could not gather the necessary political support.[6]

To illustrate the difference in abortion discussion in the public sphere between Israel and the U.S., and the saliency of the policy we conduct a Google trends analysis that suggests the policy took time to arrive to the public discussion and even when it did, it was much less salient as compared to other abortion discussions in the U.S. Figure B3 presents the Google searches for the word "abortion" in the U.S. compared to it's Hebrew equivalent ("hapala") in Israel from 2009-2019 (normalizing base levels of both countries in January 2009). We can see the peak in Israel across time is indeed in 2014, but only a few months into the year (while the policy was in effect in January already). Having the intensity of the American abortion discussion, one can imagine that this extensive coverage will create an out of the ordinary discussion. However, we can see the surge in searches of the word "Abortion" responds much more aggressively in the U.S. even without a change of the law, but just when President Trump was elected, or when Brett Kavanaugh was nominated to the supreme court.

Israel is an interesting setting to study the effects of abortion access due to the vast heterogeneity in cultural views toward abortion, ranging from: ex-USSR immigrants and secular Jews, religious and ultra-Orthodox Jews, and Christian and Muslim Israeli-Arab.[7] Figure Ia presents the wide heterogeneity in baseline abortion rates which might suggest different latent costs of abortion (or differing abortion views) across groups. Understanding these heterogeneous views is key to understanding our results and our proposed mechanism - social-stigma cost, that will be discussed in Section 5.2.

Compared to Islam and Christianity, Judaism holds relatively liberal views with respect to abortions and accepts abortions in two broad cases: threat to the woman's life and if the fetus will be born into an "unstable life".[8] These two conditions are the motivation for the abortion committee criteria discussed above (Amir, 2015). Judaism place a supreme value on mother life and health, while the fetus is part of the mother and doesn't have it's own rights before it is born. Nevertheless, Israel's (Jewish) "demographic project" strives to limit abortions among the Jewish population (motivating the abortion committee), which are considered anti-Zionist. The Jewish population consists of a wide mixture of religiosity levels, ranging from secular Jews (45%), traditional Jews (25%), religious Jews (16%) and Orthodox Jews (14%) (Central Beurue of Statistics (Israel), 2018). Broadly speaking, religiosity level is highly correlated with both fertility levels and opposition to abortion; the secular-Jewish population generally supports abortion and has relatively low fertility rates, while the other end of the

[5] At November 29th 2004, Reshef Hen ('Shinoi' party) submitted a legislative application for adding approval based on SES status. At 2006 Zehava Galon ('Meretz' party) propose to reconsider the committee practice altogether. Both bills did not pass.

[6] In 2008, Nissim Zeev ('Shas' party) proposed to make late-term abortions illegal. The bill failed not pass. In 2013 the two Chief Rabbis of Israel issued a letter in support of Efrat, an anti-abortion group that was established in the 1960's. In January 2017 Yehudah Glick (Likud party) and Abd al-Hakim Hajj Yahya (Joint List party) called a *Knesset* Committee on the Status of Women and Gender Equality meeting to propose incorporating religious representative in the abortion committee, the law failed to pass arguing that a religious entity in the committee will discourage women (especially in the Arab population) to apply to the committee, fearing information leaking to their communities.

[7] Israel is composed of 75% Jews, 18.6% Arab-Muslims, 2% Arab-Christians and 4.4% affiliated with other religious groups (or non-affiliated).

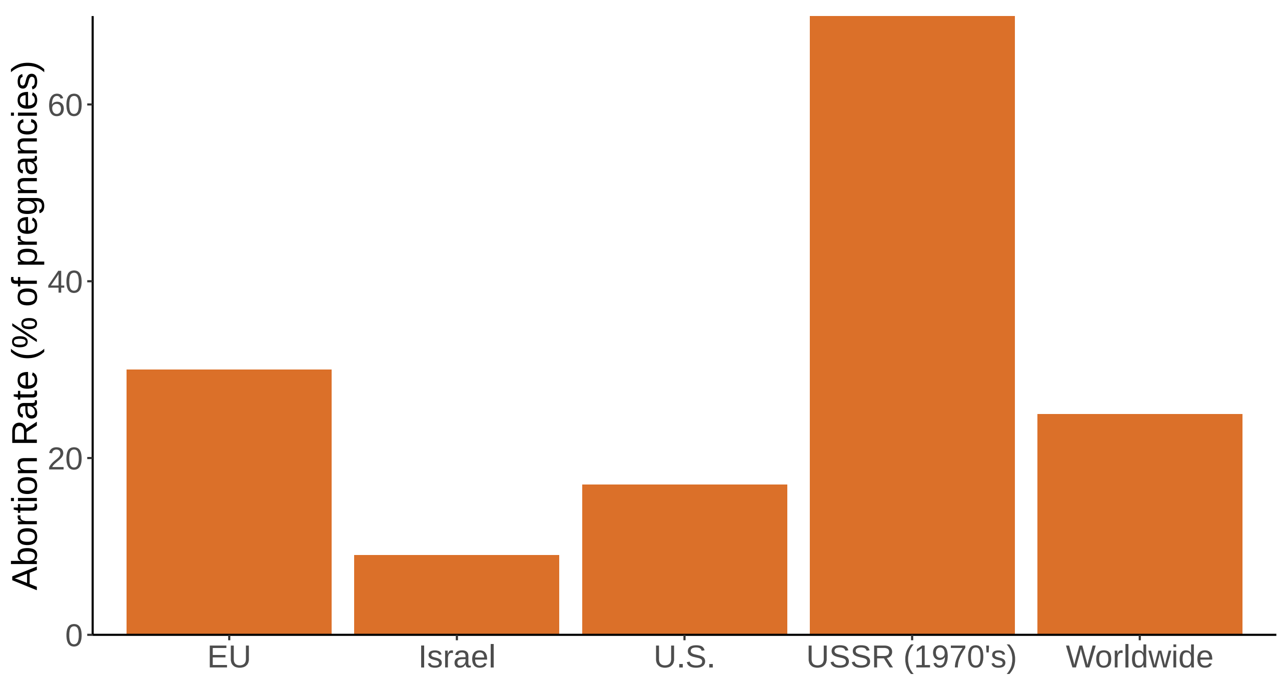
[8] There is no strict definition of an "unstable life", but characteristics of an unstable life may include cases such as unmarried parents, an extremely old or young mother, or being born with congenital disorder.

religiosity spectrum the Orthodox population is opposed to abortion and has very high fertility rates.[9]

The former-USSR immigrants are a unique population for studying the impacts of the 2014 policy due to their relatively lenient abortion views. The immigrants from former-USSR countries are both Jewish and predominantly secular (Leshem, 2009), and constitute a substantial share of the Israeli population. Over one million people immigrated to Israel in the decade following the collapse of the USSR, and make up 15% of the total population in Israel (Leshem, 2009). There are also important cultural differences in attitudes towards abortion among immigrants from former-USSR countries, stemming from the USSR's unique history with respect to abortion and contraception. The USSR was the first country in the world to completely legalize abortion in 1920, which was also the main method of contraception used in the USSR due to the lack of modern contraceptives. During the 1970s, four out of five pregnancies were aborted, giving the USSR the highest abortion rate in the world (Avdeev et al., 1995).

The Israeli-Arab population is mostly religious and considers abortion very taboo. The Muslim population consists of 11% secular, 57% traditional and 31% religious Muslims (Central Beurue of Statistics (Israel), 2018). In general, Islam opposes abortions other than in cases of a health risk for the child. Given the opposition to abortion, there may be a greater incentive for women in the Arab-Muslim community in Israel to turn to the illegal market or even self-induce abortion. The extent of self-induced and illegal abortions is extremely difficult to estimate, but a study of the Palestinian population may help contextualize the frequency with which this occurs among the Arab-Muslim population within Israel.[10] A 2006 survey conducted by Bethlehem University found that 10% of Palestinian-women self induced abortions and a quarter of the women stated it was necessary for unmarried women in order to prevent "honor killings" (Foster et al., 2007).[11]

Figure B1: Abortion Rate Worldwide



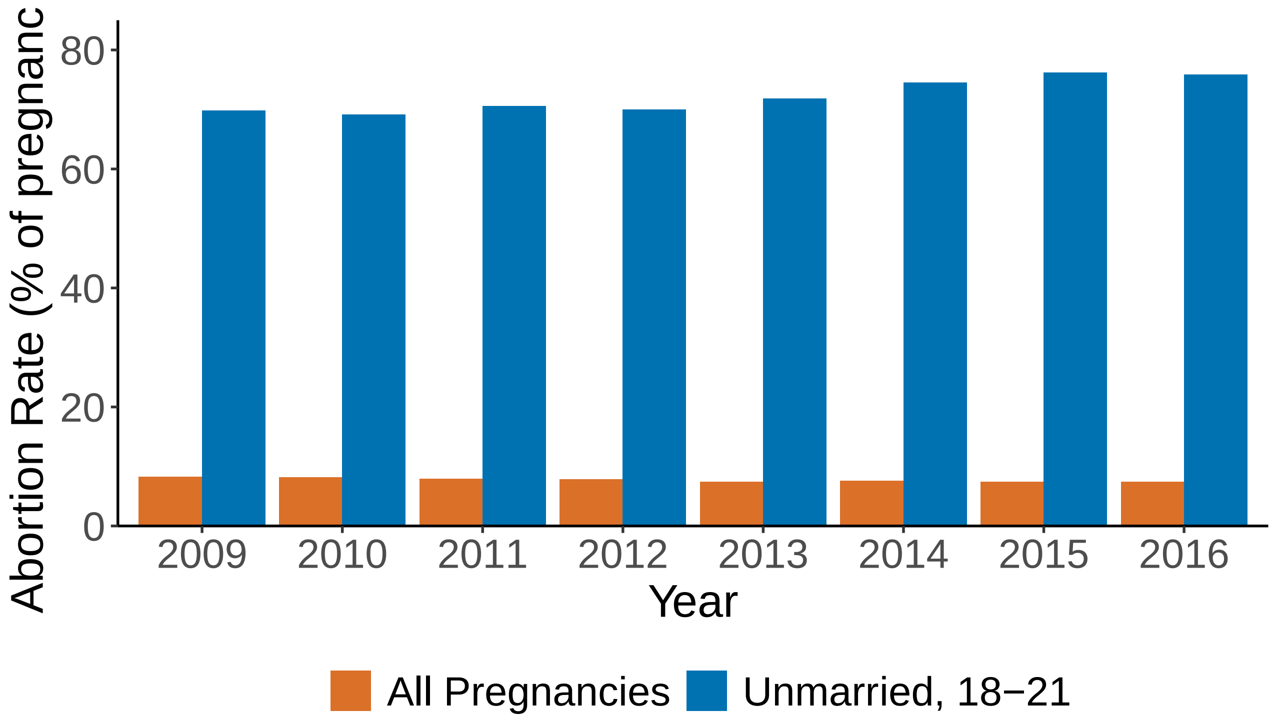
Notes: This figure presents the share of abortions per pregnancies in Israel, Europe, U.S., former USSR, and Worldwide. Source: Institute (2018).

[9]There is substantial heterogeneity in religiosity level in Israel. The religious population tends to marry and have children at a very young age (late teens to early 20s). For example, among 18-21 year-olds, 79% of the women who conceived are married. The abortion rate (out of pregnancies) among 18-21 unmarried women is 67%, while it is essentially 0% for the married population in this age group. Thus, it is safe to assume that most pregnancies among the married population in this age group are planned, while the converse is true among unmarried populations.

[10]The Israeli-Arab community commonly identifies with the Palestinian population (Tamar-Sheperman, 2008).

[11]The practice of killing women by other family members when the women have brought dishonor to the family, for example by having an abortion or having premarital or extramarital sex.

Figure B2: Abortion in Israel Over time



Notes: This figure presents the share of abortions per pregnancies in Israel per year for each year between 2009-2016. The bars in orange presents the rates for the entire population of pregnant women, while the bars in blue restricts to our population of interest - unmarried women, 18-21 years old.

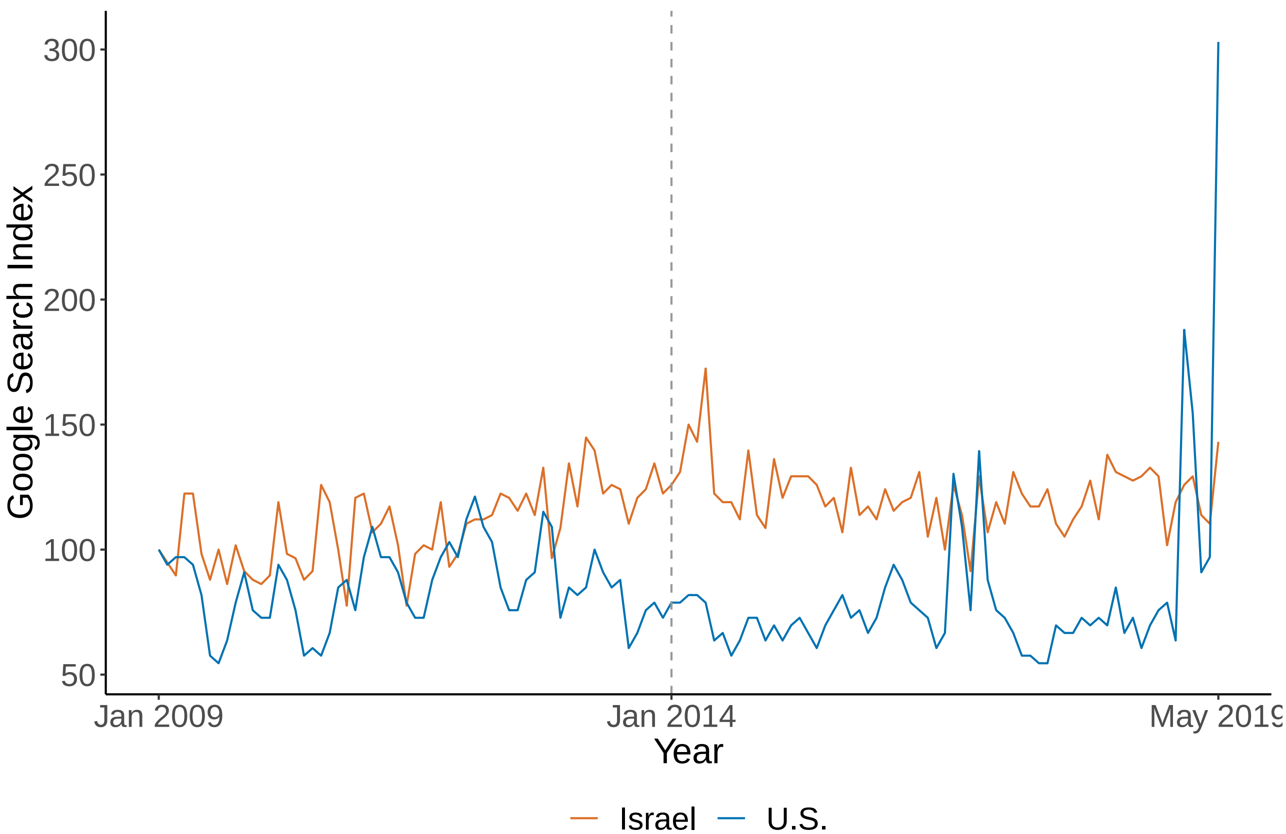
B.4 Child Rearing Cost

In order to understand the decision of a woman whether to abort as a result of abortion subsidy, one should understand first the cost of raising a child in Israel and it's significant difference relative to the cost in the U.S. First, both education and health-care are free in Israel and widely used.[12] Second, as part of the demographic agenda of the country there are several income transfers tools: birth-transfers ($145 - $484 for birth), tax-breaks ($118 a month per child per working parent) and social-security transfers ($41-$52 a month per child). Finally, one should consider the opportunity cost of having a child - the woman's displacement out of the labor force. By the Israeli law, the government covers three months of paid maternity-leave with an option to extend three months unpaid, during which employers cannot fire the parent (only one of the parents can choose to take the leave, though it is commonly the mother). In some cases the parent may ask for an unpaid extension of the maternity leave, but the employer is no longer obligated by law to do so.

