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

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October 16, 2022

[Link to the latest version]
[Link to the online Appendix]

Abstract

Monetary cost is a fundamental—yet understudied—component of abortion access. In this paper, we study the effects of eliminating the cost of abortion on fertility and women's career outcomes. We focus on a 2014 policy change that expanded eligibility for free abortion in Israel, by making women aged 20-32 eligible, and use unique administrative data that allow us to track abortions, births, employment, earnings, and formal education for the universe of Israeli women from 2009-2016. Using the younger eligibility cutoff, we examine the impact of the policy among young (18-21 years old) unmarried women and show that access to free abortion services increases abortions but does not increase conceptions. The effect is driven by low-income women from religious Jewish backgrounds. This finding suggests that making abortion free increased the privacy of the decision. In the medium-term, the policy delayed parenthood and marriage, increased college enrollment, and shifted employment toward higher-paying and more flexible work arrangements, which suggests a large opportunity cost of undesired parenthood.

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1 Introduction

The legal status of abortion remains a contested and emotionally charged issue around the world. However, the legal right to an abortion does not automatically equate with access. In many settings in which abortion is legal, women must figure out when and where to have the abortion, how to get there, whether they will face stigma for seeking an abortion, and, importantly, how to pay for it. A robust body of evidence now examines the impacts of abortion legalization and access across a number of settings,[1] and although the dimensions of abortion access intersect, monetary cost is a fundamental component that has been understudied relative to other factors. Past studies have shown that interruptions in Medicaid funding cause a reduction in abortion, but neither the mechanisms nor the downstream economic impacts for women have been investigated in those settings (Kane and Staiger, 1996; Levine et al., 1996; Cook et al., 1999; Meier and McFarlane, 1994; Morgan and Parnell, 2002). While many countries liberalized abortion under universal healthcare systems that also covered the cost (e.g., Denmark, Finland, Italy, Spain, and Ireland), many countries and US states are now moving beyond legal rights to abortion and considering new policies that eliminate the monetary cost.[2] Understanding how and why removing the monetary cost of abortion impacts women's reproductive health and socioeconomic outcomes is particularly important to the policy debate.

In this paper, we shed light on these important questions by focusing on the Israeli context. In Israel, abortion has been legal since 1977 and has been free for various groups of women (see Table B1a). However, many women were ineligible for these subsidies and unable to access abortion services due to the $600 co-pay. In response to advocacy efforts by local activists, the Israeli government massively expanded the subsidy in 2014 and made women aged 20-32 eligible for free abortion.

We leverage the 2014 policy change as a natural experiment and use unique administrative data on the universe of individual pregnancies (abortions and births) in Israel from 2009 and 2016 linked to tax data on employment, earnings, and educational enrollment to overcome data and identification limitations and answer critical questions about whether and how abortion access matters for women's lives. Using a difference-in-differences identification strategy, we first examine what happens to abortion utilization when the monetary cost is eliminated.

[1]There are numerous examples from different settings, such as abortion legalization in the US: Angrist and Evans (2000); Akerlof et al. (1996); Donohue and Levitt (2001); Ananat et al. (2007); Donohue et al. (2009); Ananat et al. (2009); Myers (2017); TRAP laws and clinic closures in the US Lindo et al. (2019); Myers and Ladd (2020); Lu and Slusky (2019); Fischer et al. (2018); Venator and Fletcher (2019); Quast et al. (2017); Jones and Pineda-Torres (2021); and variation in abortion laws across Eastern Europe after the fall of communism: Levine and Staiger (2004); Pop-Eleches (2006, 2009, 2010); Malamud et al. (2016).

[2]See, for example, California (Gutierez, 2021), Maryland (Heyward, 2022), and Illinois in the United States, which recently enacted policies to fund abortions; the Australian Capital Region, which recently announced it was removing the cost for surgical and medication abortions (Bladen, 2022); and campaign promises in the Tasmanian elections to create an abortion fund to cover the cost (Denholm, 2018).

We compare a narrow bandwidth of the "newly funded" women aged 20-21 (treated) to "always funded" women aged 18-19 (control), before and after the 2014 policy change. We find that, consistent with the existing literature, increased access to abortion increases abortion utilization.[3] Specifically, the share of abortions out of total pregnancies increased by 3-4.6 percentage points among young, unmarried women, which is robust across specifications.

We then explore the two primary hypotheses that could explain this result: moral hazard and the elimination of financial constraints. Past studies have found that making abortion free creates moral hazard because women reduce their contraceptive use due to the lower cost of abortion, which results in more pregnancies and abortions (Levine and Staiger, 2002; Ananat et al., 2009). In our setting, we find no change in conceptions, which suggests no evidence for moral hazard. On the other hand, a natural explanation for the increase in abortion is that the subsidy allows low-income and disadvantaged women to access legal abortion and avoid undesired births. To test this second mechanism, we split our population into women from low- and high-earning families and find practically no difference in the effect based on family income.

We propose a more nuanced explanation: the role of social stigma and privacy. Israeli activists suggested that prior to the subsidy expansion it was not only young women in their early 20s who struggled to come up with the abortion co-pay, but particularly young women from religious backgrounds.[4] Our data allow us to explicitly test this hypothesis among the Jewish population in Israel, which includes substantial heterogeneity in religiosity. We split our sample by Jewish religiosity and find that the data confirm the anecdotal evidence: The increase in abortion due to the policy is particularly high among women from poor *and* religious families. We interpret this result as the potential channel through which abortion increases: Making abortion free removed a binding financial constraint for poor women from social groups in which abortion is stigmatized. In other words, making abortion free increased the *privacy* of the decision because it eliminated the (financial) need for women to discuss the decision with family or friends.

Finally, using the sharp change in abortion access induced by the policy as an instrument for whether a woman can avoid an undesired birth, we explore downstream effects on future fertility, marriage, education, and employment decisions. We first show that among compliers, having an abortion results in a decrease in parenthood and marriage in the 3 years following conception. We also find an increase in college enrollment. Finally, we find that avoiding an undesired birth results in a lower probability of being in the labor market but that, conditional

[3] See, for example: Akerlof et al. (1996); Ananat et al. (2009); Myers (2017); Lindo et al. (2019); Myers and Ladd (2017); Fischer et al. (2018); Levine and Staiger (2004); Pop-Eleches (2010); Kane and Staiger (1996); Levine et al. (1996); Cook et al. (1999); Bitler and Zavodny (2001).

[4] Sharon Orshalimy, Israeli reproductive justice activist and 2013 Young Leader with Women Deliver, Tel Aviv, Israel, July 2019.

on working, women who avoid an undesired birth are more likely to work part-time and in better-paying sectors (e.g., a public sector instead of a service sector).

Taken together, these results suggest that when abortion is not free, young, pregnant women enter into early undesired parenthood and, possibly, undesired marriages, which can be avoided when the financial constraints on abortion access are removed. Subsequently, by avoiding early undesired parenthood these young women can also avoid taking low-wage jobs that offer few opportunities for advancement. Instead, they can choose jobs more selectively and invest more in their human capital by enrolling in college. The shift toward part-time (but better paying) employment hints at a substitution toward more flexible employment arrangements that allow these women to complete their studies.

Our paper advances the literature in several ways. Abortion is notoriously difficult to study because reliable data on abortion are rare and changes in abortion policy have often come simultaneously with other policy changes that affect fertility decisions. Our unique individual-level data on the universe of abortions and births in Israel, along with exogenous variation in abortion access due to the 2014 expansion of an abortion subsidy, allows us to overcome data and identification limitations in the literature and study not only whether abortion increases when the monetary cost is eliminated, but also why. We revisit the canonical "abortion as insurance" model, which theorizes a moral hazard response to reducing the cost of abortion (Kane and Staiger, 1996; Levine and Staiger, 2002, 2004; Levine, 2007; Levine et al., 1996; Ananat et al., 2009). Given that we find no evidence of moral hazard in this setting, we suggest an alternative explanation: the role of social stigma, financial constraints, and privacy.

We find that the increase in abortion is driven by the population of low-income, highly religious women. Although we cannot test this directly, our interpretation is that eliminating the financial cost enabled low-income women to make the abortion decision in private, without asking friends or family for financial support, which may matter more for women from very religious families. There is a growing body of evidence on the importance of privacy in making reproductive decisions. For example, Myers and Ladd (2020) demonstrate how parental involvement laws, which reduce privacy for minors seeking an abortion in the United States, increase teen births. In two developing country contexts, Ashraf et al. (2014) find that women in Zambia are less likely to seek family planning services if their husbands are involved, while Anukriti et al. (2022) demonstrate how leveraging social networks among women in India can help overcome stigma and social constraints in making family planning decisions. Our unique administrative data on abortion and religiosity allow us to examine the role of privacy from a different angle and show how the abortion funding policy in Israel was primarily used by those who benefited the most from increased privacy: low-income women from very religious Jewish backgrounds. Taken together, this evidence suggests that across many different settings, the privacy costs of accessing reproductive health services—and abortion in particular—can be large,even when they are legal.