One important feature of Israel - it's size - allows a smoother return to the labor force due to support of the family members in those months where the child is still out of the education system (typically until age one years old). Still, child-rearing costs is a substantive consideration when taking a decision to abort. Indeed, comparing abortion rates among women of different income groups, we can see that rates are pretty similar among high and low earners, but for unemployed women are nearly four times higher (Figure Ic).

[12]Health-care coverage per child is merely $3 a month. Pre Kinder-garden is covered from age 3 onwards. Below the age of 3 free options exist, yet even the paid options are less than the minimum full-time monthly earnings.

Figure B3: Google Search for the Word "Abortion" (Israel vs. U.S.)



Notes: This figure presents the Google searches for the word "abortion" in the U.S. compared to it's Hebrew equivalent ("hapala") in Israel from 2009-2019 (normalizing base levels of both countries in January 2009).

C Data

C.1 Data Structure

We first organize all conceptions into a repeated cross-section based on the time (month-year) at conception (henceforth the "conception cross-section"), which is our primary analytic dataset to evaluate the policy's effect on abortion and the underlying mechanisms.[13] For example, if a woman applied to the abortion committee in July 2012 when she was eight weeks pregnant, her month-year observation is pushed back eight weeks to May 2012, the time of conception. We also use year-month information on the woman's date of birth to calculate her age at conception, which we use to identify the subsidy's eligibility. We make comparable adjustments for pregnancies that result in live births (using the child's gestational age at birth). We trim the dataset to include women aged 16-40, as pregnancies for women younger than 16 and older than 40 are both rare and extreme cases that do not generalize well to the rest of the population. Table II shows the evolution of our sample size, in terms of total observations (i.e., pregnancies) and total women, as we narrow down from the universe of pregnancies in Israel to the analytic population of interest described here.

We then link the conception cross-section to the tax records to get a yearly-level employment and earnings panel for all women who conceived within our sample period (henceforth the "labor-market panel"), this panel is used to estimate the effects of the policy on labor-market outcomes. We take all pregnancies conceived between January 2002 - March 2016 and link them to the woman's employment and earnings data from the tax registry. We link approximately 74% of our sample, since the tax registry includes the universe of the labor force we assume the unlinked women are out of the labor force (henceforth non-employed). While we acknowledge this is an underestimate (due to informal jobs), their earnings are unlikely to be substantial. Moreover, informal workers do not have access to social benefits, such as vacations, parental leave, retirement and unemployment insurance. Hence, we believe categorizing them as non-employed is a reasonable approximation.

Table A2 presents summary statistics for key variables in our analysis, separately for the population of women who aborted and gave birth and the overall sample. We also run basic t-tests comparing the means of the two populations, which highlights how different they are, implying selection into aborting on observables. Women who aborted are younger, less likely to be married, have fewer children, have completed fewer years of education, and earn less (both on a monthly and yearly level) than women who gave birth. All of these differences are highly statistically significant. In fact, the only characteristic in which women who aborted are not statistically distinguishable from those who gave birth is the proportion of women whose highest degree earned is a high school degree or vocational training (12% of both samples).

Lastly, we construct a *balanced* panel of all women of reproductive age in the country with the timing of their conception and the outcome of that conception (henceforth the "women panel"), this panel will be used for assessing moral hazard in response to the policy (see Section 5.1). We take all Israeli women who are of reproductive age (16-40) in a given year (2002-2016). For each woman we indicate whether or not she conceived that year, and what was the outcome of that pregnancy (abortion or birth). A woman that did not conceive (either ever or not on that given year) will still have a row for that year where the indicator for conception will be zero for that woman-year. We then link this woman-year observation to the demographic information from the census and labor-market information from the tax data.[14]

C.2 Variable Definition

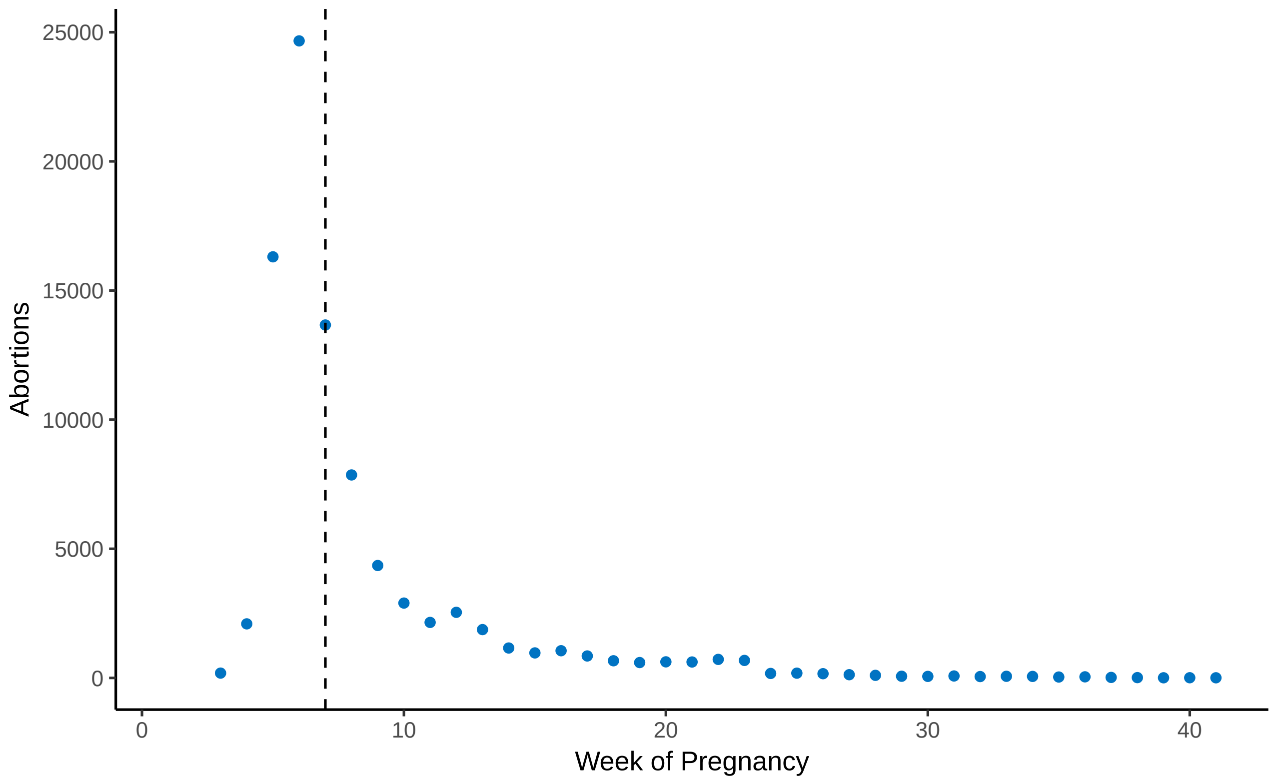
The policy expanding the abortion subsidy went into effect in January 2014. However, women who conceived at the end of 2013 would be eligible for the subsidy if they applied to the committee in 2014 (and met the age requirements). In Israel, most legal abortions occur by the 8th week of pregnancy (Figure C1). Therefore, we move the treatment timing a month back to account for pregnancies that were conceived in December 2013, but may not even have been discovered until January 2014 when the policy was already in place, and thus should be considered treated.

We classify each woman-year observation into three income groups: part-time employment, below median

[13] For example, a woman will appear once if she gave one birth, while she will appear three times if she had one abortion and two births.

[14] We chose not to use the balanced woman-panel for our primary analysis to reduce bias by keeping our sample as homogeneous as possible. Unmarried young women that conceive are more homogeneous group (at least on their contraception decision) then all other young unmarried women who did not conceive.

Figure C1: Distribution of Abortions by Week of Pregnancy



Notes: This figure plots the distribution of abortions by week of pregnancy. The vertical dashed line marks the 7th week of pregnancy, which is the cutoff point for being able to use the medical abortion pill (the common name for using two different medicines to end a pregnancy: mifepristone and misoprostol).

earnings, and above median earnings.[15] Given our focus on young women (see Section 3.2 below), we also construct comparable earnings variables at the household level, combining parental income.

Finally, we classify religion and ethnicity-based on data from the census and the Ministry of Education. Specifically, we define a woman as religiously based on the type of school she attended. Israel has three types of schools: secular ('mamlachti'), religious ('mamlachti-dati'), and Orthodox.

[15]We define part-time employment as earning below the 2011 (baseline year for our inflation correction) minimum full-time monthly earnings defined by law (41% of our sample) – 3,890 NIS a month (USD $1,090) from the Social Security website. The rest of the sample we split into two equal-sized groups and classify them as below and above-median earnings.

D Robustness of Difference-in-differences Strategy

D.1 Parallel Trends

Our difference-in-differences approach requires that women eligible for the subsidy would have experienced similar changes in their abortion rate over time as ineligible women in the absence of the 2014 subsidy. In this section we present several pieces of evidence to support the plausibility of parallel trends for the population of interest (unmarried, 18-21 year olds), as well as comparable plots for two other populations: a 30-35 year olds, where the 33-year-old age cutoff was used to determine treatment, and the full sample of women aged 16-40 with both age cutoffs (19-years-old and 33-years-old) used to define treatment.

Figure II plots the estimates of δ_k from Equation 2. Here we show the comparable plots for the 30-35 year olds (Figure D1) and the 18-40 year olds (Figure D2). For the population of the 30-35 year olds around the 33-year-old cutoff, while the pre-trends are quite parallel there also does not seem to be a policy effect. In contrast, when we use the entire population and both age cutoffs, there is a strong policy effect but also a very clear violation of the parallel trends assumption.

Figure D4 presents the equivalent monthly-level abortion rate for eligible and ineligible unmarried women over time to assess this underlying identifying assumption. The dashed line represents the timing of the 2014 policy change. The blue and orange lines are a linear fitted line. We do this separately for women aged 18-21 (around the 19-year-old cutoff) and women aged 30-35 (33-year-old cutoff) to assess the validity of the difference-in-differences for each cutoff, as well as for the entire population of women.

Figure D4a presents the parallel trends for the 18-21 year old sample. In general, the trend in the abortion rate is quite parallel for the treated (20-21 year olds) and untreated (18-19 year olds) in the pre-period, although perhaps there is a slight narrowing of the difference for the years closer to 2014. In 2014 we observe a sharp increase in the abortion rate for the treated 20-21 year olds, and no real difference for the untreated 18-19 year-olds, as would be expected if the subsidy expansion affected the abortion rate.

Figure D4b presents the comparable parallel trends for the 30-35 year old sample. In this sub-sample the trends are clearly not parallel as the abortion rate for the eligible 30-32 year olds increased over time much more than that of the ineligible 33-35 year olds. While we do observe a noticeable increase in the abortion rate for the treated population after 2014, this is a clear violation of the parallel trends assumption. Thus, for the main analysis we choose to focus on the younger population of women for whom the difference-in-difference is valid, but we include a comparable set of results using the 33-year-old cutoff among the sample of 30-35 year olds in the appendix for completeness.

Lastly, we examine parallel trends for the entire population of women, where women affected by the policy change (ages 20-32) are considered treated and unaffected women (ages 16-19 and 33-40) are considered untreated (Figure D5a). Similar to Figure D4b the trends are clearly not parallel. However, given the vast number of ages we included (16-40) it is reasonable that trends in abortion rates would be very different. Therfore, we re-estimate the parallel trends for the entire population of women after residualizing on a time trend in the pre-2014 period. Figure D5b shows that after correcting for differential pre-trends we still observe a 2 percentage points increase in abortion rate. This serves as a graphical illustration of including linear time trends (LTT) when we will analyze the entire population (or the 30-35 population).

To account for these differential pre-trends, we estimate the DiD with a linear time trend (LTT) interaction:

$$abort_{it} = \delta Post_t \times T_i + \lambda T_i \times y + \gamma_{a_i} + \gamma_{y_t} + \gamma_{m_t} + X'\gamma_i + \epsilon_{it} \tag{1}$$

For comparison and robustness, we also estimate Equation 1 for the 18-21 year-old sample, as well as test variations of this approach with and without year fixed effects[16] and a final variation including age-specific linear time trends.

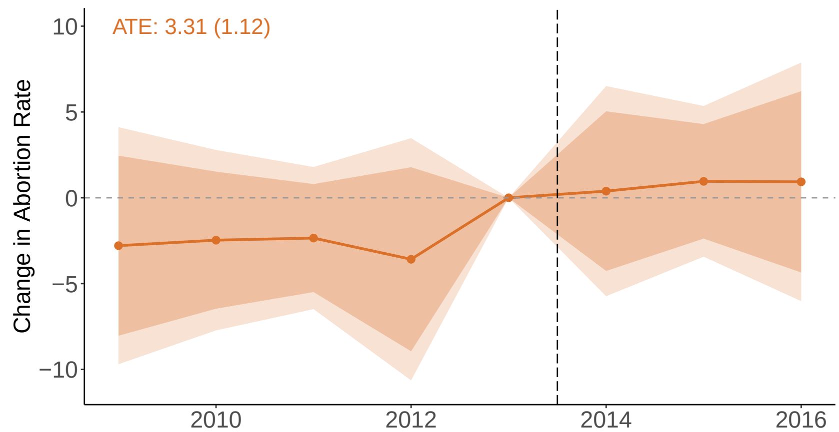
Ultimately, given the pattern of age effects shown in Figure I2b and the parallel trends shown in Figure D4a, we choose to focus on the 19-year-old age cutoff and restrict the sample to unmarried women aged 18-21 for the main analysis,[17] where the difference-in-difference design is most valid. This population is also extremely relevant from a policy perspective. Given the two-year mandatory military service in Israel, women usually start higher education at the ages of 21-23. Thus, 18-21 year-olds are deciding whether or not to obtain higher education and the ability to avoid an unwanted pregnancy at this time could have important impacts on this decision, whereas 30-35 year-olds have largely completed their educational attainment. Additionally, 18-21 year-olds are

[16]Thus allowing for a separate pre-trend for the treated and control groups.

[17]We assess robustness to other age bandwidths around the 19-year-old cutoff, such as 16-23 year-olds, 17-22 year-olds, etc.

less financial independent and the subsidy is likely more valuable for them. Finally, given familial and cultural pressures, 18-21 year-olds may also be more likely to have a "shotgun marriage" in response to an unexpected or unwanted pregnancy, whereas the majority of women in Israel are already married by the time they reach ages 30-35.

Figure D1: Generalized Difference-in-difference (30-35)



Notes: This figure presents the difference in abortion rate between treated (aged 30-32) and control (aged 33-35) women over time (2009-2016). The dashed line indicates the timing of the 2014 policy change. Each dot represents the coefficient δ_k estimated from the generalized difference-in-differences (Equation 2). The shaded regions marks the 90% and 95% confidence interval around the point estimate, respectively.