Second, our work contributes to the literature on the economic effects of family planning, particularly the "power of the pill" literature that examines state-level variation in the timing of policies that expanded access to oral contraceptives (Goldin and Katz, 2002; Bailey, 2006; Bailey et al., 2012; Ananat and Hungerman, 2012). This body of work suggests that expanding access to oral contraceptives allowed affected cohorts of women to delay entry into parenthood, increase their employment and earnings, and invest in their careers. More recently, Myers (2017) and Lindo et al. (2020) argue that these effects were confounded by simultaneous changes in abortion access. We shed light on this by showing how increasing abortion access, while holding constant contraceptive access and other fertility policies, allowed women to delay early parenthood and marriage in the shortterm, invest in higher education, and take jobs in higher-paying sectors. Also consistent with our findings, González et al. (2022) find that women affected by the legalization of abortion in Spain were more likely to graduate from high school and less likely to marry young or divorce in the longterm.

Our findings also advance a more nascent literature on the economic consequences of being denied an abortion, although we study the converse margin: the positive economic impacts from *expanding* abortion access. Miller et al. (2020) and Foster et al. (2018), use the landmark Turnaway Study[5] and find a large and persistent increase in financial distress and a decrease in employment among women who were denied an abortion. Our findings are conceptually consistent with Miller et al. and Foster et al., although we examine different economic outcomes—namely, early career and human capital investment. Moreover, we are able to document the economic effects for the *entire* population of Israeli women affected by the expanded abortion subsidy.

Finally, our analysis also contributes to the "child penalty" literature, which documents a large and permanent drop in wages for women after they give birth (Kleven et al., 2019b,a; Eckhoff Andresen and Havnes, 2019). We build on this literature by studying the penalty associated with an *undesired* birth. Evidence from the US suggests that women who seek abortions are lower-income, are less likely to have health insurance, and typically are more disadvantaged than the general population (Kavanaugh and Jerman, 2018; Jerman et al., 2016; Steingrimsdottir, 2016). Likewise, the Turnaway Study establishes that women seek abortions primarily for financial or economic reasons (Biggs et al., 2013). Thus, one might expect the child penalty to be larger for undesired births than for births overall. In the Israeli setting, we show how avoiding early undesired parenthood allows poor and religious women to temporarily avoid a child penalty in earnings and invest more in their long-term earnings

[5]The Turnaway Study compares women who received abortions just under the facility's gestational limit (near-limit group) with women who sought but were denied an abortion because they were just beyond the legal gestational limit (turnaway group).

potential by enrolling in college.

The rest of the article is organized as follows. In Section 2 we describe abortion in the Israeli context and provide details of the 2014 policy change that serves as our natural experiment. Section 3 describes the data and sample selection. Section 4 explains the difference-in-differences approach and reports the increase in abortion that occurs in response to the policy. In Section 5, we explore alternative explanations for the policy's effect on abortion and show that, while moral hazard does not explain the result, the increase in abortion occurs primarily in the subpopulation of socially and financially constrained women. Section 6 presents our identification strategy and results that use the 2014 policy as an instrument for having an abortion to examine women's demographic, educational, and labor market outcomes. Section 7 concludes.

2 Israeli Context and 2014 Policy Change

The unique context of abortion in Israel is important for understanding our empirical strategy and the heterogeneity in abortion views by ethnicity and religiosity that allow us to disentangle different mechanisms.

2.1 Abortion in Israel

Abortion has been legal in Israel since 1977, conditional on approval from a committee composed of two medical professionals and a social worker, one of whom must be a woman. All legal abortions in Israel must go through this committee process, including when women opt to have the procedure performed by a private doctor outside of the public healthcare system. Although the committee process may seem obstructive, the committee itself effectively serves as a rubber stamp, and in practice many women who would not strictly be approved according to the criteria are "coached" through the process in order to get approved (Oberman, 2020). Consequently, almost all applications are approved; our data show that 99% of applications are approved and 97% are acted upon. See Appendix B.1 for more details on the abortion committee in Israel.

The committee approves an abortion if *at least* one of the following conditions is satisfied: (1) the woman is under 18 or over 40 years of age; (2) the pregnancy is out-of-marriage; (3) the pregnancy is the result of an illegal act (rape or incest); (4) the pregnancy risks the life or the health of the woman; or (5) the fetus suffers from congenital disorders. These criteria, along with approval shares by each criterion, are shown in Table B1a. By definition, all unmarried women automatically meet the out-of-marriage criterion and are automatically approved, whereas married women must either report that their pregnancy was out-of-marriage (e.g., the result of infidelity) or meet one of the other criterion for approval.

After receiving the committee's approval, women have to pay an out-of-pocket (co-pay) cost for the abortion, in contrast to the majority of healthcare services in Israel. The cost of an abortion varies from NIS 2,100 to 3,500 (USD 600 - 1,000), depending on the procedure, which is determined by the stage of the pregnancy. A woman can choose to have an abortion with a private doctor after receiving approval from the committee, which is quicker but also more expensive. Among private physicians, the cost of an abortion can be as high as 8,000 NIS (USD 2,200). Putting these figures in context, the average monthly earnings for women in Israel in 2014 were NIS 7,666 (USD 2,270), and the average household monthly income was NIS 15,427 (USD 4,565). For the young women we focus on in our analysis, the average monthly earnings were NIS 2,109 (USD 624), conditional on working that month.

The high approval rates could indicate the existence of an illegal market for women whose abortion requests otherwise would not be approved by the committee. Anecdotal evidence suggests that illegal abortion does exist in Israel but is dominated by "high-end" (and high-cost) providers who operate outside the committee process, rather than the unsafe conditions that are more characteristic of illegal abortion in other settings (Newman, 2017; Oberman, 2020).[6]

Given the high accessibility, quality, and low—or in some cases free—cost of legal abortion, the incentives to have an illegal abortion in Israel are low, particularly since a woman can have the abortion performed by a doctor of her choosing and avoid the wait times in the public sector by simply having the abortion procedure outside the public healthcare system after receiving committee approval. However, some women may opt out of the committee process to avoid the bureaucracy or perceived judgment of sitting in front of a committee (Oberman, 2020).[7] Although anecdotal evidence suggests that incentives to obtain an abortion outside the legal system are low, especially among low-income women, our data on abortion come from the official abortion committee; thus we do not capture any illegal abortions in our data. The potential existence of an illegal market would complicate the interpretation of our results, because any change in abortion could be due to shifts from the illegal to the legal abortion market. We address this concern directly in Section 5.3.

[6]An article in the Israeli newspaper *Seven-Days* ('Shivaa Yamim') suggested that there are 15,000 illegal abortions a year in Israel (Newman, 2017). However, after contacting the reporter and organization quoted in the article, we found no evidence for the original source of the data or anyone who could confirm the number.

[7]Lastly, the ability to order medication abortion pills online could present an alternative way to evade the committee process to obtain an abortion. While we cannot rule out this possibility, abortion pills for purchase online do not appear to be widely available in Israel, which has also been confirmed by abortion advocates in Israel.

2.2 Baseline Heterogeneity in Abortion

Figure 1a shows the variation in abortion across income levels in Israel prior to the 2014 policy change. We observe an increasing gradient with income, in which women from higher-earning households are more likely to have an abortion. In 2013, the Israeli economic newspaper *Calcalist* ran a survey that asked, "Could you raise NIS 8,000 within a month if you had to?" Sixty-seven percent of unmarried Israeli women aged 18-24 stated they would not be able to or would require family support (Peled, 2013). Overall, these factors imply that the co-pay might be a binding financial constraint for young and lower-earning Israeli women.