D.2 Comparing Linear Time Trends Specifications

Another potential risk for identification is that women aged 18-19 were on a different trend then women aged 20-21. To address this concern, we interact the treatment with a linear time trend, to allow for separate pre-trends for the treatment and the control group. The results in Table D1 are lower then our main DiD specification (3.6 vs. 4.6 p.p.) yet still significant and in the same magnitude.

There are several ways one can control for pre-trends. We begin by interacting the treatment with a linear time trend as follows:

$$\text{LTT - Level Shift: } abort_{it} = \delta \cdot Post_t \times T_i + \beta \cdot T_i \times t + \gamma_{a_i} + \gamma_{y_t} + \gamma_{m_t} + X'\gamma_i + \epsilon_{it} \tag{2}$$

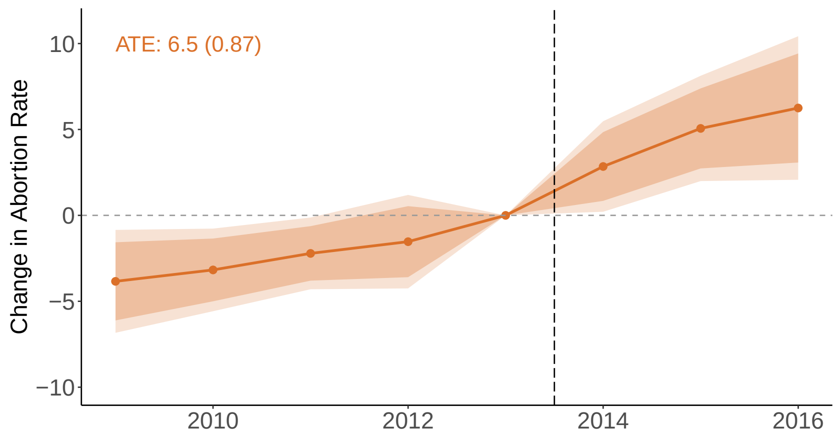
The dependent variable ($abort_{it}$) equals one if woman *i* had an abortion in year *t*. On the right-hand-side, $Post$ is an indicator for the policy being in effect ($\mathbb{1}\{t \geq \text{Dec-2013}\}$) and T_i indicates woman *i* is eligible for the subsidy ($\mathbb{1}\{20 \leq age \leq 32\}$).[18] The interaction between $Post$ and T is the standard difference-in-differences effect controling for seperate pre-trends (δ). We include age fixed effects (γ_{a_i}) to control for common, characteristics at different ages that affect fertility and abortion choices, month fixed effects to capture seasonality in fertility, and year fixed effects (γ_{y_t}) and month fixed effect (γ_{m_t}) to control for age-invariant time trends and seasonality that affect abortion rates and fertility. Lastly, X_{it} represents a set of pre-pregnancy controls (ethnicity, education, yearly earnings, months worked and total number of children). Standard errors are clustered at the age level.

However, this strategy is assuming a level shift rather then a more continouse increase as suggested in Figure II. Therefore, we apply a second strategy in which we first residualize the abortion rate on seperate pre-trends for the control and treated groups (Equation 3) and then run the standard DiD on the residualized abortion rate (Equation 4):[19]

[18]As described in more detail in the 3 Section, we move the treatment timing a month back to account for pregnancies conceived in December 2013, which may not even have been discovered until January 2014 when the policy was already in place, should be considered treated.

[19]Since there is no straight forward way to calculate standard errors in this case, we calculate them using a 1,000 bootstrap replications.

Figure D2: Generalized Difference-in-difference (18-40)



Notes: This figure presents the difference in abortion rate between treated (aged 20-32) and control (aged 16-19 & 33-40) women over time (2009-2016). The dashed line indicates the timing of the 2014 policy change. Each dot represents the coefficient δ_k estimated from the generalized difference-in-differences (Equation 2). The shaded regions marks the 90% and 95% confidence interval around the point estimate, respectively.

$$\text{Resid. Pre-trends: } \tilde{abort}_{it} = \delta \cdot Post_t \times T_i + \gamma_{a_i} + \gamma_{y_t} + \gamma_{m_t} + X'\gamma_i + \epsilon_{it} \tag{3}$$

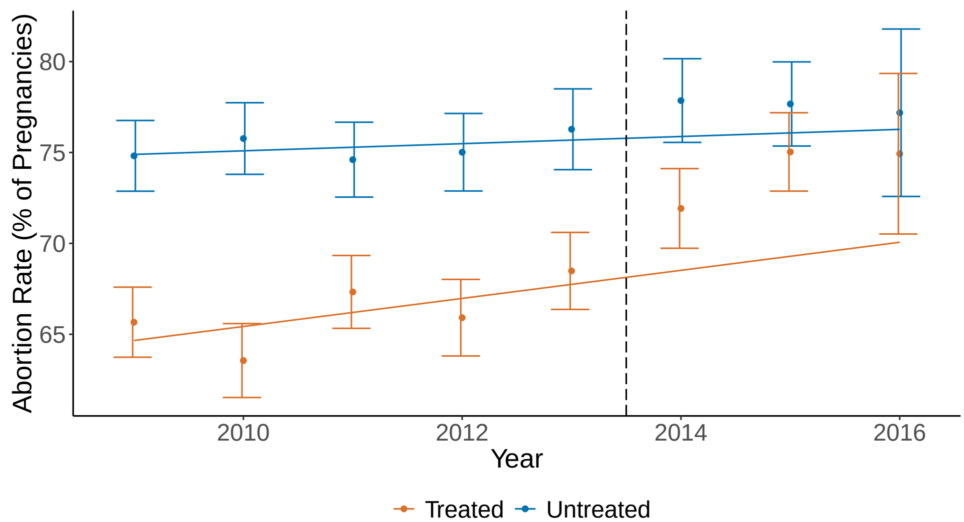
$$\tilde{abort}_{it} \equiv abort_{it} - \widehat{\beta}^{T}_{Pre} \times T_i \times t \tag{4}$$

We can see in Table D1 the results are fairly similar in both specifications (and to our main results in Table III). While these specification might seem more prudent, they also imply a strong (linear) functional form which we prefer to avoid. Therefore, we keep our DiD estimation as our main specification to keep our analysis as non-parametric as possible.

Table D1: Effect is Stable Across Various LTT Specifications

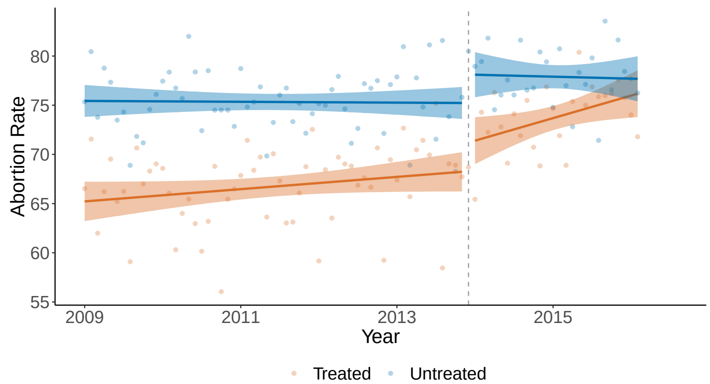
	Level-Shift	Resid. Pre-Trends
	(1)	(2)
Treatment Effect	3.41***	3.62***
	(1.29)	(1.09)
Observations	56,885	56,885
R^2	0.03	0.004
Note:	*p<0.1; **p<0.05; ***p<0.01 Standard errors for the residualized linear time trends specification are calculcated using 1000 bootstrapped replications.	

Figure D3: Parallel Trends Assessment (18-21)

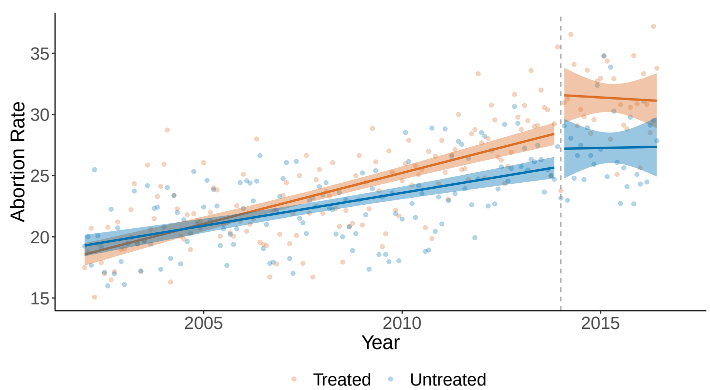


Notes: This figure presents the abortion rate for treated (20-21) and control (18-19) women over time (2009-2016). The dashed line indicates the timing of the 2014 policy change. Each dot represents the mean abortion rate in a given year for the tratment and control groups of women, respectively, and the lines mark the 95% confidence interval around the point estimate. The linear lines are fitted separately before the policy change for each group (and extrapolated post the policy). The control population is presented in blue, and the treatment population is presented in orange.

Figure D4: Parallel Trends Assessment at the Age Cutoffs (Monthly)



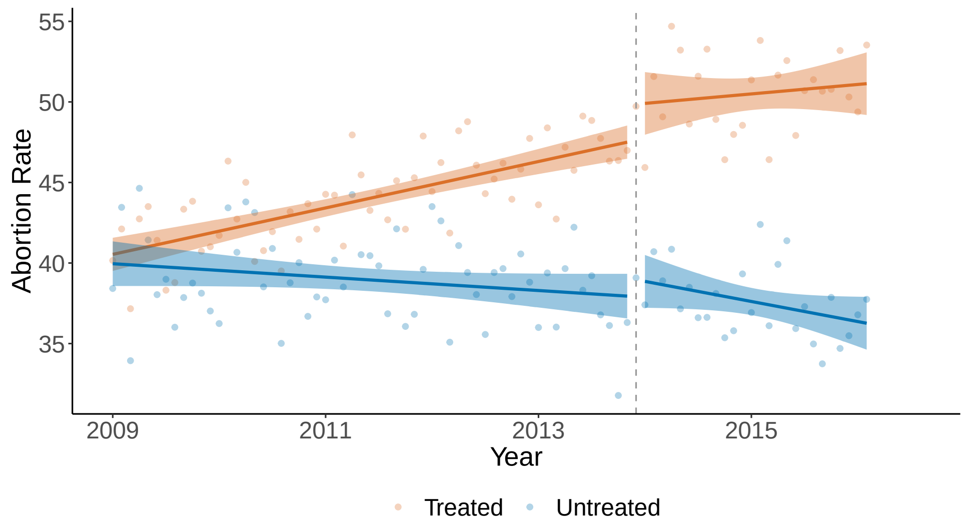
(a) 18-21



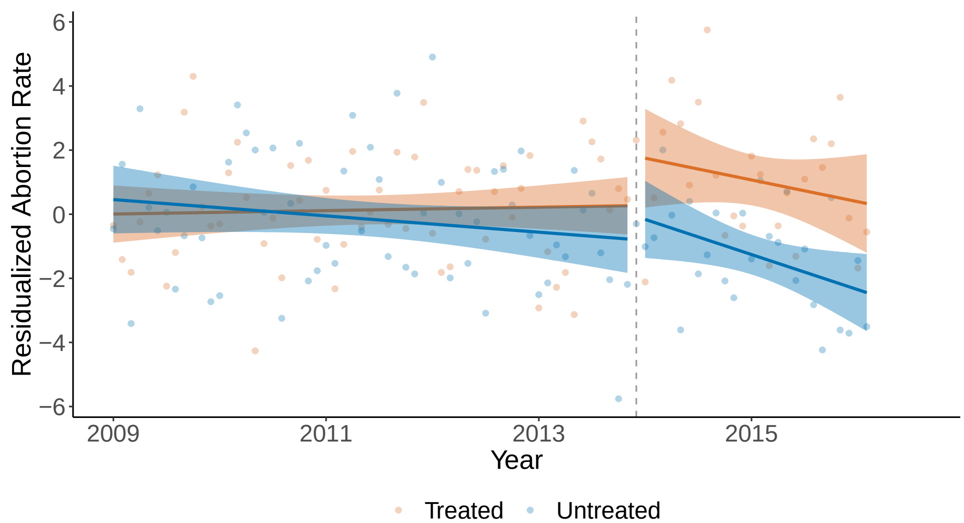
(b) 30-35

Notes: This figure presents the monthly abortion rate for treated and control women over time (2002-2016). The dashed line indicates the timing of the 2014 policy change. Each dot represents the mean abortion rate in a given month-year for the eligible and ineligible groups of women, respectively. The linear lines are fitted separately before and after the policy change for each group. The ineligible population is presented in blue, and the eligible population is presented in orange. Panel (a) presents the trends in abortion rate for women aged 18-21 (eligibility is defined as 20-21). Panel (b) presents the trends in abortion rate for women aged 30-35 (eligibility is defined as 30-32). Since the older cutoff doesn't suffer from the reason that pushed us to restrict the sample to 2009 (change in policy for 19 years old in 2008) we use our entire sample since 2002.

Figure D5: Parallel Trends for the Entire Sample (Monthly)



(a) Parallel Trends (Women aged 16-40)



(b) Residualized Parallel Trends (Women aged 16-40)

Notes: This figure presents the abortion rate for eligible and ineligible women over time (2002-2016). The dashed line indicates the timing of the 2014 policy change. Each dot represents the mean abortion rate in a given month-year for eligible (orange) and ineligible (blue) groups of women. The linear lines are fitted separately before and after the policy change for each group. The ineligible population are represented in blue, and the eligible population are represented in orange. Panel (a) presents the trends in abortion rate for women aged 16-40 (eligibility is defined as 20-32). Panel (b) presents the same population residualized on the pre-2014 linear time trend.

E Toy Model

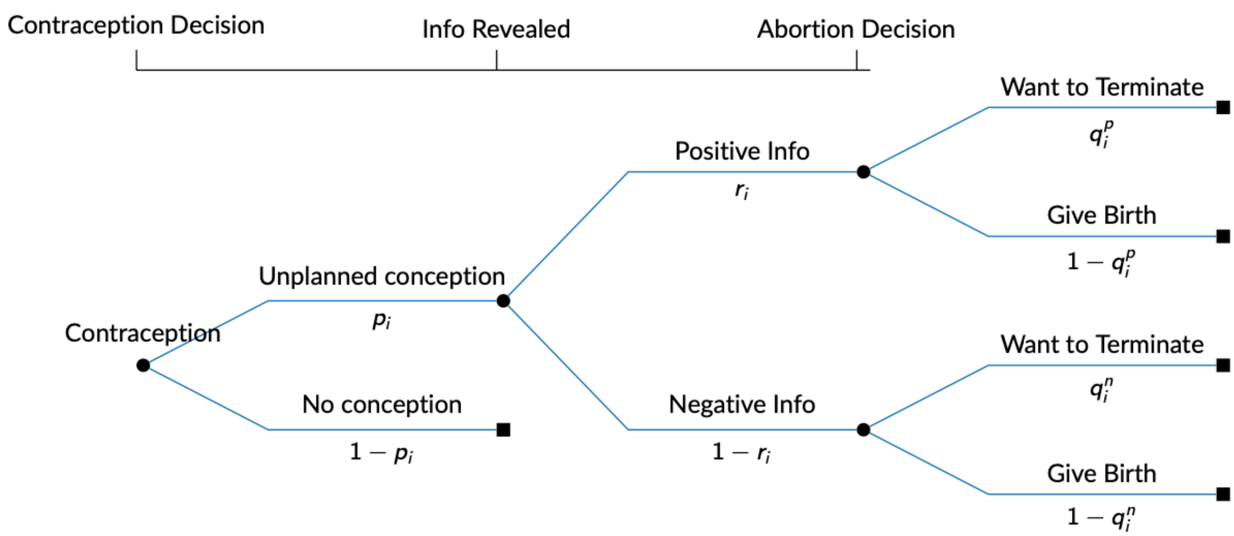
In this Appendix we describe in further details the micro-foundations of the conceptual model presented in Subsection 5.2. We then show how this simple economic model maps to a statistical model of our reduced-form estimation, as well as provide us with alternative estimation methods.