Because of its cultural and religious heterogeneity, Israel is an interesting setting to study abortion. Israel is composed of 75% Jews, 18.6% Arab-Muslims, 2% Arab-Christians, and 4.4% affiliated with other religious groups or non-affiliated (see Appendix B.2 for more information about abortion norms in Israel). Figure 1c demonstrates the substantial variation in the baseline probability of abortion, which might suggest differing abortion views, sexual behavior, and use of contraception across groups. Furthermore, Figure B4 shows large variation in contraceptive use by religiosity. The Jewish population consists of a wide mixture of religiosity levels, ranging from secular Jews (45%) to traditional Jews (25%), religious Jews (16%), and Orthodox Jews (14%) (Central Bureau of Statistics (Israel), 2018). Broadly speaking, religiosity is highly correlated with both fertility and opposition to abortion: The secular-Jewish population generally supports abortion and has relatively low fertility rates; in contrast, the Orthodox population is opposed to abortion and has very high fertility rates. The Israeli-Arab population is mostly religious and regards abortion as taboo; in general, Islam opposes abortion, except when the fetus's health is compromised (Shapiro, 2014). The Muslim population consists of 11% secular, 57% traditional, and 31% religious (Central Bureau of Statistics (Israel), 2018). Our administrative data allow us to directly observe the religiosity of the Jewish population (see Section 3.1.2), which enables us to leverage the variation in Jewish religiosity and abortion views to unpack the mechanisms that drive the change in abortion in response to the 2014 policy.

2.3 The 2014 Natural Experiment: Eliminating the Cost of Abortion

Prior to 2014, women aged 19 and under could obtain an abortion free of charge.[8] However, since co-pays are typically rare (and small) in the Israeli healthcare system, women aged 20 or above were often surprised to learn they needed to pay between \$600 and \$1000 for an abortion upon arriving at the clinic. According to Dr. Hedva Eyal, head of the Haifa Women's Coalition—a women's rights organization that also helps young women access reproductive

[8]Additionally, abortion has been free since 1977 for women aged 17 and under, if the pregnancy results from rape or incest, or if there is a medical risk for the woman or fetus (see Table B1a, column 3).

health services—women from lower-earning families frequently struggled to come up with enough money to cover the co-pay.[9] Religious women in particular faced difficulties because they could not ask friends or family members for financial support for an abortion.[10]

In January 2014, the Israeli government expanded the subsidy from the previous cutoff of 19 years of age to include all women up to 32 years of age, (see Table B1b). The funding coverage also includes the cost of the committee itself (Kelner, 2013). Thus, eligible women do not have to pay any costs for the abortion. The government decided to use age as a proxy for income and, due to budget constraints, capped the coverage at 32 years of age (Amsterdamski et al., 29.04.21). Thus, after the 2014 policy change all women in Israel up to the age of 32 could obtain an abortion free of charge. The 2014 policy only changed the cost; women still had to go through the same committee process to obtain an abortion. To the best of our knowledge, no other reproductive health, family, or income policies in Israeli change discontinuously at 19 or 32 years of age. We use this 2014 policy change as a natural experiment to study the impacts of providing free abortion, focusing on the younger age cutoff of 19 years, as we describe below.

3 Data and Sample Definition

We use administrative data on the universe of abortions and births in Israel, detailed tax records, and education registry data from the Central Bureau of Statistics (CBS) of Israel. We combine the four data components to create an individual-level panel of pregnancies (abortions and births) linked to detailed monthly-level tax data on women's earnings and education registry records. Here we define the variables we use and describe how we construct our analysis sample.

3.1 Data and Variable Construction

3.1.1 Pregnancies, Abortions, and Births

Our administrative data on abortion, which come from the abortion committee, include every woman who applied to the committee between 2009 and 2016 and provide information on the woman's pregnancy (such as the week of pregnancy at the time of application). To identify all live births registered in Israel as well as demographic information about the women at the time of conception (including age, religion, ethnicity, marital status, education, and parents'

[9]Conversation between Tom Zohar and Hedva Eyal, President of the Haifa Women's Coalition, Tel Aviv, Israel, April 2020.

[10]Sharon Orshalimy, Israeli reproductive justice activist and 2013 Young Leader with Women Deliver, Tel Aviv, Israel, July 2019.

identifiers), we use 2016 civil registry data. Combined, the abortion committee data and the civil registry data allow us to identify the universe of recorded conceptions in Israel between January 2009 and March 2016 with the exception of pregnancies terminated without permission of the committee (illegal abortions) and miscarriages that occur early in pregnancy.[11] An important advantage of these unique data is that they allow us to examine individual-level births and abortions, rather than rely on aggregated abortion rates.[12]

3.1.2 Religiosity and Ethnicity

To classify religion and ethnicity for the women in our sample, we rely on data from the census and the Ministry of Education. Ethnicity is reported when citizens are issued their identification card and is recorded in the census data. We define religiosity based on the type of school the woman attended. Israel has three types of schools: secular ("mamlachti"), religious ("mamlachti-dati"), and Orthodox. For statistical power, we aggregate both the religious and Orthodox into a single category. Women may change their religiosity before or after completing their schooling or may not be as religious as the school they attend, either of which could complicate this classification. On the other hand, the choice of school is a good proxy for the religiosity of the woman's *parents* and her broader social network, which is highly relevant for thinking about the role of social stigma and privacy of the abortion decision among younger women.

3.1.3 Labor Force Participation, Earnings, and Education

To identify labor market participation and earnings, we use tax data composed of a monthly panel of labor market employment, earnings, and sector identifiers from 2005 to 2018. We also use data from the education registry spanning 2005-2018 that include information on whether and when a woman enrolled in higher education. To examine the downstream effects of the 2014 policy on women's labor-market outcomes, we construct several variables. Yearly earnings are defined as the sum of earnings across all firms a woman worked for and earnings from self-employment in a given calendar year. Following Abowd et al. (1999), we estimate sector-level wage premiums by running a log-wage regression on individual and firm fixed

[11] Third-trimester abortions (defined as an abortion after 24 weeks of gestation) are captured in the abortion committee data; however, they are required to go through a special committee and were fully subsidized during the span of our data. Overall, these are rare (approximately 250 per year among the entire population of Israel) and we exclude them from our analysis.

[12] Throughout our analysis, our primary sample consists of the universe of conceptions in Israel (that is, the total number of legal abortions and live births). Thus when we calculate mean abortion, it should be considered an abortion ratio: the share of pregnancies that end in abortion. This is in contrast to the more commonly used abortion rate, which refers to the number of abortions per 1,000 women of a given age. In cases in which we present the abortion rate, it will be explicitly noted.

effects and averaging the wage premiums of all firms within a given sector (see further details in Appendix G). On the extensive margin, we create several variables. Self-employment and employment in a firm are directly reported in the tax records. We classify a woman as in the labor force ("working") if she is either self-employed or hired by a specific firm in a given year. We also construct a proxy for part-time employment, in which part-time employment is defined as earning below the 2011 minimum full-time monthly earnings defined by law (3,890 NIS/month or USD $1,090/month), and 2011 is the baseline year for our inflation correction. This part-time employment measure is defined for the total population of unmarried women agged 18-21 who conceived.

3.1.4 Household Socioeconomic Status

Given our focus on young women, economic resources and family religiosity are central to the mechanisms we examine in Section 5. To identify household economic resources, we use data on father's earnings and classify each woman-year observation into two groups: below and above median father's earnings.[13]

3.2 Sample Definition

Our primary analytic sample consists of unmarried 18- to 21-year-old women who conceived between 2009 and 2016. We restrict our analysis to *unmarried women* for two reasons: (1) all unmarried women are automatically approved by the committee for an abortion and (2) the structure of the 2014 policy change. As described in Section 2.1, since pregnancies that occur out-of-marriage are automatically approved for a legal abortion by the committee, unmarried women are automatically approved for an abortion (see Table B1a). We describe in Appendix B.1 how married women can get around the committee criteria. However, this practice raises serious concerns about selection; not all married women may be willing to go through the cumbersome process of obtaining approval under a different criterion or perhaps lying in order to obtain approval for the out-of-marriage criterion. Thus, the only way to ensure comparability of the women who had abortions and those who gave birth is to restrict the sample to all unmarried women, in which marital status is identified at the month of conception.

Second, given the prior criteria for government funding of abortion, the only population for whom the funding coverage changed in 2014 are women with an out-of-marriage pregnancy, which further motivates the restriction to unmarried women (Table B1a). Figure 1b shows that abortion is rare among married women in Israel: 71.5% of pregnancies among young, unmarried women are aborted and 0.75% of pregnancies are aborted among married women.

[13]Father's earnings are more predictive of a child's future ranking than household or mother's earnings (see Table A3).

Importantly, we restrict our analysis to women who are unmarried at the time of conception, and thus our sample is not affected by the endogenous decision to get married after becoming pregnant. This also allows us to assess the policy's downstream impact on marriage following conception (see Section E.2.1).