E.1 Canonical Model - 'Abortion as Insurance' (Levine & Staiger 2002)

Levine and Staiger (2002) present the 'Abortion as Insurance' model as a two-step decision process with incomplete information, as illustrated in Figure E1. In the model, a woman first makes a decision on contraception intensity p_i ($p_i \in [0, 1]$), which implies a conception will happen with probability $1 - p_i$. At this point, the woman has incomplete information about the conditions in which the birth will occur (i.e., whether the father wants the pregnancy or whether her family will support her). She assigns a probability r for receiving a good signal and with probability $1 - r$ a bad signal. Once conception is realized, the information is revealed, and the woman chooses whether to terminate ($q_i = 1$) or give birth ($q_i = 0$).

The theory predicts that incomplete information creates moral hazard in response to increases in abortion access, resulting in an increase in both abortions and births. A reduction in the abortion cost (i.e. monetary, physical, or psychological) will translate, by backward induction, to a lower contraception decision, resulting in more conceptions. A share of those added conceptions will receive positive information, resulting in more births; and a share will receive negative information, resulting in more abortions. The increase in abortions and births mechanically implies an increase in total conceptions.

Figure E1: 'Abortion as Insurance' Model (Levine & Staiger (2002))



Notes: this Figure illustrates the 'abortion as insurance' model: a two-step decision process with incomplete information as suggested by Levine and Staiger (2002). See full description above.

E.2 Updating the Model

We modify the Levine and Staiger (2002)'s 'abortion as insurance' model to incorporate credit constraints, as illustrated in Figure IV. Given the lack of evidence of the moral hazard channel presented in Subsection 5.1 we collapse the incomplete information and the abortion decision to one decision node. Then, we add another layer for the model capturing the combination of credit constraints and social stigma cost (hence socially constrained). This feature of the model captures the fact that credit-constrained young women from traditional households cannot rely on their social network as a safety net for the monetary cost of the abortion.

In the model, a woman first makes a decision on contraception intensity p_i ($p_i \in [0,1]$), which implies a conception will happen with probability $1-p_i$ (decision I). At this point (decision II), the woman chooses whether to terminate ($q_i = 1$) or give birth ($q_i = 0$). Finally, conditional on wanting to terminate a pregnancy, a woman is either credit constrained ($z_i = 0$) or unconstrained ($z_i = 1$).[20] Below we setup each decision in the tree more formally.

E.2.1 Decision I: Contraception Intensity

Let V_i^j be the expected preferences of woman i over outcome $j \in \{noBirth, birth, abort\}$, and $C(p_i)$ be the contraception cost.[21] A woman i chooses p_i to maximize her expected utility:

$$\max_{p_i} U_i(p_i) - C(p_i)$$

Where:

$$\begin{aligned} U_i = &(1-p_i) \cdot V_i^{noBirth} \\ &+ p_i \cdot q_i \cdot z_i \cdot (V_i^{noBirth} - V_i^{abort}) \\ &+ p_i \cdot q_i \cdot (1-z_i) \cdot V_i^{birth} \\ &+ p_i \cdot (1-q_i) \cdot V_i^{birth} \end{aligned}$$

E.2.2 Decision II: Abortion Decision

Let π^{abort} be the monetary cost of abortion, y_i woman i's disposable income, and $w_i^{abort} = \mathbb{1}\{$ Woman i has financial support for an abortion (family or government)$\}$. Conditional on conceiving an unplanned pregnancy, a woman choose to terminate her pregnancy if: $q_i \cdot z_i = 1$. Where:

$$\begin{aligned} q_i \equiv &\mathbb{1}\{V_i^{noBirth} - V_i^{abort} \geq V_i^{birth}\} \\ z_i \equiv &\mathbb{1}\{\pi^{abort} \leq y_i + w_i^{abort} \cdot \pi^{abort}\} \end{aligned}$$

Note that V_i^{abort} is the dis-utility from doing the abortion (mental, physical, social, and monetary), in absolute values.

[20]It is possible to switch the order of the tree (e.g., the woman first knows her type). However, the model's final derivations are agnostic regarding the timing of this decision tree.

[21]Incomplete information on V_i^{birth} at contraception time is the explanation for the (potential) moral hazard.

F Selection Bias in Abortion Decision: OLS vs. IV

F.1 Estimation

The Israeli policy allows us to identify the effect of giving birth to an *unplanned* child, which may upend education or career plans more than a planned child, since one can plan ahead and thus choose the optimal time for parenthood given career and educational goals. Consider a naive OLS estimation for the effect of having an abortion on employment (and a range of other outcomes in Figure F2). Formally:

$$y_i^{Post} = \theta^{OLS} \cdot abort_i + \rho^{OLS} \cdot y_i^{Pre} + \gamma_{a_i} + \gamma_{c_i} + \epsilon_i^{OLS} \tag{5}$$

where y_i^{Post} is the mean outcome of women i in the year of and three years post conception year c_i, y_i^{Pre} is the mean outcome of women i in the three years prior to the conception year c_i, $abort_i$ is an indicator for whether women i had an abortion at time c_i, γ_{a_i} are age at conception fixed effects, γ_{c_i} are year-month of conception fixed effects, and ϵ_i is the error term.[22]

The problem with the naive OLS estimation is that women who have an abortion might be systematically different from those who do not, in ways that affect labor market outcomes. Column 9 in Table A2 emphasizes how women who had an abortion are statistically different on almost all observable dimensions from women who gave birth.[23]

The ideal experiment for studying unplanned children's impact would be to randomly assign whether women have children and compare labor market outcomes between women who ended up with kids to those who did not. In the absence of such an experiment, we use the 2014 policy, which eliminated the monetary cost of abortion, as a natural experiment to estimate the effect of avoiding an unplanned parenthood on education and labor market outcomes. We first estimate the intention-to-treat (or reduced form, RF) effect of the policy. However, the full effect of avoiding an unplanned parenthood is also of interest, which we instrument for using the 2014 policy and estimate the treatment-on-the-treated (or IV). Formally:

$$\text{2nd Stage: } y_i^{Post} = \theta^{IV} \cdot \widehat{abort}_i + \rho^{IV} \cdot y_{c_i}^{Pre} + \gamma_{a_i} + \gamma_{c_i} + \epsilon_i^{IV} \tag{6}$$

$$\text{1st Stage: } abort_i = \delta \cdot Post \cdot T_i + \rho^{abort} \cdot y_{c_i}^{Pre} + \gamma_{a_i} + \gamma_{c_i} + \epsilon_i^{abort} \tag{7}$$

$$\text{Reduced Form: } y_i^{Post} = \theta^{RF} \cdot Post \cdot T_i + \rho^{RF} \cdot y_{c_i}^{Pre} + \gamma_{a_i} + \gamma_{c_i} + \epsilon_i^{RF} \tag{8}$$

As in Equation 1, $Post$ is an indicator for the policy being in effect ($\mathbb{1}\{c_i \geq \text{Dec-2013}\}$) and T_i indicates woman i is eligible for the subsidy ($\mathbb{1}\{20 \leq age_{c_i}\}$). The rest of the terms are defined as in Equation 5.

Ex-ante, one should expect two types of differences between the OLS and the IV estimates. First, the OLS suffers from the standard selection problem (described above). Second, the compliers are women who could not afford \$600-\$1,000 to perform the abortion. Therefore, the IV is estimating a local average treatment effect (LATE) of removing the financial constraints of having the abortion among a disadvantaged population. The exclusion restriction in our case implies that the only channel through which the 2014 subsidy policy affects labor market outcomes is by changing the probability of having a child. While this assumption is not directly testable, we argue that it is plausible because the policy only changed the cost of having an abortion without changing the expected benefits of having an abortion or any other fertility-related policies.[24]

F.2 Results

Why would access for free abortions effect women's career outcomes? It is well documented that parenthood acts as a 'penalty' for women's careers.[25] Therefore, we need first to establish the increase in takeup of abortions

[22]Unlike Equation 1, here the outcomes are collapsed relative to the conception year c_i, which is the reason for the change in notation.

[23]Specifically, it's suggesting a negative selection on the earnings and education margins.

[24]One potential violation of the exclusion restriction is via the always takers that are now getting a lump-sum transfer at the size of the abortion subsidy. While we cannot test directly for this assumption, we show in Figure F2 the effects are consistent (and if anything stronger) in the socially and financially constrained population - where we have a higher rate of compliers, suggesting the estimated effects are driven by the share of compliers.

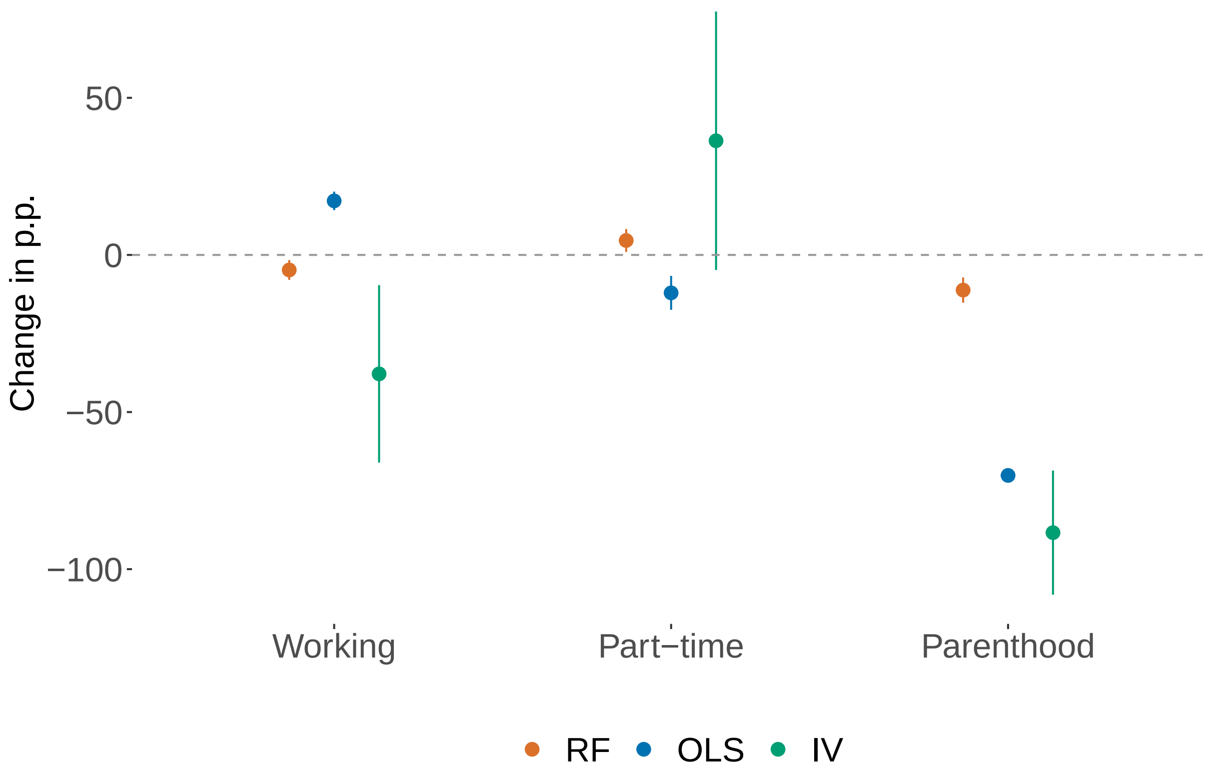
[25]This is known as the "child-penalty" literature, which documents a large and permanent drop in wages and employment for women after giving birth (Kleven et al., 2019b,a; Eckhoff Andresen and Havnes, 2019).

allowed women to delay parenthood. For this purpose we define a binary parenthood outcome ($\mathbb{1}\{\text{Is a parent}\}$) and estimate Equations 5 - 8. We present the OLS and IV results in Figure F1 for the socially and financially constrained population (see Table F1 for comparison of the estimates between the socially constrained and all the young unmarried women). The results supports our prior – 88% of the compliers did not enter parenthood in the subsequent four years following the conception they aborted. Furthermoe, the removal of financial constraints allowed the constrained women to avoid unplanned parenthood *and* a subsequent unplanned marriage (see Figure F2a).

Does avoiding an unplanned parenthood increase or reduce employment? the 'child-penalty' literature suggests a big and persistent decrease in employment after becoming a mother, but is that the case for *unplanned* parenthood as well? To answer that we need first to understand what happens to young unmarried religious women in Israel who concieve. Based on the International Social Survey Programme (ISSP) 80% of Israelies believe a 'women with children under school age should work outside the home', the highest share in the OECD (Kleven et al., 2020). Our data supports this claim in our sub-population – 75% of our young unmarried socially and financially constrained women who concieved are employed at the year of conception (see Figure **??**); this share grows up to 80% three years post-conception.[26] Therefore, avoiding the need to provide for the new-born child as a young unmarried mother might result in a *decrease* in employment. We find that avoiding an unplanned pregnancy resulted in a decrease in employment. We interpret the positive naive estimates from the OLS as the result of selection, where some women abort the pregnancy (if they are not constrained) due to a higher opportunity cost of working.

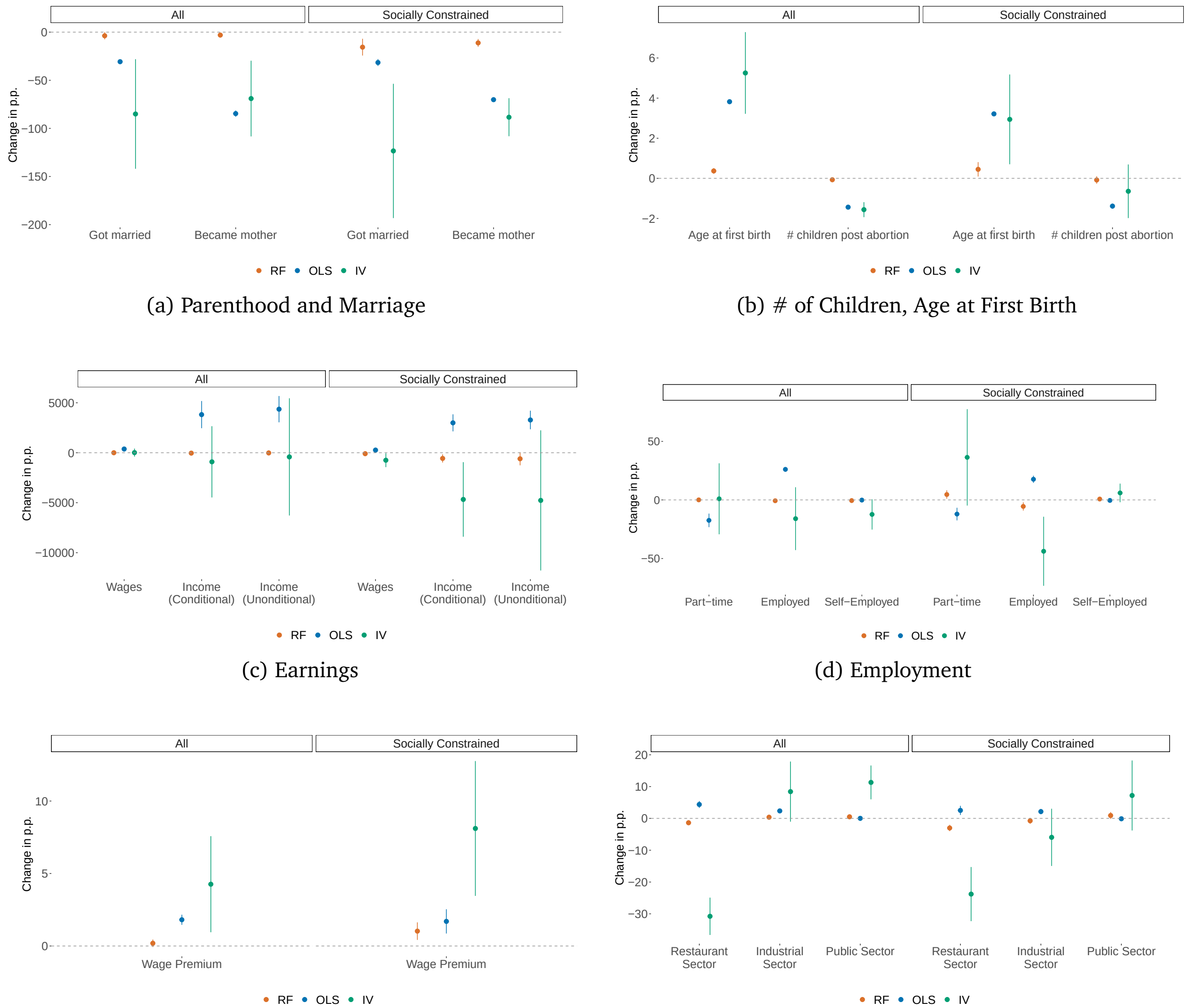
[26] It is important to note the socially and financially constrained women in our sample are coming from a very religious Jewish background, including the ultra-Orthodox community in which women are primary the bread-earners because the men are expected to devote themselves to studying the Torah. The Orthodox community is also very patriarchal and the burden of child-rearing is commonly shared between the women in the family (Lidman, 2016).

Figure F1: Decrease in Parenthood and a Shift Towards Part-time Employment



Notes: This figure presents the OLS (orange) and IV estimation (blue) as estimated in Equations 5 - 8 respectively. The sample consist of unmarried women 18-21 years old who conceived between 2009-2016, and are socially and financially constrained (religious and low-SES, see Section 5.2 for definition). The dashed line indicates a null effect. The lines represents the 95% confidence interval around the point estimate.

Figure F2: OLS vs. IV for Socially Constrained and All Women (18-21, Unmarried)



(a) Parenthood and Marriage

(b) # of Children, Age at First Birth

(c) Earnings

(d) Employment

(e) Wage-Premium

(f) Sector

Notes: This figure presents the OLS (orange) and IV estimation (blue) as estimated in Equations 5 - 8 respectively. The left panels consist of unmarried women 18-21 years old who conceived between 2009-2016. The right panels further restrict to the socially constrained (religious for low-SES, see Section 5.2). Each panel presents the results for a different se of outcomes: Panel (a) on an 'are you a parent?' and 'are you married?' indicators; Panel (b) on employment, self-employment and part-time employment; Panel (c) on earnings either conditional or unconditional on working, and on monthly wages; Panel (d) on employment by sector. The dashed line indicates a null effect. The lines represents the 95% confidence interval around the point estimate.

Table F1: Effect on Human Capital and Labor Market Outcomes

	Main Sample			Socially Constrained		
	OLS	IV	RF	OLS	IV	RF
Is a parent	-0.85	-0.69	-0.031	-0.7	-0.88	-0.11
	(0.016)	(0.2)	(0.0061)	(0.0098)	(0.1)	(0.021)
BA Enrollment	0.038	-0.087	-0.0039	0.028	-0.14	-0.018
	(0.014)	(0.067)	(0.0032)	(0.01)	(0.23)	(0.028)
Working	0.26	-0.28	-0.013	0.17	-0.38	-0.048
	(0.0075)	(0.2)	(0.0063)	(0.015)	(0.14)	(0.016)
Employed by a firm	0.26	-0.16	-0.0071	0.18	-0.44	-0.055
	(0.0079)	(0.14)	(0.0046)	(0.016)	(0.15)	(0.018)
Employed part-time	-0.17	0.0096	0.00043	-0.12	0.36	0.046
	(0.03)	(0.15)	(0.0068)	(0.028)	(0.21)	(0.019)
Self-employed	-0.0014	-0.12	-0.0055	-0.0043	0.06	0.0076
	(0.00076)	(0.066)	(0.0017)	(0.0011)	(0.041)	(0.0057)
N	24,650	24,650	24,650	1,790	1,790	1,790

Notes: This table presents results for the effect of the 2014 policy on a range of human capital formation and labor market outcomes. In the first three columns we present results for our primary analytic sample (unmarried, 18-21 year olds) and in the second three columns we present results for the sub-sample of women who are driving the effect – the low-income women from poor and religious backgrounds, ie. the "socially and financially" (as described in Section 5). The first and fourth column present the naive OLS (Equation 5), the second and fifth columns present results for the IV (Equation 6), and the third and sixth columns present results for the reduced from (Equation 8). Standard errors cultered by age at conception in parentheses.

G Event-study Relative To (Potential) Birth

G.1 Identification Assumptions

In this section we set out a simple conceptual framework (following (Kleven et al., 2019b) Appendix B) to clarify what is being estimated in the event studies. We denote by $k_i = (0, ..., k_{it}, ..., k_{iT})$ the anticipated lifetime path of fertility for woman i: the woman starts life with zero children, has k_{it} children at time t, and ends up with k_{iT} children over the lifetime. Earnings at time t are chosen based on the number of children present at time t as well as the anticipated lifetime path of fertility. Specifically,

$$Y_{it} = F(k_{it}, x_{it}, z_{it}) = F(kit, x(k_{it}, k_i, z_{it}), z_{it}) \tag{9}$$

where Y_{it} is earnings, $x_{it} = x(k_{it}, k_i, z_{it})$ is a set of earnings determinants that are chosen based on children, and z_{it} is a set of earnings determinants that do not depend on children. Compared to the Equation 12, we simplify notation by leaving out indexation of calendar time. The elements of x_{it} include variables such as months worked, employment, sector – variables which we have seen respond to children in the event studies – while the elements of z_{it} include factors such as age, ability and preferences. Hence, in this framework earnings may respond directly to children conditional on choices (e.g. the impact of being tired or distracted at work) and indirectly through labor market choices x_{it} (e.g. the impact of switching to a lower- paying, but more family-friendly firm). Furthermore, we allow labor market choices x_{it} to respond both to the contemporaneous number of children k_{it} and to the entire path of past and future fertility. The latter effect captures for example that some women may take less education or opt for family-friendly career tracks knowing that they will eventually have many children.

While we do not specify the demand for children, we make the assumption that children k_{it} are exogenous to the outcome variable Y_{it} conditional on the set of underlying determinants z_{it}. The assumption that "the event" (in our case, potential child birth) is not determined by the outcome variable is fundamental to any event study analysis. The graphical evidence presented above lends support to this assumption: there is no indication that outcomes respond prior to child birth (or prior to pregnancy as discussed above); the sharp breaks in career trajectories always occur just after having children. This framework allows for two conceptually different effects of children on earnings. One is a pre-child effect of future children, conditional on the current number of children k_it, which operates through the dependence of labor market choices x_{it} on anticipated lifetime fertility k_i. The other is a post-child effect of current children, conditional on anticipated lifetime fertility k_i, which operates through both the direct effect of k_{it} and the effect of k_{it} on labor market choices x_{it}. An obvious but important point is that the event studies cannot capture pre-child effects – these are incorporated in the pre-event levels that are differenced out – and is designed to identify only post-child effects. If women are investing less in education and career in anticipation of motherhood (as the child penalty sharply reduces the return to such investments), then the pre-child effect on female is negative and the event study provides a lower bound on the total effect.[27]

Under what conditions do the event studies correctly identify the post-child impacts? It is important to distinguish between short-run and long-run impacts. The short-run impact is estimated by comparing event times just before and after time zero. Denoting these event times by t_-, t_+ and using equation 9, the short-run event study estimates capture

$$E[Y_{it_+} - Y_{it_-}] = E[F(1, x(1, k_i, z_{it_+}), z_{it_+})] - E[F(0, x(0, k_i, z_{it_-}), z_{it_-})] \tag{10}$$

when we do not directly control for elements for z_{it} through for example age and year dummies. Assuming smoothness of the average non-child earnings path, i.e. $E[F(0, x(0, k_i, z_{it_-}), z_{it_-})] \approx E[F(0, x(0, k_i, z_{it_+}), z_{it_+})]$, Equation 9 identifies the short-run effect of the first child conditional on z_{it_+}. With direct controls for z_{it}, the smoothness assumption can be relaxed. The long-run impact is obtained by considering an event time t_{++} long after time zero, i.e.

$$E[Y_{it_{++}} - Y_{it_-}] = E[F(1, x(1, k_i, z_{it_{++}}), z_{it_{++}})] - E[F(0, x(0, k_i, z_{it_-}), z_{it_-})] \tag{11}$$

[27]On the other hand, if women are engaging in intertemporal substitution of work effort around the event of having a child, then the pre-child effect could be positive. However, our event graphs feature very stable pre-trends, indicating that no significant intertemporal substitution is taking place.

There are two differences between this impact measure and the previous one. The first difference is that the long-run impact captures the effect of total lifetime fertility k_{iT} as opposed to the effect of only the first child. The second difference is that the smoothness assumption is no longer sufficient for identification as we can still have large changes in non-child earnings components over a long event time window. Hence, if we are not fully controlling for z_{it}, then the long-run child penalty may be a biased estimate of the true post-child impact. Allowing for non-parametric age and year controls as we do in specification 12 may go a long way in alleviating this problem, but we cannot be certain that there is no remaining bias, which is the reason we use an interacted version of this estimation in Section 6.1.

G.2 Baseline Estimates for Young, Unmarried Women

Our detailed panel data on employment and education over a long period of time allow us to examine temporal dynamics of these effects. The temporal dynamics can help us shed light on the underlying mechanisms. For example, the negative effect on employment we identified with the IV is surprising if we expect that relative to women who gave birth, women who aborted should be working more, given they avoided the demands of child-rearing, at least in the year of birth. For this purpose, we will present in Section 6.1 an event-study relative to timing of (potential) birth, interacted with our policy variation. In order to understand better what's happening behind the interaction of the event-study and our variation we begin by presenting in this Section the standard event-study design relative to timing of potential birth (known as the 'child-penalty').

The analyses is estimated on the labor-market panel - a yearly-level employment and earnings panel for all women who conceived within our sample period. In order to have estimates that are not contaminated by treatment, we restrict to conception that took place prior to 2014, when the policy took an effect. In order to have a comparable benchmark for our later analysis of the policy, we restrict to three years before and after the year of potential birth. In Appendix G.3 we provide estimation on the entire sample (18-40 years old), ranging five year prior and 10 years post potential birth.

We adopt an event study approach comparing sharp changes in earnings around the birth of the first child for mothers relative to fathers. Although fertility choices are not exogenous, the event of having a first child generates sharp changes in labor market outcomes that are arguably orthogonal to unobserved determinants of those outcomes as they should evolve smoothly over time.The event study approach has the additional advantages of tracing out the full dynamic trajectory of the effects and of being very precise, as it exploits individual-level variation in the timing of first births. We spell out the identification assumptions of this approach in Appendix G.1.

For each woman in the data, we denote the year in which she has her first child by c_i, and index as $j = t - c_i$ all years relative to that year. Our baseline specification considers the labor-market panel - a balanced yearly earnings panel of women who we observe from three years before having her first child to three years after, and so the event time j runs from -3 to +3. We study the evolution of a wide set of human capital formation and labor market outcomes as a function of event time. Specifically, denoting by y_{it} the outcome of interest for woman i in year t and at event $j = t - c_i$, we run the following regression:

$$y_{it} = \sum_{j \neq -1} \alpha_j \cdot \mathbb{1}\{j = t - c_i\} + \gamma_{a_{it}} + \gamma_t + \gamma_i + \epsilon_{it} \tag{12}$$

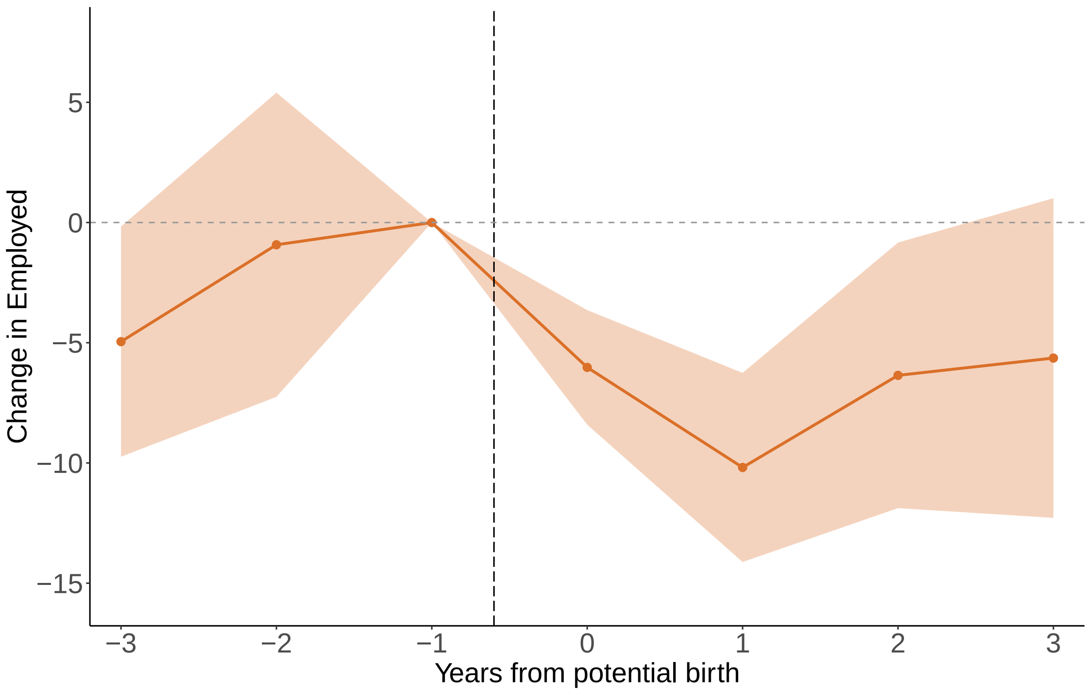
where we include a full set of event time dummies (first term on the right-hand side), age dummies (second term), and year dummies (third term). Unlike the age and time dummies estimated in Sections 4.1 and F which were the age at conception and year of conceptions, these are the age and year in which we observe a women in the panel. We omit the event time dummy at $j = -1$, implying that the event time coefficients measure the impact of children relative to the year just before the first child birth. If we did not include age and year dummies, the estimated event coefficient α would correspond simply to the mean value of the outcome at event time j, relative to the year before birth. By including a full set of age dummies, we control non-parametrically for underlying age-specific effects, and by including a full set of year dummies, we control non-parametrically for time trends such as wage inflation and business cycles. We are able to identify the effects of all three sets of dummies because, conditional on age and year, there is variation in event time driven by variation in the age at which individuals have their first child.[28] Lastly, for robustness we include a specification with person fixed effect

[28]The inclusion of age dummies is important for comparing women who abort to women who give birth (as we will do below) because women who abort tend to be younger than women who give birth.

γ_i.[29] Standard errors are clustered at the age level.