We further restrict our analysis to the population of unmarried women who are *18 to 21 years old*. For both empirical and conceptual reasons, we take a bandwidth of two years above and below the younger age cutoff (19 years old) for the subsidy. Empirically, we focus on a small bandwidth around the age cutoff for higher statistical power and bias minimization. On the one hand, we gain power by focusing on the age group most affected by the policy, but we lose power due to the smaller sample size. While we could extend the sample to include older women, thus increasing our sample size, the further we go from the cutoff, the greater the bias introduced to our estimates (Appendix Section C discusses the parallel trends in more detail and presents various forms of evidence for each of these samples). Conceptually, we focus on young, unmarried women because they are more likely to have abortions. Among unmarried 18- to 21-year-olds, 67.2% of pregnancies are terminated (Figure 1b). While this may sound high, it is important to remember that abortion tends to be higher among young women. In our data, among all 18- to 21-year-olds (married and unmarried) 14.5% of pregnancies are terminated, which is about half the number for women under the age of 20 in the United States, which was 29% in 2013 (Kost et al., 2017).[14]

Focusing on young women is also important for studying the effects of abortion access on education and labor market outcomes. This age (18-21) represents a critical time for women—particularly in the Israeli context—to invest in human capital such as higher education. In Israel, about 57% of Jewish women serve in the military between their 18th and 20th birthdays, which delays entrance to higher education. In our data, only 5.6% of women aged 18-21 are enrolled in higher education, while 18.2% of women aged 22-24 do. A potential concern with this age restriction is that the control group is 18-19 years old and serving in the military, while our treated group (20-21 years old) are not.[15] Military service may pose a threat to our identification if it affects women's fertility decisions. Age-based differences, such as military service, are precisely why we take two differences and are addressed by our difference-in-differences econometric strategy.[16]

[14] For further comparison, 10% of all pregnancies in Israel are terminated, which is relatively low compared with global rates (see Figure B2). Twenty-five percent of pregnancies are aborted worldwide, while in Europe the share is 26% and in North America 16% (Guttmacher, 2018).

[15] In practice, some women may end up serving in the military for a couple of months after their 20th birthday, which could potentially contaminate our treatment. To account for that we run a robustness analysis in which we drop women aged 20 years old and include women aged 21-22 as the treated group. We find very similar results (Figure C4).

[16] Two additional facts regarding military service in Israel may help to alleviate concerns. First, most Israeli soldiers are stationed in "open-bases," meaning their service allows them to return home every day like a standard job. Women who serve in "closed-bases" are still based in Israel and can return home on weekends. Second, only

Finally, we focus our analysis on *first* conceptions occurring between January 2009 and March 2016. We focus on first conceptions to address composition concerns related to marriage. For example, an unmarried 20-year-old who gives birth in 2013 and subsequently got married, would mechanically be excluded from the pregnancy sample in subsequent years, creating a composition change of the panel of all conceptions. Focusing only on first conceptions, frees our inference from such composition concerns. As a result, our sample is a repeated cross-section of first conceptions. We chose 2009 as the starting year because, prior to 2009, 19-year-olds were not universally funded. In 2009, the Israeli government expanded the abortion subsidy to include all women up to the age of 19 (previously, only up to the age of 18 was covered).[17] Therefore, starting the sample period before 2009 could contaminate the treatment since we cannot identify who serves in the military in our data. To avoid contaminating the treatment, we restrict the sample to conceptions from January 2009 onward, at which point the government already covered all 19-year-olds, regardless of their status in the military.[18] We restrict our analysis to conceptions up until March 2016 for two reasons. First, the latest live births we observe occurred in December 2016, and thus we observe conceptions only until March 2016. An additional reason is because in March 2016, Israel expanded the permitted use of medication abortion, which could complicate the interpretation of our findings (Gal, 2016). Ultimately, after restricting our sample to unmarried women aged 18-21, our sample is composed of 24,564 pregnancies across 20,621 women (Table A1).

4 Effect of Subsidy on Abortion Utilization

4.1 Empirical Strategy

To identify the effect of the 2014 policy on abortion, we use a difference-in-differences (DiD) strategy that leverages the timing of the policy change (2014) and the age cutoff (19 years, highlighted in Table B1b). Specifically, we estimate the following DiD model on a repeated cross-section of all first conceptions that occurred in Israel between January 2009 and March 2016, based on the time (month-year) of conception:

20% of religious Jewish women serve in the military. This increases our confidence in our results, because religious Jewish women are the main population that drives our results (as we show in Section 5.2).

[17] We separately test for an effect of the 2009 change in funding coverage for 19-year-olds and find that the policy had a negligible and insignificant effect on abortion. It is important to note that 19 year olds are still serving in the military at this age and the military covers the cost of all medical procedures, including abortion, thus making the 2009 policy change redundant.

[18] Alternatively, we could include earlier years and use a staggered difference-in-difference in which the treatment status of 19 years old changes over time. This robustness test produced results very similar to our main effect. Thus, we chose to start from 2009 due to the confusing nature of the military coverage, which renders interpretation of the staggered difference-in-differences results more complex.

$$abort_{it} = \delta Post_t \times T_i + \gamma_{a_i} + \gamma_{y_t} + \gamma_{m_t} + X_i'\gamma_i + \epsilon_{it}. \quad (1)$$

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's being in effect ($\mathbb{1}\{t \geq \text{Dec-2013}\}$)[19] and T_i indicates that woman *i* is eligible for the subsidy ($\mathbb{1}\{20 \leq age \leq 21\}$).[20] The coefficient on the interaction between $Post$ and T is the standard DiD effect (δ), which can be interpreted as a percentage-point change in the probability of abortion due to the policy. We include age at conception fixed effects (γ_{a_i}) to control for common characteristics at different ages that affect fertility choices, while year of conception fixed effects (γ_{y_t}) and month fixed effects (γ_{m_t}) are used to control for age-invariant time trends and seasonality that affect abortion and fertility. Lastly, X_i represents a set of pre-pregnancy controls that are known to impact fertility decisions, including ethnicity, religiosity, education, and family's earnings (Kearney and Levine, 2012; Eckstein et al., 2019; Almond et al., 2019). We control for these nonparametrically in some specifications as a robustness test. Standard errors are clustered at the age-at-conception level.

Our DiD approach assumes parallel trends: women eligible for the subsidy would have experienced changes in abortion over time similar to those of ineligible women in the absence of the 2014 subsidy. To assess the validity of this assumption, in Figure we plot 2 mean abortion by group and year and the corresponding 95% confidence intervals around each point estimate.Fitted lines represent the estimated linear pre-trend for each group (and extrapolated post-policy). Intuitively, this serves as a visual illustration of the treatment effect, taking into account differential pre-trends (a la Agha and Zeltzer (2022)). In the pre-period, the trends in abortion for the treated (20-21 year olds) and untreated (18-19 year olds) are quite parallel, although there is a narrowing of the difference for the years closer to 2014. Nevertheless, in 2014 we observe a significant increase in abortion beyond the pre-trends for the treated 20- to 21-year olds and no substantive difference relative to pre-trends for the untreated 18-to 19-year olds, as would be expected if the subsidy expansion affected the probability of abortion among newly eligible women. Appendix C presents additional parallel trends assessments for the 18-21 year old sample, as well as for two alternative populations: 30-35 year olds, where the 33-year-old age cutoff is used to determine treatment, and the full sample of women aged

[19] The policy 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. Therefore, we move the treatment timing a month back to account for pregnancies that were conceived in December 2013, but may not have been discovered until January 2014, when the policy was already in place, and thus should be considered treated.

[20] A potential concern is that the policy may also induce changes in the fertility decisions of 18-19 year olds if, by backward induction, they know that they will continue to be eligible for a free abortion after the age of 19, which might be somewhat implausible in practice. Indeed, we also see no evidences for a change in abortion utilization among the always eligible (18-19 years; see Figure 3)

18-40 with both age cutoffs (19 years old and 33 years old) used to define treatment. Although the pre-trends are quite parallel for the population of the treated group (30 to 32 years of age) and control group (33 to 35 years of age), there also does not seem to be a policy effect (Figure C1 Panel a). In contrast, when we use the entire population and both age cutoffs, there is a strong policy effect and a very clear violation of the parallel trends assumption (Figure C1 Panel b). Finally, we also present a generalized DiD, which interacts treatment with individual year fixed effects to test for pre-trends, shown in Figure C2, and find no statistical difference in the probability of abortion between treated and control women before the policy change. Thus, for the remainder of the analysis, we focus on the population of 18-21 year olds but include comparable analyses for these other populations in the appendices as appropriate.