Figure G1 presents the event-study relative to year of potential birth, as estimated from Equation 12 on an indicator for employment. The population is all socially constrained, young (18-21) unmarried women who conceived. While child-penalties are commonly estimated only for the women who gave birth (as we do in Appendix G.3), we pull together both women who gave birth and the ones that had an abortion. We do so in order to provide the reader with a benchmark of the child-penalty estimates that will be on a comparable population as the ones estimates by the reduced-form in Section 6.1. Therefore, the standard child-penalty from merely women who gave birth is mitigated by the women who aborted in this figure. Nevertheless, we see the same pattern - a dip of 5-10 percentage points post conception without a full recovery.

Figure G1: Event Study on Employment



Notes: This figure presents the event-study relative to year of potential birth as described in Equation 12 for the probability of being employed. Each orange circle represents the treatment effect for the reduced form estimated from three years prior until three years post potential birth timing, relative to one year prior to potential birth (the dropped category). The effect on the probability of being employed is shown in orange, while the effect on being self-employed is shown in blue. The shaded region mark the 95% confidence interval around the point estimate. The dashed horizontal line is at 0, indicating an insignificant result. The sample is restricted to unmarried women aged 18-21.

G.3 Abortion vs. Birth

Our baseline specification considers the labor-market panel - a balanced yearly earnings panel of women who we observe from 5 years before having her first child to 10 years after, and so the event time j runs from -5 to +10. We study the evolution of a wide set of human capital formation and labor market outcomes as a function of event time. The specification follows the description in Section G.2 and Equation 3.

When comparing two populations, we want to state the effects in percentage change. To do so we follow Kleven et al. (2019b) and specify Equation 3 in levels rather than in logs to be able to keep the zeros in the data (due to non-participation) and report the effects in percentage change. We convert the estimated level effects into percentages by calculating $P_j \equiv \alpha^j / E[\tilde{Y}^g_{itj}|j]$, where $\tilde{Y}^g_{itj}$ is the predicted outcome when omitting the contribution of the event dummies, i.e., $\tilde{Y}^g_{itj} \equiv \sum_k \gamma^g_k \cdot \mathbb{1}\{k = age_{it}\} + \sum_l \gamma^g_l \cdot \mathbb{1}\{l = t\}$. Hence, P captures the year-j effect of children as a percentage of the counterfactual outcome absent children. We bootstrap the

[29] The results are also robust to a specification with a set of pre-pregnancy controls X_{it} (ethnicity, marital status, parental earnings, and total number of children). For the sake of space we don't show these specifications.

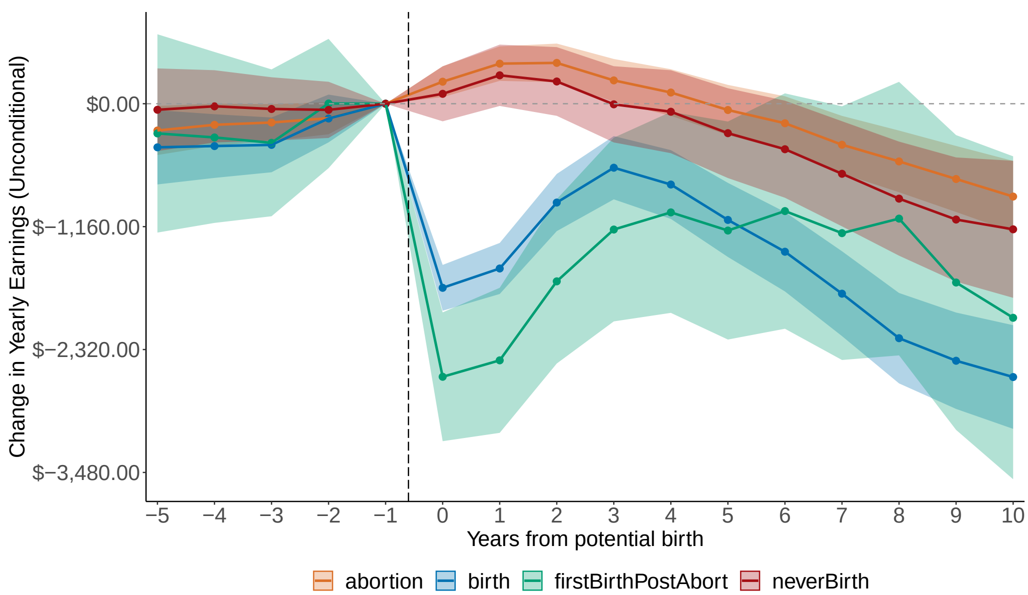
standard errors of P_j since it is a function of two object estimated separately. In each bootstrap we take 90% of the women in our sample, estimate Equation 3, take the estimates of $\alpha^j/E[\tilde{Y}^g_{itj}|j]$ to construct P_j. We repeat this 100 times and take the standard deviation of our results as the corrected standard errors.[30]

Women who gave birth experienced a greater decline in earnings than those who aborted, consistent with the expectation, as shown in Figure G3a. However, we also see a smooth reduction in earnings for the women who terminated their pregnancy, which may not have been expected. An intuitive explanation is that women who aborted went on to have children a few years later (as shown in Figure G3b). Thus, we observe a delay in the child-penalty.

We return to the same logic of the human capital investment - if the human capital investment decision is affecting the abortion decision, we should see returns on the long run. Focusing our attention on the population of our study - 18-21 years old unmarried women without children prior to this conception - we see a slightly different pattern. Figure G2 presents the same event-study just with two added lines for births happening after a prior abortion and women who abort and never gave birth in our sample. Women who had their first birth post abortion seem to suffer a deeper penalty than all women without children who gave birth, but a better recovery (with wide standard errors, due to smaller sample size). If the abortion allowed them to invest in their human capital, that could explain both the steep initial drop and the better recovery in the long-term.

We interpret the difference between the women who abort and the one who gave birth as the average treatment-on-the-treated (TOT) effect of abortion on women with different gains from performing an abortion. These estimates are novel by their own due to the unique data linking abortions, birth, and tax records. We present these comparisons between women who abort and women who gave birth across a range of labor-market and education outcomes on a different paper (Brooks and Zohar, 2020).

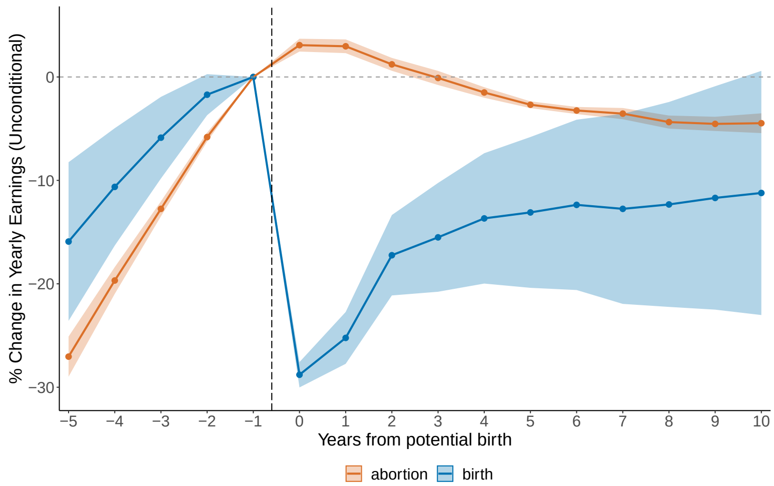
Figure G2: Yearly Earnings Abortion vs. Birth (18-21)



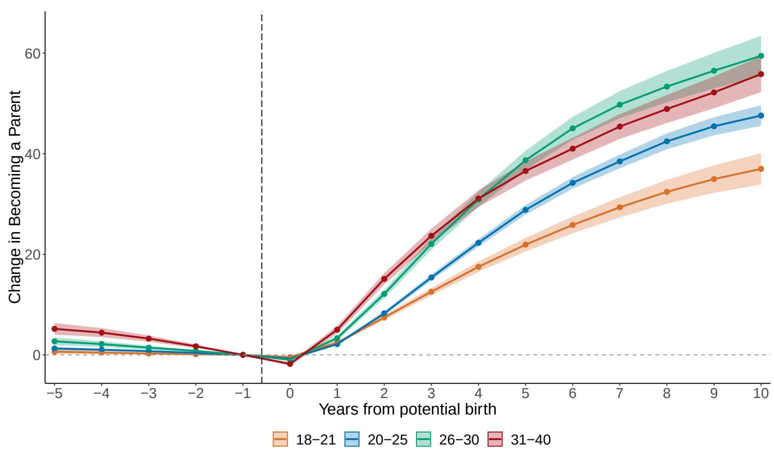
Notes: The figure shows event time coefficients estimated from Equation 3 as a percentage of the counterfactual outcome, absent children (i.e., $P_j \equiv \alpha^j/E[Y_{itj}|j]$ as defined in Section G.2) for all women who gave birth aged 18-21, split by women who gave birth and ones that had an abortion. All of these statistics are estimated on a balanced sample of women who had their first child between 2002-2013 and who are observed in the data during the entire period between five years before and ten years after child birth. The effects on earnings are estimated unconditional on employment status. The dashed horizontal line is at 0, indicating an insignificant result. The shaded region mark the 95% confidence interval around the point estimate, based on 1000 bootstraps.

[30]This procedure is computationally costly, given the standard errors correction seems to stabilize in our case at around 40 replications we decided to run 100.

Figure G3: Slower Decrease in Earnings for Women Who Had an Abortion



(a) Unconditional Earnings by Pregnancy Outcome



(b) Entrance to Parenthood Post Abortion by Age Group

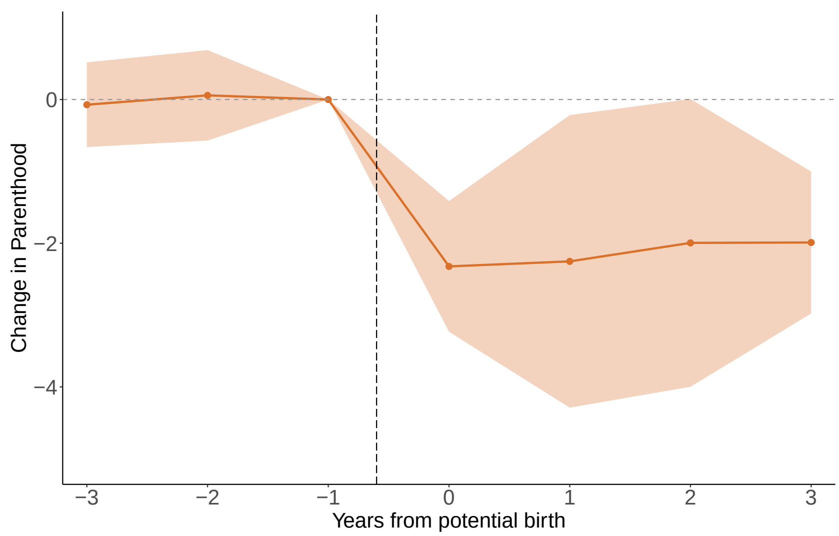
Notes: The figure shows event time coefficients estimated from Equation 3 for all unmarried women who conceived aged 18-40 (fertile age). All of these statistics are estimated on a balanced sample of women who had their first child between 2002-2013 and who are observed in the data during the entire period between five years before and ten years after child birth. Panel (a) presents the effects on earnings unconditional on employment status, as a percentage of the counterfactual outcome, absent children (i.e., $P_j \equiv \alpha^j / E[Y_{itj}|j]$ as defined in Section G.2), split by women who had an abortion and women who carried the pregnancy to term. Panel (b) presents the effect for an indicator of 'are you a parent?' in a given year, for the women who aborted, split by age groups at time of abortion. The dashed horizontal line is at 0, indicating an insignificant result. The shaded region mark the 95% confidence interval around the point estimate, based on 1000 bootstraps.

H Effect on Human Capital and Labor Market Outcomes (18-21, Unmarried)

H.1 Human Capital Investment

First, we want to establish that the increased abortion access did indeed delay entrance to parenthood and thus avoiding an unwanted child is a channel that could drive differences human capital investments and labor market outcomes. We define a binary parenthood outcome ($\mathbb{1}\{\text{Is a parent}\}$) and estimate Equation 4. We observe a decrease in the probability of entering into parenthood following an abortion (Figure H1).[31] The share of 18-21 unmarried women who conceived and then entered into early parenthood was reduced by 2.1 p.p. due to the reduction of abortion cost (mean in years 0-3 = 17% - 39%). Indeed the magnitude of these effects is smaller then the estimated first stage (4.6 percentage points). This is consistent with the pattern of entrance to parenthood post abortion shown in Figure G3b.[32]

Figure H1: Abortion Access Decreased Entrance to Parenthood



Notes: This figure presents results for the reduced form effect of the 2014 policy on the probability a woman will become a mother relative to year of potential birth as described in Equation 4. Each orange circle represents the treatment effect for the reduced form estimated on 'are you a parent' indicator, from three years prior until three years post potential birth timing, relative to one year prior to potential birth (the dropped category). The shaded region mark the 95% confidence interval around the point estimate. The dashed horizontal line is at 0, indicating an insignificant result. The sample is restricted to unmarried women aged 18-21.

Given the young age of these women, we examine what happens to their human capital investment after terminating a pregnancy. Our data includes a rich set of time-varying educational outcomes, including whether a woman received or retook her high school diploma ('Bagrut'),[33] whether she took the SAT, if she is enrolled in academic studies (i.e. BA or some non-vocational college), or took part in some type of non-academic training (e.g. vocational training, teacher diploma).

We first estimate Equation 4 for a broad human capital formation outcome: whether the woman is currently

[31] Also, abortion access can increase the total number of conceptions via a moral hazard explanation and thus have no effect on the likelihood of becoming a parent. However, we show in Section 5.2 this is not the case.

[32] Another reason for the smaller results in Figure H1 is that part of the effect of the first stage are coming from women that already had children, while Equation 4 is estimated over women who had no children prior to conception.

[33] Bagrut is a series of standardized examinations across different subjects that Israeli students must pass in order to get their high school diploma (matriculation certificate). After high school, students that don't have a Bagrut diploma or would want to improve it, can retake it as many times as they would like, resulting in people still taking it in their early 20's. In Israel, access to college majors is also determined by Bagrut performance, with some professional programs requiring minimum overall average scores for admission. Furthermore, admission decisions in Israel are based almost entirely on concrete measures of student performance such as the Bagrut or the SAT (e.g. no weight assigned to extracurricular activities or student essays). As a consequence, Bagrut scores can affect an individual's entire academic career, and subsequent labor market outcomes.

studying [34]. We find an increase in the probability of studying following an abortion (Figure H2a). Specifically, the share of 18-21 unmarried women who conceived and studied increased by 1.2-6.9 p.p. due to the reduction of abortion cost (mean in years 0-3 = 13% - 20%). The negative pre-trends are due to our treated group (20-21 year olds), who mechanically are more likely to take the Bagrut exams three years prior to conception because of their age (17-18), as compared to our control group (18-19 year olds), who are 15-16 years old three years prior to conception, in which age they are not likely to take the Bagrut exam.[35]

We have seen that increased access to abortion results in a greater likelihood of studying. Importantly, our compliers are (by construction) individuals that are affected by a subsidy of $600 - $1,000 on abortions, thus likely to come from low SES background (which we will show in detail in Section 5). Therefore, identifying precisely what they are studying is important for understanding the implications for human capital accumulation. To examine this, we drill down to three more specific outcomes: whether a woman received her high school diploma ('Bagrut'), took the SAT, or was enrolled in academic studies using the same approach. Figure H2b shows that there is no effect on the probability of being enrolled in academic studies or taking the SAT. Instead, it seems the compliers are more likely to finalize (or perhaps retake) their high-school diploma. In other words, the share of 18-21 unmarried women who conceived and retook their high-school diploma exams increased by 3.5 p.p. due to the reduction of abortion cost (mean in years 0-3 = 1% - 0%). Note these effects are huge given 62% of our sample didn't have high-school diploma at baseline.

H.2 Labor-Market Outcomes

Does this investment in human capital translate into better paying jobs? To answer this we estimate Equation 4 on two outcomes: the probability of working in a white collar sector (defined as tech, finance, law, R&D, education, health, or public service), and the sector-level wage-premium. We estimate the sector-level wage-premiums following Abowd et al. (1999) by running a log-wage regression on individual and sector fixed effects (known as AKM, see further details in Appendix I.3).

The results in Figure H4a suggest The share of 18-21 unmarried women who conceived and worked in a white collar sector increased by 0.5-4.2 p.p. due to the reduction of abortion cost (mean in years 0-3 = 16% - 19.7%). Similarly, Figure H4b suggest an increase of 1% in the wage-premium of the sector these women are working at.

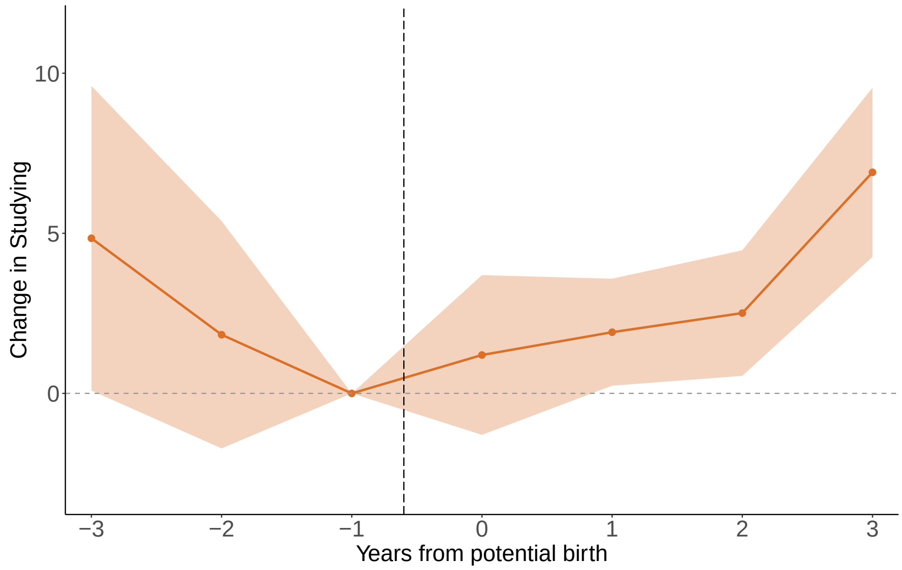
Given these results, one might expect an increase in yearly earnings due to the policy change. We find a temporary increases in yearly earnings unconditional on working (Figure H4c). Specifically, yearly earnings (unconditional on working) of 18-21 unmarried women who conceived increased temporarily by $406 due to the reduction of abortion cost (mean in years 0-3 = $5,912 - $10,597). A relative increase in the year of potential birth is intuitive, as the counterfactual woman would have given birth to a child. The subsequent decrease is consistent with a shift toward human capital investment, as shown in Figure H2a.

Finally, we test for the effect of abortion access on employment (extensive margin) as shown in Figure H3. Surprisingly, we find no effects on employment or self employment. Instead, we find a shift towards part-time employment (see Figure H4d) due to the increase in abortion access. Specifically, the share of 18-21 unmarried women who conceived and worked part-time (earned below min. monthly earnings) increased by 6.5 p.p. due to the reduction of abortion cost (mean in years 0-3 = 77% - 56%). This results is consistent with substitution towards educational attainment as shown above. In other words, the counterfactual women that could not have had the abortion before the policy, ended up working full-time in order to provide for the baby. Now, when abortion is provided for free, she is more likely to finalize her high-school diploma and work in higher paying sector, but since time is scarce she is shifting to part-time while finalizing (or improving) her bagrut.

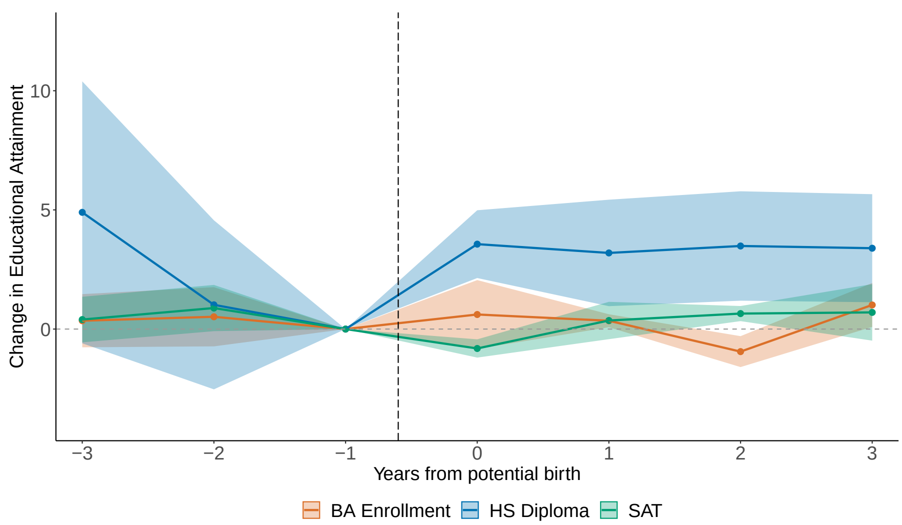
[34] Defined as studying=1 if she got the bagrut, took the SAT, was in academic studies, or in non-academic studies.

[35] Bagrut exams are taken during high-school (age 16-18), mostly in the last couple years of high-school 17-18.

Figure H2: Abortion Access Increased Human Capital Accumulation



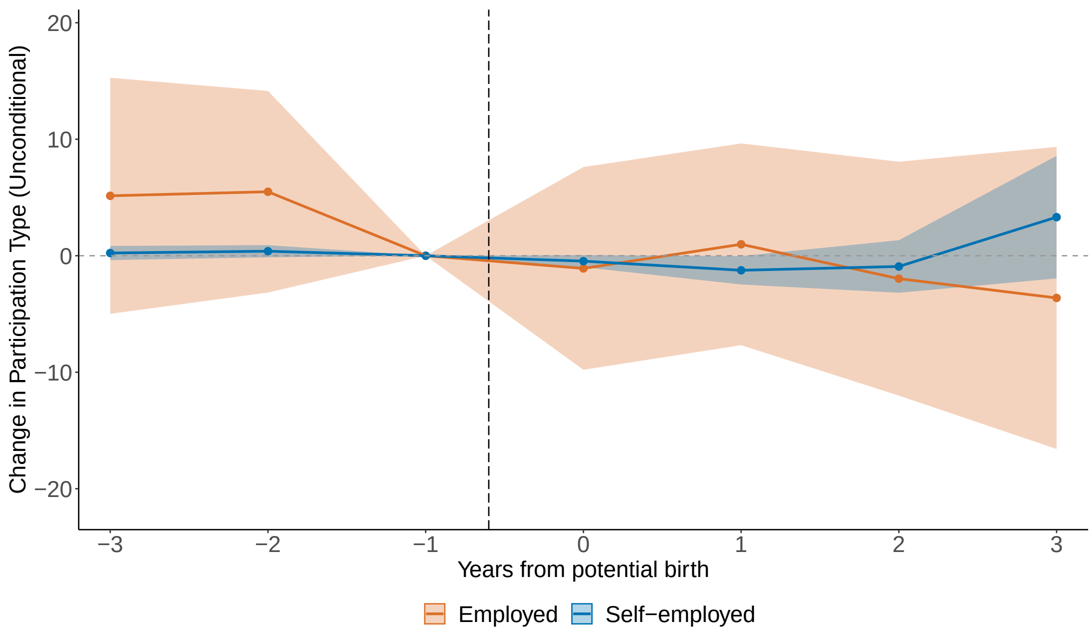
(a) Effect on Studying (High-school Diploma/SAT/BA/Non-academic Training)



(b) Effect by Education Type

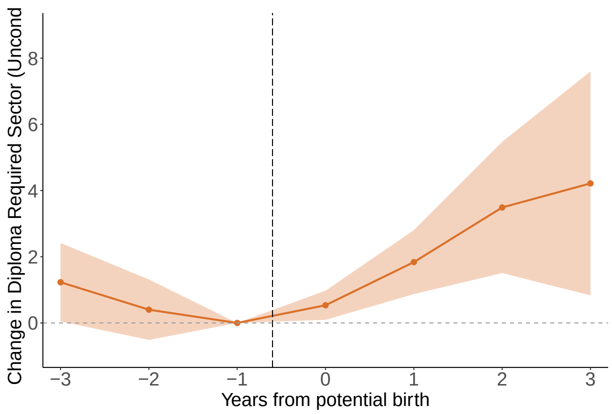
Notes: This figure presents results for the reduced form effect of the 2014 policy on the probability a woman will study relative to year of potential birth as described in Equation 4. Panel (a) presents the treatment effect for the reduced form estimated on 'are you studying' indicator, from three years prior until three years post potential birth timing, relative to one year prior to potential birth (the dropped category). Similarly, Panel (b) presents the reduced form estimates on three outcomes: got her high school diploma ('Bagrut'), took the SAT, or enrolled to academic studies. The shaded region mark the 95% confidence interval around the point estimate. The dashed horizontal line is at 0, indicating an insignificant result. The sample is restricted to unmarried women aged 18-21.

Figure H3: Effect of Increasing Abortion Access on Labor Market Participation

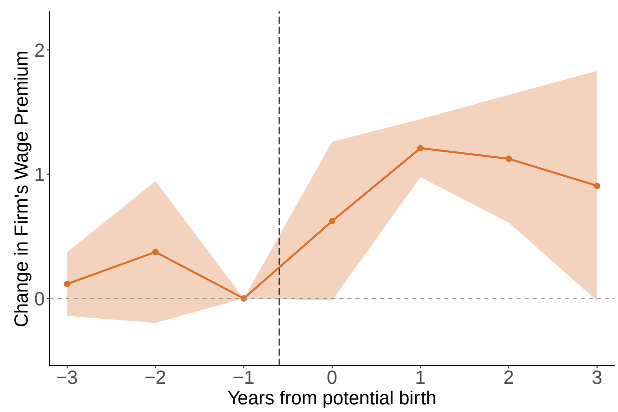


Notes: This figure presents results for the reduced form effect of the 2014 policy on the probability a woman will study relative to year of potential birth as described in Equation 4. Each orange circle represents the treatment effect for the reduced form estimated from three years prior until three years post potential birth timing, relative to one year prior to potential birth (the dropped category). The effect on the probability of being employed is shown in orange, while the effect on being self-employed is shown in blue. The shaded region mark the 95% confidence interval around the point estimate. The dashed horizontal line is at 0, indicating an insignificant result. The sample is restricted to unmarried women aged 18-21.

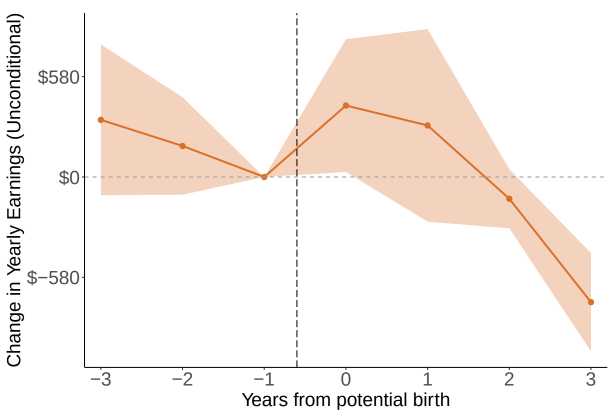
Figure H4: Effect of Increasing Abortion Access on Labor Market Outcomes



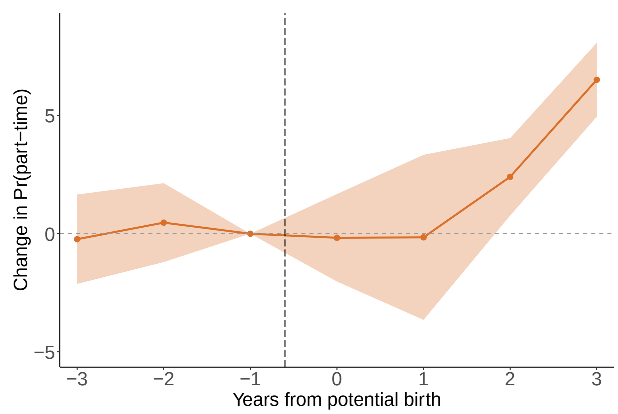
(a) Shift Towards Employment in White Collar Sectors



(b) Increase in Employment in Sectors with Higher Wage-Premium



(c) Temporary Increase in Unconditional Earnings



(d) Delayed Increase Part-time Employment

Notes: This figure presents results for the reduced form effect of the 2014 policy on the probability a woman will study relative to year of potential birth as described in Equation 4. Each orange circle represents the treatment effect for the reduced form estimated from three years prior until three years post potential birth timing, relative to one year prior to potential birth (the dropped category). Panel (a) presents the effect on the probability of working in a white collar sector (i.e. Tech/Finance/Law/R&D/Educ./Health./Public Service). Panel (b) presents the sector-level wage premiums (estimated from an AKM regression). Panel (c) presents the effect on the yearly earnings (unconditional on working). Panel (d) presents the effect on the probability of working part-time (defined as monthly earnings lower than the minimum monthly earnings). The shaded region mark the 95% confidence interval around the point estimate. The dashed horizontal line is at 0, indicating an insignificant result. The sample is restricted to unmarried women aged 18-21.

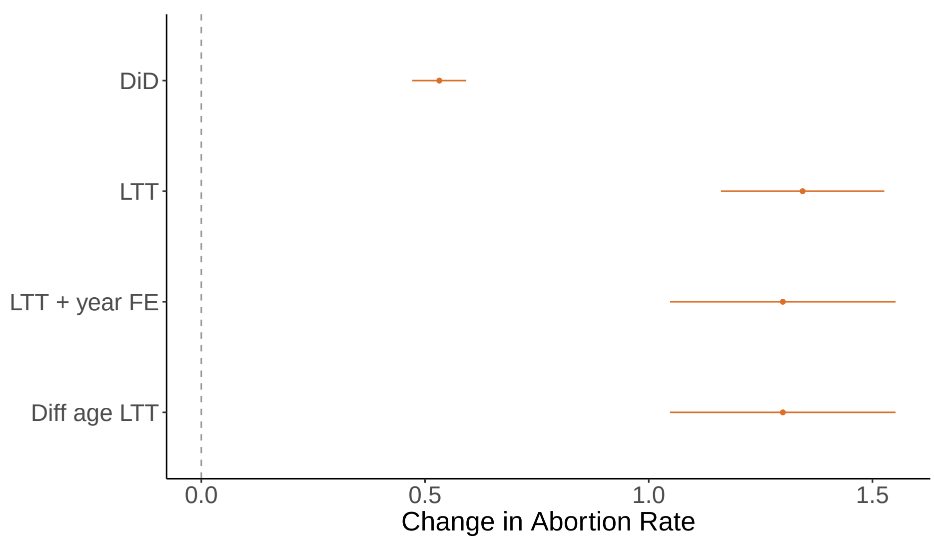
I Miscellaneous

I.1 Hassle Cost

To test for the effect of reduction in the hassle cost of navigating the health insurance system, we exploit another policy change that took place in May 2004, eliminating the need for proof of coverage for women aged 18 or below. Even when women do not have to pay for a health service, such as an abortion, in the Israeli health system they are required to provide the hospital with a payment guarantee from their HMO (health insurer) demonstrating that the procedure is covered. This requires obtaining the payment guarantee form upfront before seeking an abortion or paying out of pocket and going through the HMO to get reimbursed later. Motivated by the concern that the teenagers are unable to navigate the bureaucratic system, and would not want to involve their parents or extended family and social network for support, the Ministry of Health decided in 2004 to eliminate the requirement to obtain a payment guarantee for an abortion for women aged 18 and under.