4.2 Abortion Utilization Results

We find that removing the abortion cost increased the probability of abortion by 3-4.6 percentage points relative to younger women who were already subsidized (Table 1). This is equivalent to an increase of approximately 4.5%-7%, compared with the baseline abortion ratio of 66% among unmarried women aged 18-21 who conceived. The 3-4.6 percentage-point effect is the *average* effect across all three post-policy years. The initial increase in 2014 doubled in 2015 and 2016 (Figure C2), which is consistent with a lag in awareness of the policy.[21] While our primary analysis focuses on a narrow bandwidth around the younger age cutoff (19 years old), women up to the age of 32 also became eligible. A simple first-differences exercise by age provides suggestive evidence that the policy resulted in a 4-8 percentage-point increase in the probability of abortion among women aged 20-27, before tapering off for older women closer to the 32-year-old cutoff (Figure 3).

The literature on the effects of changes in financial access on abortion is limited, particularly in settings outside the United States. The magnitude of the effect we find among 18-21 year olds (4.5%-7% increase) is somewhat lower than the 17%-68% reduction in abortion others have found using fluctuations in state Medicaid funding in various US states (Cook et al., 1999; Meier and McFarlane, 1994; Morgan and Parnell, 2002); however, when we turn to a more economically disadvantaged population that may more closely resemble the US Medicaid-eligible population in Section 5, we see similar magnitudes.

Our baseline specification follows Equation 1. Estimating it without controls ("DiD"), we find a 4.6-percentage-point increase in the probability of abortion. However, given the concerns regarding differential pre-trends discussed above, we estimate three alternative specifications and find similar results across all three (columns 2-4 in Table 1). When we include

[21] Appendix Section B.2 discusses this further and presents an analysis of Google search trends for the Hebrew word for abortion, "hapala").

pre-pregnancy controls, we find a 3.2-percentage-point increase in the probability of abortion ("DiD + controls" in Table 1). Second, to address the differential time trends more directly, we run a specification following Agha and Zeltzer (2022) in which we first residualize the abortion outcome on separate pre-trends for the control and treated groups and then run the standard DiD (Equation 1) on the residualized abortion (see Appendix C for more details on this approach). This specification results in a 3-percentage-point effect size (third column of Table 1) and is statistically indistinguishable from the "DiD + controls" specification.[22]

Finally, a potential concern with our DiD analysis is that other unobserved factors unrelated to the 2014 policy might have differentially affected the abortion decisions of 20-21 year olds relative to 18-19 year olds. To address this concern, we estimate a triple differences approach, using married women aged 18-21 as the third difference. The magnitude (3.9 percentage points) and significance of the effect in the triple difference (column 4) are statistically indistinguishable from any other specification in Table 1, and thus serves as stronger evidence for the exogeneity of the policy. Given the stability of the results across these three specifications, we focus on the controlled DiD as our preferred specification (due to its simplicity). For the sake of completeness, Figure A2 presents these estimates while varying both the specification (DiD, DiD + controls, DDD, LTT) and the population group (18-21, 30-35, 16-40).

5 Potential Channels

In the previous section, we demonstrated that providing abortion free of charge increases abortion. It may be counterintuitive that eliminating such a small cost (the co-pay for the abortion) relative to the cost of raising a child would result in such a large effect.[23] In this section we explore potential underlying mechanisms including moral hazard, privacy, shifts in abortion views, and substitution from the illegal market.

5.1 Effect on Conceptions: Test for Moral Hazard

It has been widely shown that reducing the cost of abortion (monetary, physical, or psychological) increases abortion (Kane and Staiger, 1996; Levine and Staiger, 2002, 2004; Levine, 2007; Ananat et al., 2009). Within the economics literature, the canonical "abortion as insurance" model predicts that the option value of cheaper abortion increases risky behavior at the time of the contraception decision (i.e., moral hazard). In this model, a woman first makes a

[22]As an additional test, we implement the "Honest DiD" approach (Rambachan and Roth, 2020) and find our results are robust to allowing for violations of parallel trends up to 40% of the maximum possible violation in the pre-treatment period (see Figure C3).

[23]Note that child-rearing in Israel is orders of magnitude less expensive than in the United States. For example, both education and healthcare are public and universal in Israel (see Appendix B.3 for more details).

decision about contraception intensity, which implies that an unplanned conception will happen with some probability (see Decisions I and II in Figure A1 and the full model in Appendix D.1)

Based on this model, a reduction in the abortion cost translates by backward induction into less contraceptive use, resulting in more conceptions. We test for an increase in conceptions by constructing a balanced panel of *all* unmarried women aged 18-21 in the country (not only those who conceived), and test whether the policy impacted the probability of conception. We use the same empirical design presented in Section 4 and present results in Figure 4. We find a small and insignificant effect across a range of specifications, which suggests no evidence of moral hazard in our setting.[24]

The use of emergency contraception could complicate our test for moral hazard, because we only observe conceptions that end in abortion or a live birth in our data and are unable to observe any contraceptive use, including emergency contraception.[25] If the change in abortion policy led to an increase in risky sexual behavior, these potential pregnancies could have been prevented using emergency contraception. Nevertheless, if women are paying out of pocket for emergency contraception, it is unclear whether moral hazard is the correct interpretation. Nonetheless, if this were occurring it would represent a change in sexual behavior that undermines our test.

The lack of evidence for moral hazard in this setting differs from much of the economic literature on abortion. However, this literature predominantly studies changes in US laws, and thus a moral hazard response may be highly context dependent. Levine and Staiger (2004) review a range of changes in abortion policies in Eastern Europe in the late 1980s and early 1990s and conclude that moderate policy changes (e.g., a shift from abortion available only to those with medical problems to abortion available on demand) result in moral hazard, while large changes, such as legalization, did not result in moral hazard. Ananat et al. (2009) examine abortion legalization in the US using state and time variation in abortion laws and find a bigger increase in abortion relative to the decrease in births, which suggests that moral hazard is present in response to a large policy change, such as legalization, in the US context. In contrast, an analysis of abortion legalization in Mexico City finds no changes in sexual behavior, contraceptive use, or contraceptive knowledge and concludes the increase in abortion is driven by increased access, rather than moral hazard (Clarke and Mühlrad, 2016). Our finding suggests no evidence of moral hazard in a context in which abortion is already legal

[24]A similar exercise in levels shows a *decrease* in births, reinforcing the lack of evidence for moral hazard (Figure A3).

[25]Emergency contraception, or Plan B, has been available in Israel since 2002 (Efrati, 2019). It is available over the counter in pharmacies and public health clinics—which means that a prescription from a doctor is not required—but is not on the shelves and must be requested from a pharmacist (Joanne Zack Pakes et al., 2015)

and a more moderate policy change, providing abortion free of charge, was implemented.[26]

5.2 The Role of Financial Constraints and Privacy

Having found no evidence of moral hazard, we test an alternative explanation: Free access to abortion removed financial constraints that prevented women from having wanted abortions. The young age of the women in our sample (18-21) implies that they are largely dependent on their family's financial resources. To proxy for family resources we split our population into two groups based on their father's income: women from low-earning and high-earning families. However, we find practically no difference in the effect of the policy among women from low-earning families relative to higher-earning families, although the coefficients are both insignificant and noisy (see Figure A4a).

Therefore, we propose a more nuanced explanation, motivated by the Haifa Women's Coalition's experience: Women from religious backgrounds in particular struggled to pay for the abortion (Orshilamy and Zohar, 2019). We hypothesize that the *marginal* abortion decision is impacted by a combination of social views and financial constraints (henceforth "social and financial constraints"). In other words, for lower-income women from religious backgrounds, asking friends or family to help pay for an abortion may not be an option;[27] thus, when abortion is provided for free, these constraints are relaxed and young women can make the decision in private. This theory is supported by the descriptive evidence presented in Section 2, which shows that baseline abortion is highest among women from higher-earning households and more secular backgrounds and lowest among women from more religious backgrounds and lower-income households (Figure 1a and 1c). Additionally, when we split the sample according to the same ethnic groups and estimate Equation 1, we see a large and statistically significant effect among religious Jewish women and a similarly large, but noisy effect among Arab women, who are generally more traditional and religious (Figure A4b), which provides further support for this hypothesis.

We extend the "abortion as insurance" model, presented in Figure A1, to consider these social and financial constraints (Appendix D). The updated model implies two testable hypotheses: First, baseline abortion should be the lowest among more socially and financially constrained women; second, the effect of the policy, which removed those constraints, should be higher for women who are more socially and financially constrained.

[26] As we show in Section 5.2, the effect is driven by the population of low-income religious women. We conduct the same test for moral hazard within this population and again find no evidence of moral hazard.

[27] As noted in Section 2, the explicit motivation for expanding the subsidy was to prevent cost from being a barrier for low-income women. In a survey conducted in 2013, 67% of unmarried Israeli women aged 18-24 stated they could not (or would need family support) to raise 8,000 NIS within a month, which suggests that financial constraints, or at least perceived constraints, are binding at these cost levels for young Israeli women.

To illustrate these hypotheses, consider the two-by-two table in Figure 5a, in which we split our population across two dimensions: social views about abortions and financial constraints. A woman who is financially unconstrained and comes from a social background that accepts abortion (top left) faces no barriers to obtaining an abortion. A woman who belongs to the same accepting social group but is financially constrained (bottom left) faces a credit constraint barrier CC to paying the co-pay. Similarly, a woman who is financially unconstrained and belongs to a social group that finds abortion unacceptable (top right) will bear only the cost of the social unacceptability of the abortion SU (which could also include personal opposition to abortion). Finally, a woman who is from the same social group but is financially constrained (bottom right) will face both credit constraints and the social cost $CC \times SU$.

To test for evidence of this mechanism, we again split the sample into high- and low-SES groups according to father's income. We then proxy for social costs using religiosity in the Jewish population because we can directly observe religiosity in the data: We classify secular Jews as accepting of abortion and religious Jews as not accepting of abortion (Figure 5b).[28] As shown in Figure 5b, the baseline abortion ratio follows the logic in Figure 5a: The highest abortion ratio occurs among financially unconstrained women from secular backgrounds, while the lowest occurs among the socially and financially constrained women who face both $CC \times SU$ constraints. This observation is consistent with the latent cost of abortion described in Ananat et al. (2009).

Our second hypothesis is that the effect of the 2014 policy, which removed those constraints, should follow the opposite of the pattern in Figure 5b. To test this, we split our sample of unmarried 18-21 year olds into the same four groups and estimate Equation 1 within each of them. Figure 5d presents the estimates of the effect in percentage change relative to the baseline (see Figure 5c for the effect in percentage points). We can see that the effect of the policy on abortion follows the logic presented in Figure 5a: the highest (and only significant) effect is concentrated among poor, religious Jewish women. The magnitude of the effect is also quite large: among the poor, religious women, removing the cost of abortion increased abortion by 13.4 percentage points, which is equivalent to a 25.1% increase relative to the baseline mean for this population. The magnitude of this effect is in line with the 17-68% reduction in abortion in response to reductions in funding access for abortion that others have found using fluctuations in state Medicaid funding (Cook et al., 1999; Meier and McFarlane, 1994; Morgan and Parnell, 2002). One concern is that the poor religious population is poorer than the poor secular population and, thus, faces greater financial constraints. Figure A5 suggests

[28]We exclude the Israeli-Arab population for this exercise in order to make cleaner comparisons. While most of the Arab population is traditional and religious, religiosity is not directly reported in our data. Additionally, Jewish and Arab populations differ in terms of culture and religion. By focusing only on the Jewish population we can minimize the chance that the differences we observe are driven by cultural differences. See Section 3.1.2 for more details on the classification of religiosity.

otherwise: Father's earnings are similar across both groups (Panels c and d).[29]

These results suggest that the combined relaxation of social and financial constraints is an important driver of the increase in abortion in response to the policy change ($CC \times SU$ constraints).[30] This finding differs from that of Ananat et al. (2009), who find that legalizing abortion in the US had a larger impact on abortion in liberal states than in conservative states. They ascribe the difference to the latent cost of an abortion, which they frame as a personal, moral objection to abortion. Thus, conservative women did not utilize abortion services even after legalization because they opposed abortion.

One explanation for the differences between the Ananat et al. (2009) results and ours lies in the framing of the latent cost of abortion and the type of policy change (price reduction vs. free). Consider the latent cost to be composed of two components: a personal moral objection to abortion and a social cost. While both religious women in Israel and more conservative women in the US may have a personal moral objection to abortion,[31] the social component of the latent cost did not change with legalization in the US. Although abortion legalization in the US reduced the cost of an abortion (via travel costs), women were still responsible for paying for the full price of the abortion, which may have been out of reach for many low-income women. Thus financially constrained women in conservative US states still had to seek financial support to have an abortion; consequently, the abortion decision was not fully private, which may have resulted in lower utilization of abortion services after legalization. But in the Israeli case, the 2014 policy enabled women who could not afford an abortion to avoid asking for financial help—which we interpret as increasing the *privacy* of the decision—and helped them avoid the social cost.[32] Although this population may seem very specific to the Israeli setting, our finding on the importance of privacy in decision-making may generalize to low-income, conservative, or religious women in other settings.[33]

[29]Another explanation of these results is that military service is confounding this heterogeneous effect since 60% of secular women serve while only 20% of religious women do. Specifically, some women are discharged around age 20, which might attenuate the results for them. To test this, we ran a simulation in which we assume the true treatment effect is the same across groups and check how long after turning 20 these women need to be released in order to explain the heterogeneity. The difference in the effects would require that women be discharged more than a year after they turn 20, which is not possible in the IDF. Therefore, this does not present a problem with the interpretation of our findings.

[30]A slightly different interpretation is that because these are young women, their parents would pay for medical procedures regardless of household earnings. However, a more financially constrained family may need to ask other family members for help, thereby imposing a social cost on the entire family.

[31]Both in Ananat et al. (2009) and in our context, a shift in personal moral objections to abortion could have occurred. We test for this possible explanation in Subsection 5.3 and find no supporting evidence.

[32]To further support the importance of privacy, we ran an analysis on a 2004 policy that eliminated the requirement that 18-year-olds obtain documentation from their HMO in order to receive the abortion for free (Barilovich, 2004). This policy implicitly increased the privacy of the decision for young women who share HMOs with their family. This small policy change resulted in a 1% increase in abortion (see Appendix F for more details). Future work will examine this policy in more depth.

[33]For example, the Medicaid-eligible population in the US may be somewhat similar to the low-SES population in our study. Although some states may choose to allocate their state Medicaid budget for abortion coverage,

5.3 Alternative Explanations

Finally, we explore two additional explanations of what could be driving the increase in abortion: (1) a change in personal moral views regarding abortion and (2) substitution from the illegal to legal market for abortion. We find minimal evidence for either.[34]

To explore the first explanation, we consider whether the policy change drove a shift in personal moral views about abortion, which resulted in more abortions. A shift in personal moral views could have happened if the policy itself signaled greater social acceptability of abortion. If this had occurred, we would expect the probability of abortion to increase among women who were unaffected by the policy change. Note that this puts the stable unit treatment value assumption (SUTVA) at risk. That is, 18-19 year-olds are also potentially affected by the treatment (through changing moral views) and, hence, they are not an untreated control group. Figure 3, which presents a first-differences exercise by age, shows no significant change in the probability of abortion among age groups ineligible for the subsidies. Given the small and insignificant shift in the untreated groups shown in Figure 3, we are not very concerned about this possibility, but cannot rule it out completely. Still, we can assume a weaker assumption: the shift in moral views was *constant* between the two groups (18-19 and 20-21). Consequently, our identification strategy still holds.

Finally, we explore the second alternative explanation, which posits that the 2014 policy could have induced a spillover of abortions from the illegal to the legal market (see Section 2.1 for a discussion of the Israeli illegal abortion market). The presence of an illegal abortion market complicates our interpretation of the results in two ways. First, the increase in abortion that we observe might not be an absolute increase but rather a substitution away from the illegal to the legal market in response to the subsidy. Second, in response to the increased funding coverage after the 2014 policy change, the illegal market could have reduced prices in order to retain customers. While we do not observe illegal abortions in our data, we attempt to infer changes by investigating the policy's effects on births.[35] If the entire effect of the policy is due to a shift from illegal to legal abortions, we should observe no change in births. On the other hand, if there was an increase in both illegal abortions and legal abortions in response

most low-income women on Medicaid are required to pay the full cost of an abortion (Guttmacher Institute, 2020). Notably, Cook et al. (1999) find that shortfalls in (state) Medicaid funding in North Carolina resulted in a 33% increase in pregnancies carried to term that otherwise would have been terminated.

[34]Another possible explanation is a standard price-theory effect (i.e., a reduction of the price of apples will result in an increase in the purchase of apples). While we cannot entirely rule out this possibility, we find no evidence to support it. If there is a pure price effect, we should see some change among secular women because of the lower latent cost of abortion. However, as suggested in Figure 5d, there is no effect among constrained or unconstrained women.

[35]In some settings, an increase in illegal abortion may result in an increase in hospitalizations due to abortion-related complications. We do not have data on hospitalizations for such a test; moreover, in Israel many illegal abortions are performed by trained medical doctors but done outside of the committee system (making them illegal), which may result in fewer post-abortion complications than other settings.

to the policy, we should see a decrease in births that is greater than the increase in the legal abortions we observe. Finally, if the change in abortion is indeed due to an increase in legal abortions, we should see a decrease in births that is proportional to the increase in abortions.