We test for the effect of the policy and find an increase of 0.8-1.2 p.p in abortion rate due to the reduction in hassle cost. More specifically, we exploit the May 2004 policy change and comparing the effect of women aged 18-19 who conceived around the cutoff (18 years old). We restrict to conceptions between 2002-2008 (before the next change of policy that added 19 years old women to the subsidy). Figure I1 presents the results. These results suggest that for very young women who may find it difficult to navigate the bureaucratic and administrative complexities of the health care system necessary to have their abortion paid for by the state, eliminating the requirement to get a payment guarantee has pronounced effects. Note that this exact reduction in hassle cost did not take place on the 2014 policy, but removing the need to come up with the funding can be a reduction in hassle cost by itself.[36] We use the 2004 policy as some evidence for pronounced effects of a small reduction in hassle cost.

Figure I1: Reduction in Hassle Cost Increased the Abortion Rate



Notes: This figure presents difference-in-difference results for the effect of the May 2004 policy that eliminated the requirement to have a payment guarantee from the *kupa* for women below 19 on abortion rates. We exploit the May 2004 policy change and compare the effect of women aged 18-19 who conceived around the cutoff (18 years old). We restrict to conceptions between 2002-2008 (before the next change of policy that added 19 years old women to the subsidy). Each dot represents the treatment effect ($\delta \cdot Post_t \times T_i$) and the lines mark the 95% confidence interval around the point estimate. The dashed vertical line is at 0, indicating an insignificant result.

[36] Even if a woman doesn't have the money in her bank account but have a source of funding, still figuring out how to raise the money could be seen as a hassle cost.

I.2 Age Heterogeneity

To estimate the behavioral response to the policy by age, we conduct a "before-and-after" exercise by age that illustrates variation in abortion patterns around the timing of the policy change. As illustrated in Figure I2, we show that of the two age cutoffs (i.e., 19 and 33), the policy only had an effect on the 19-year-old cutoff. This result is intuitive: younger women are less financially independent than women in their early 30s; thus, removing the monetary cost has a bigger effect on the younger age cohort.

We use the conception cross-section, restricted to two years before and two years after the 2014 policy change (2012-2015), to avoid capturing time trends in this exercise to the greatest degree possible. Then, we restrict the sample to all unmarried women of a given age a (e.g., $a = 23$) and estimate the difference in abortion rate before and after the 2014 policy change by regressing an indicator for having an abortion on a "post" dummy. Formally:

$$abort_{it} = \sum_{j=18}^{40} \delta_j^{Post} \cdot \mathbb{1}\{a_{it} = j\} \cdot Post_t + \sum_{j=18}^{40} \delta_j^{Pre} \cdot \mathbb{1}\{a_{it} = j\} + \epsilon_{it} \tag{13}$$

where $abort_{it} = \mathbb{1}\{Abortion\}$, $Post = \mathbb{1}\{t \geq Dec2013\}$ and ϵ_{it} is an error term. We repeat this exercise for each age in the 18-40 sample and plot in Figure I2a the coefficients on "post" for each age group and 95% confidence intervals. The reported estimates are in percentage points, but to account for the different baseline rates of each age group, we also show in Figure I2b the effects in percentage terms relative to baseline rates.

Among those 20 to 32 years of age, we see an increase in the abortion rate in the post-period relative to the pre-period, with larger and statistically significant increases among younger women aged 20-26. These results suggest a 20% increase among 25-26-year-olds during the post-period relative to the pre-period. In contrast, among women unaffected by the policy change (19 and under and 33 and older, shown in blue in the figure), the effects are mostly statistically indistinguishable from zero. This pattern is mostly consistent with our prior, and the results are shown in Section 4: younger women are more likely to be credit constrained than older women and, thus, fully covering the cost of abortion should matter more to them. While this is only a before-and-after exercise, it is illustrative of the change in abortion rates that followed the 2014 policy change among affected women.[37]

I.3 Estimating Wage Premiums (AKM)

In this section we explain our estimation of the sectors' wage premiums in the spirit of Abowd et al. (1999) (AKM). To do so we use a firm-level estimates on tax data spanning the entire economy (men and women) we have from a different project (Dobbin and Zohar, 2020). We then take these estimates in the firm level and collapse them to the sector level to match to the sector in which each woman is working pre- and post-potential birth.

To estimate the firm's wage premium we assume earnings follow a log-linear functional form:

$$w_{i,t} = \alpha_i + \psi_{J(i,t)} + \mathbf{X}_{i,t} \cdot \boldsymbol{\beta} + \epsilon_{i,t}^y \ , \tag{14}$$

where $w_{i,t}$ is log-earnings of individual i at time t, $\alpha_{i,t}$ is the *individual component of earnings*, $\psi_{J(i,t)}$ the *firm component of earnings*, $\mathbf{X}_{i,t}$ a set of time-varying covariates and $\epsilon_{i,t}^y$ an error term.

Next, we show that the data fits this framework well. In this specification, the *individual component* captures the earnings dispersion within the firm, while the *firm component* (firm earnings premium) captures the earnings dispersion across firms. For equation 14, we have:

$$w_{i,t} = \alpha_i + \psi_{J[i,t]} + \mathbf{X}_{i,t} \cdot \boldsymbol{\beta} + \epsilon_{i,t}^y \ ,$$
$$w_{i,t+1} = \alpha_i + \psi_{J[i,t+1]} + \mathbf{X}_{i,t+1} \cdot \boldsymbol{\beta} + \epsilon_{i,t+1}^y \ .$$

Taking first differences:

$$\Delta w_{i,t} - \Delta \mathbf{X}_{i,t} \cdot \boldsymbol{\beta} = \Delta \psi_{J[i,t]} + \Delta \epsilon_{i,t}^y \ .$$

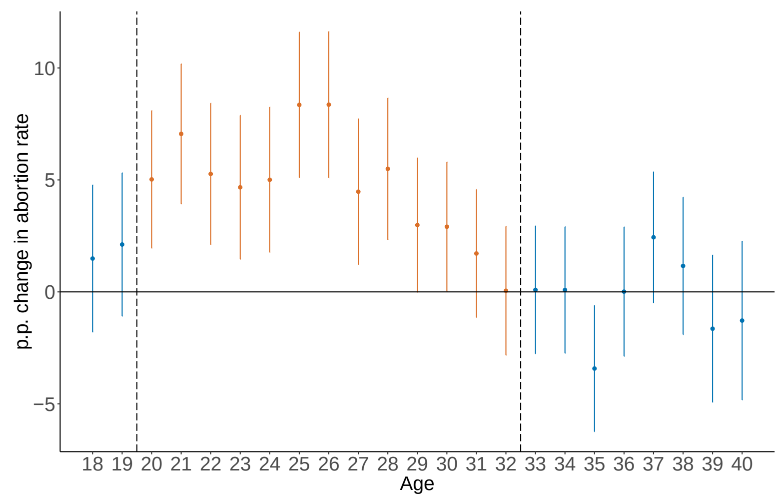
[37] As a placebo test, we conduct an identical exercise using the married women that were not affected by the policy (see Section 2). The results are shown in Figure I3a. The placebo test shows mostly null coefficients for both treated and untreated women. As a second placebo test, we conduct an identical exercise using a two-year bandwidth around 2012 as the cutoff (rather than 2014) (including pregnancies from 2010-2013) and show these results in Figure I3b. The placebo test shows mostly null coefficients for both treated and untreated women, espacially among women aged 18-22 around the 19-year-old cutoff.

In expectation:

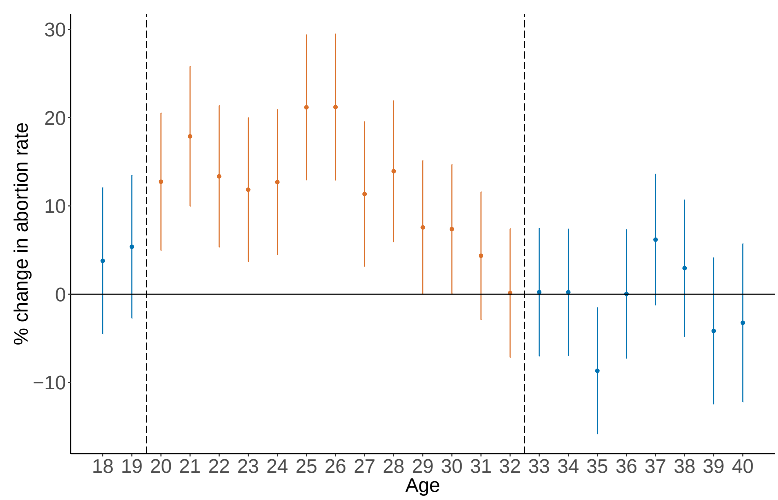
$$\mathbb{E}\left[\Delta w_{i,t} - \Delta \mathbf{X}_{i,t} \cdot \boldsymbol{\beta}\right] = \mathbb{E}\left[\Delta \psi_{J[i,t]}\right] \ .$$

We take this restriction to the data by focusing on job switchers and comparing their earnings change against their firm-effect change. The results are in Figure I4. The solid blue line plots the best-fitting line estimated based on the micro-data. The dashed line plots the 45 degree line. Indeed, a percentage change in earnings is corresponds to a percentage change in wage premiums, as implied by the log-linear structure we use.

Figure I2: Difference in Means by Age (Raw Data)



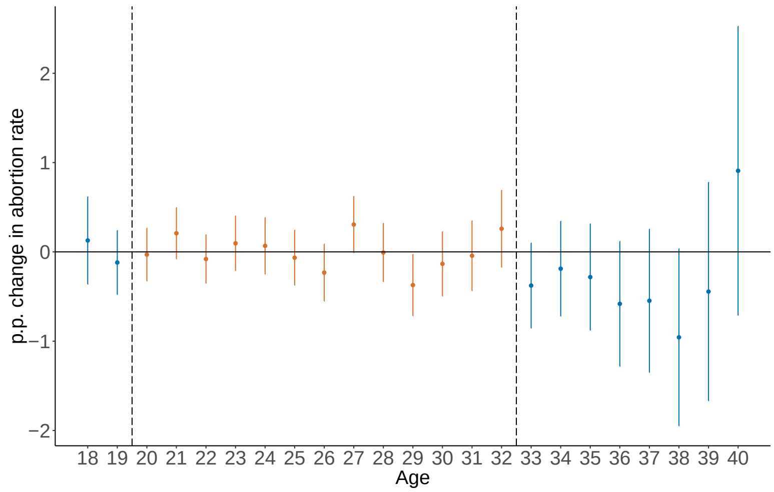
(a) Percentage Point



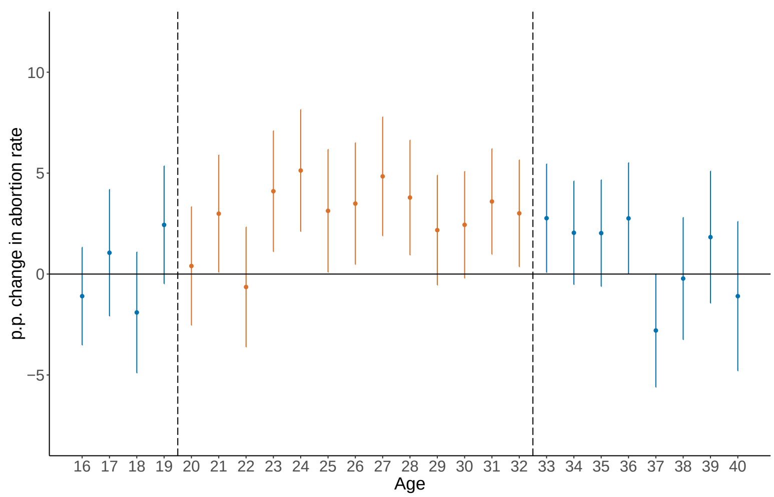
(b) Percentage Change

Notes: This figure presents the before-and-after coefficient on abortion rate by each age group in our sample. Each figure presents the coefficient on $Post$ from the before and after analysis by each age group. The point estimates in Panel (a) can be interpreted as the difference in abortion rate for each age group following the introduction of the 2014 policy. Panel (b) presents the same estimates, scaled as percentage of baseline abortion rate in each age group. The lines are 95% confidence intervals. The ages that were eligible for the 2014 subsidy expansion are indicated in orange, while those ineligible are presented in blue. The dashed vertical lines mark the two age cutoffs for the subsidy change eligibility: 19-years-old and 33-years-old.

Figure I3: Placebo Before & After Analysis by Age (Using Married Women)



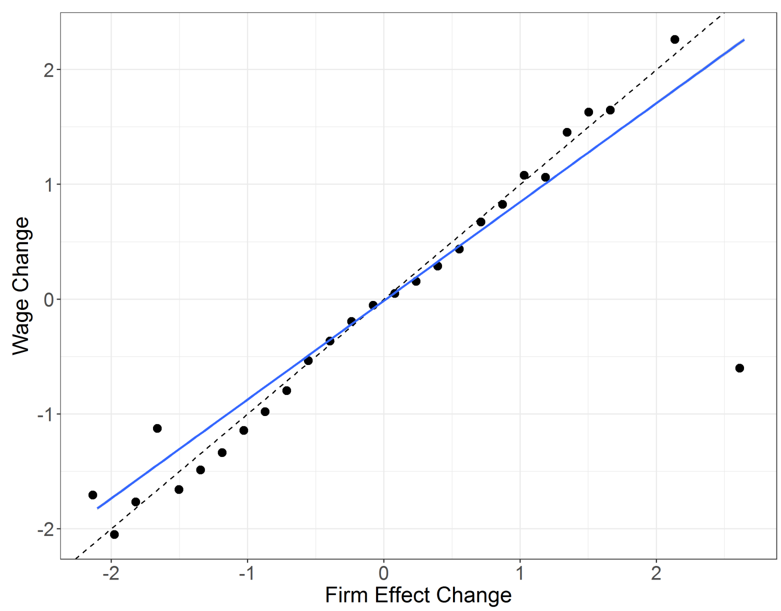
(a) Married Placebo



(b) 2012 Placebo

Notes: This figure presents the coefficient on $Post$ from the before and after placebo analysis by each age group in our sample. Panel (a) shows the placebo using married women (which were not affected by the policy). Panel (b) uses 2010-2013 and 2012 as the year of the (placebo) policy change. Each point estimate can be interpreted as the difference in abortion rate for each age group following the introduction of the 2014 (2012) policy. The lines are 95% confidence intervals. The ages that were eligible for the 2014 subsidy expansion are indicated in orange, while those ineligible are presented in blue. The dashed vertical lines mark the two age cutoffs for the subsidy change eligibility: 19-years-old and 33-years-old.

Figure I4: Earnings Change Corresponds to Firm Fixed Effect Change



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