To test this hypothesis, we collapse our dataset to the year-month-age level and run the same DiD specification in Section 4.[36] The results in Figure A3 show an increase of five abortions per age-month and an approximately proportional decrease of eight births per age-month. Thus, the increase in abortion does not seem to be driven by a shift from illegal to legal abortions. The bigger decrease in births relative to the increase in abortions might suggest some price reduction in the illegal market, but the large standard errors suggest that this test is insufficient to provide strong evidence for this. Given the confidence interval for the result on births, we also cannot rule out a decrease in births that is smaller than the increase in abortions and thus indicative of some spillover from the illegal market to the legal. Additionally, our test above for moral hazard (Figure 4) provides some supporting evidence against substitution from the illegal to legal market. If we were simply picking up a substitution effect, we would expect to see an increase in conceptions (out of all women) because we would be counting conceptions that were previously occurring outside of the abortion committee system. However, as we showed above, we observe no change in conceptions. Ultimately, while we cannot fully rule out either spillover or a price response from the illegal market, we do not believe the presence of either is sufficiently large to undermine our main results.

Overall, our findings suggest that an increase in abortion access, which acts via a reduction in financial and social constraints, is the primary mechanism behind our finding of increase in abortion. This is consistent with an interpretation whereby the increased *privacy* of the abortion decision (associated with eliminating the financial cost) is an important factor. While we cannot observe privacy directly in our setting, other studies have emphasized the importance of privacy in reproductive decision-making, which supports our interpretation. For example, Myers and Ladd (2020) demonstrate that parental involvement laws enacted after the 1980s for minors seeking an abortion in the United States, which reduced privacy, increased teen births. There is also evidence from several low-income countries that highlights the importance of privacy. Ashraf et al. (2014) find that women in Zambia are less likely to seek family planning services if their husbands are involved, while Anukriti et al. (2022) demonstrate that leveraging social networks among women in India can help overcome stigma and social constraints in making family planning decisions.

[36] We cannot use the original conception cross-section data for this purpose because running the same specification with births as an outcome will mechanically lead to the inverse of the results on abortion because the population is comprised of conceptions (abortions + births). Therefore, an observation in this collapsed dataset is how many women conceived in a given year-month (say November 2013) at a given age (say 20) and the pregnancy result (abortion or birth). However this does not allow us to control for individual-level observable characteristics.

6 Downstream Social and Economic Effects

After establishing that making abortion free increased the probability of abortion among young, unmarried women, we shift to examining the effect this change had on women's medium-term fertility, marital status, human capital investment, and labor market outcomes. Given the results in Section 5.2, we focus our analysis in this section on the socially and financially constrained subpopulation . However, our results for the entire population are qualitatively consistent and presented in Appendix Table A4.

6.1 Empirical Strategy

Because women who have abortions are systematically different from those who give birth, and these differences are likely strongly correlated with labor market outcomes (see Column 9 in Table A2), the naïve OLS would be biased. To address selection and obtain exogenous variation in childbearing, we use the 2014 policy—which eliminated the monetary cost of abortion—as a natural experiment to estimate the causal effect of avoiding an undesired birth on education and labor market outcomes, and focus explicitly on the subpopulation of constrained women (our compliers) who grew up in poor and religious families.

We first estimate the intention-to-treat (or, reduced form, RF) effect of the policy. Since the full effect of avoiding an undesired birth is also of interest, we instrument for this using the 2014 policy and estimate the treatment-on-the-treated (or IV). Formally, we estimate the following:

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

$$\text{1st Stage: } abort_i = \delta \cdot Post \cdot T_i + \rho^{abort} \cdot y_{c_i}^{Pre} + \gamma_{a_i} + \gamma_{c_i} + X_i'\gamma_i + \epsilon_i^{abort} \tag{3}$$

$$\text{Reduced Form: } y_i^{Post} = \theta^{RF} \cdot Post \cdot T_i + \rho^{RF} \cdot y_{c_i}^{Pre} + \gamma_{a_i} + \gamma_{c_i} + X_i'\gamma_i + \epsilon_i^{RF} \tag{4}$$

As in Equation 1, $Post$ is an indicator for the policy's being in effect ($\mathbb{1}\{c_i \geq \text{Dec-2013}\}$) and T_i indicates woman *i* is eligible for the subsidy ($\mathbb{1}\{20 \leq age_{c_i}\}$). y_i^{Post} is the mean outcome of woman i in the year of and through 3-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.[37] We include pre-pregnancy woman-level (nonparametric) controls that are known to have a first-order effect on the fertility decision

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

(ethnicity, religiosity level, education, and father's earnings).

Because compliers are women who could not afford \$600-\$1,000 to have an abortion, 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.[38]

6.2 Effect on Demographic Outcomes

Why would access to free abortion affect women's human capital investment or labor market outcomes? It is well documented that parenthood acts as a penalty for women's careers. Therefore, we need first to establish that the increase in abortion allowed women to delay parenthood. For this purpose, we define a binary parenthood outcome that equals 1 if the woman gave birth in any of the 3 years following the 2014 policy change and estimate Equations 2 - 4.

We present the IV and RF results, as well as the the naïve OLS for comparison, in Table 2 across a range of demographic outcomes. The results support our prior: The reduced form specification shows an 11.7-percentage-point decrease in parenthood in the subsequent three years following the index pregnancy. This is a large effect and represents a 19.2% reduction in medium-term parenthood relative to the baseline of 61% in this population. Myers (2017) finds reductions of similar magnitude (19-34%) in the probability of giving birth before the age of 19 in the United States and attributes these to abortion liberalization and confidential access. However, we note that the population (teens) and outcome (giving birth before age 19) in Myers' analysis differ from our setting.

Similarly, conditional on giving birth in the subsequent four years, women's age at first birth increased on average by 0.56 in the reduced form specification. These too are meaningful magnitudes, particularly considering that this outcome is censored because we are only able to look at fertility 3 years after the index conception. Perhaps as a result of the censoring, we do not find a significant effect on the total number of children born among the socially and financially constrained women, although the magnitudes (0.12-0.84 fewer children) are not inconsequential. A study of abortion access across 97 countries finds a reduction in the

[38] A potential violation of the exclusion restriction is the always takers, who are now getting a lump-sum transfer in the amount of the abortion subsidy. While we cannot test this hypothesis directly, we show in Table A4 that the effects are consistent (and, if anything, stronger) in the socially and financially constrained population–where we have a higher rate of compliers, which suggests that estimated effects are driven by the share of compliers.

total fertility rate of 0.05-0.06 children due to abortion access when considering the entire life course of women's childbearing (Bloom et al., 2009).

Avoiding an undesired birth might also reduce the probability of marriage in the medium term, because women who avoid an undesired birth may also avoid marrying the father. The data seem to support this hypothesis: Among the population of socially and financially constrained unmarried 18-21 year olds, the 2014 policy reduced the probability of getting married by 16 percentage points (in the reduced form specification) in the years following the index pregnancy, which is equivalent to a 41% reduction relative to a baseline of 39%. These results suggest that the removal of financial constraints to abortion allowed these women to avoid undesired parenthood *and* a subsequent undesired marriage. This finding is consistent with Myers (2017) analysis of the effect of abortion legalization on marriage before the age of 19 , although the magnitude of the effect we find is larger (39% relative to 19% attributed to confidential access to abortion). The delays in parenthood and marriage we find are also in line with findings on reproductive health access more broadly, including the oral contraceptive pill in the US (Bailey, 2013, 2006; Goldin and Katz, 2002; Ananat and Hungerman, 2012)[39] and recent work by Gershoni and Low (2021), who find a substantial increase in average age at first marriage following Israel's 1994 adoption of free in vitro fertilization.

6.3 Effect on Human Capital Investment and Labor Market Outcomes

Given the young age of the women in our sample, this is a critical time for their human capital investment. To explore this margin, we estimate Equations 2 - 4 on university enrollment (see Table 2). Furthermore, our detailed panel data on fertility, employment, and education allow us to go beyond average effects post-conception and examine the temporal dynamics of these effects, and reveal a more nuanced story (see Appendix E for a full description). Our reduced form specification finds a 1.3-percentage-point increase in the probability of university enrollment (relative to a baseline of 5.4%),[40] which continues to increase over the 3 years following the potential undesired birth (Figure 6c). This result, coupled with the delay in parenthood and marriage, is consistent with Goldin et al. (2006), who argue that the reversal of the gender gap in college graduation was driven by increases in girls' expected economic returns to college due to perceived labor market opportunities and an increase in the age at first marriage.

Next, we examine whether an undesired birth affects labor force participation. The child-

[39] Although we note that Myers (2017) argues that these effects are attributable to abortion legalization rather than contraceptive pill access.

[40] While 5.4% might sound surprisingly low, this is due to the delayed timing of college enrollment in Israel due to military service. Across the entire population, only 26% of women in Israel have graduated from college (Table A2). Furthermore, the religious population has lower rates of college completion.

penalty literature suggests a large and persistent decrease in employment after becoming a mother, but is that the case for *undesired* parenthood as well? In this setting, we find a 5.8-percentage-point decrease in employment (including part-time, full-time, and self-employment) in the short to medium term. Why would labor force participation be reduced among women who delayed parenthood relative to their counterfactual outcome of having a child? To understand what might explain this result, it is important to understand the context and expectations of women in Israel. According to the International Social Survey Programme (ISSP), 80% of Israelis believe "a women with children under school age should work outside the home"; this is the highest share in the OECD (Kleven et al., 2019a).

Additionally, it is important to note that socially and financially constrained women in our sample come from very religious Jewish backgrounds, including the ultra-Orthodox community, in which women are the primary earners in the household because the men are expected to devote themselves to studying the Torah. At the same time, the Orthodox community is also very patriarchal and the burden of childrearing falls on women but is commonly shared between all the women in the family (Lidman, 2016). Thus, these women are expected to work outside the home and also raise children. This reality is reflected in our data: 77.7% of young, unmarried, and socially and financially constrained women who conceived are working in the year of conception (Table 2). Therefore, avoiding the need to provide for a newborn child as a young unmarried religious woman in Israel could result in a *decrease* in employment.

Looking across types of labor force participation allows us to add more nuance to this finding as well. While we see a reduction in overall labor force participation, we see a 5.5-percentage-point increase in part-time work relative to full-time work (see Section 3.1.3 for how these categories of labor force participation are defined). These findings are consistent with substitution toward human capital investment: The counterfactual women who could not have had the abortion before the policy, gave birth and worked full-time; when abortion is provided for free, they are more likely to invest in their human capital by enrolling in college, but shift to part-time and self-employment because of the flexibility this affords. The importance of flexible employment arrangements has been cited as a key factor in closing the gender wage gap (Goldin, 2014; Bang, 2021; Goldin and Katz, 2016), and our findings point to how the combination of abortion access and flexible work arrangements may allow women to invest more in human capital.

Examining the dynamic effects helps to shed further light on the labor force participation results. Appendix Figure E2 shows an initial increase in total months worked in the year of potential birth for women who avoided an undesired birth, followed by a decay. A temporary increase in months worked in the year of potential birth is intuitive: Prior to removing the financial barrier to abortion, the counterfactual woman would have given birth to a child and likely have taken maternity leave or reduced their months worked. Then, over time the counterfactual woman who gave birth returns to full-time work and the woman who avoided an undesired birth (due to the subsidy) enrolls in college and shifts toward part-time work; therefore, we see relatively higher months worked among the counterfactual women who gave birth.

Next, we examine whether this investment in human capital translates into higher earnings. Our results in Table 2 find a statistically insignificant decrease in yearly earnings; however, it is important to highlight the fact that these are short- to medium-term effects because are limited to 3 years of post-policy data, and many women who enrolled in university may not have graduated yet. Additionally, as demonstrated above, the women who avoided an undesired birth due to elimination of the cost of abortion were more likely to work part-time, which also helps to explain the slight decrease in earnings conditional on working. Digging into the dynamics reveals a *temporary* increase in yearly earnings conditional on working (see Figure 6d). Specifically, the yearly earnings (conditional on working) of socially and financially constrained women increased in the year of potential birth due to the policy. Overall, our findings on earnings should not be overinterpreted given the short timeframe available in our data for college enrollment to generate economic returns; future work should examine whether higher college enrollment led to higher college graduation, which has been found in other settings (Ananat et al., 2009).

Finally, we ask whether the human capital investment translates into employment in better paying jobs. To answer this, we estimate Equations 2 - 4 on the sector-level wage premium. Following Abowd et al. (1999), we estimate the sector-level wage-premiums by running a log-wage regression on individual and sector fixed effects (see further details in Appendix G). The results in Table 2 suggest an increase of 0.015 log-points (50% increase relative to a baseline of 0.03 log-points) in the wage premium of the sector in which these women work.

Although our 3 years of data following the 2014 policy limit our ability to examine the full labor-market implications, our results suggest that removing of the cost of abortion resulted in an investment in human capital, substitution toward more flexible work arrangements, and a shift toward better-paying jobs in the short to medium term. In this section, we presented results for the population of unmarried low-income religious women (the socially and financially constrained population) because the effect on abortion utilization was driven by this subgroup. Results for the overall population of all unmarried 18-21 year old women are similar (Table A4) but often smaller in magnitude and noisier, which suggests that higher human capital and economic returns accrued to the more economically disadvantaged group of women. This finding is consistent with researchers who have found, in studies of the US, that educational attainment and labor force participation increase more for low-income or Black women (Angrist and Evans, 2000; Kalist, 2004).

7 Conclusion

In this paper, we study the economic consequences of expanding access to free abortion services. Abortion access is widely discussed across the world, and often in highly charged moral and ethical debates; many focus on whether abortion should be *legal* (e.g., recent examples from Argentina, Mexico, and US states such as Texas and Mississippi). However, in settings in which abortion is already legal, the financial cost can still impose barriers to access. In many settings in which abortion is already legal, policymakers are turning to the question of removing or relaxing financial barriers to make abortion more accessible (Gutierez, 2021; Heyward, 2022; Denholm, 2018; Bladen, 2022). Thus, understanding the impacts of removing such barriers is particularly timely. We take advantage of a change in an Israeli policy to examine the impact of expanding access to *free* abortion. Using a difference-in-differences strategy, we compare "newly funded" women aged 20-21 (treatment) to "always funded" women aged 18-19 (control) before and after the 2014 policy reform. We find that expanding access to free abortion services increases the utilization of abortion. The magnitude of the effect is meaningful: The 3-4.6 percentage-point increase represents a 4.5%-7%, given the baseline abortion ratio of of 66% among unmarried women aged 18-21 who conceived.

Our analysis investigates two primary mechanisms that explain this increase. First, we examine whether the policy induced a moral hazard response, in which women reduced contraception use because abortion became less costly. We find no evidence of moral hazard. Rather, we find that a combination of social stigma and financial constraints drives the increase in abortion that follows implementation of the policy. Our interpretation is that providing free abortion allows women to avoid asking for financial help to cover the abortion cost, which increases the privacy and independence of their reproductive decisions.

Furthermore, our results suggest that *undesired* parenthood imposes an added penalty to a woman's careers. When abortion is not free, young women who cannot afford an abortion or lean on social networks to help cover the cost enter into early, undesired parenthood and possibly undesired marriage; we show that this is avoided when the financial constraints to obtaining an abortion are removed. Consequently, we find that avoiding early, undesired parenthood allows young women to invest more in their human capital by enrolling in college, assume more flexible employment arrangements while completing their studies, and work in sectors with a higher wage premium.

Our findings suggest that for young unmarried low-income religious Israeli women, an undesired birth induces an additional penalty to their career plans. Therefore, eliminating these monetary barriers may be a useful policy to enable women to time parenthood and increase their early career investment. However, given the specificity of this population, one may wonder whether these results generalize to other populations. Young, unmarried, and low-income women are more likely to have unintended pregnancies and undesired births in both Israel and other settings (Biggs et al., 2013; Buckles et al., 2019; Bankole et al., 1999; Israel Defense Forces (IDF), Medical Corps et al., 2019; Rottenstreich et al., 2017, 2018; Sikron et al., 2003).

Thus, our findings are relevant for other settings in which abortion is legal but may be costly and out of reach for low-income populations or those who cannot lean on social networks for support. Expanding financial support for abortion is being discussed both internationally and in many US states following the Supreme Court decision that overturned *Roe v. Wade*. For example, in the United States some states allow Medicaid-funded abortions for low-income women under certain conditions, and several states have recently mandated that insurance companies fully cover the cost of abortion (Gutierez, 2021; Heyward, 2022). Past studies have shown that interruptions in Medicaid funding cause a reduction in abortion, but neither the mechanisms—and particularly the role of *privacy* and *social stigma*—nor the downstream economic impacts for women have been investigated in those settings (Kane and Staiger, 1996; Levine et al., 1996; Cook et al., 1999; Meier and McFarlane, 1994; Morgan and Parnell, 2002).

Future research should investigate the potential privacy mechanism in other settings, as well as the role of hassle costs in affecting abortion utilization. A limitation of our analysis of career consequences of the Israeli policy is the short time horizon and specificity of our sample (unmarried women aged 18-21). A promising future avenue for research is to examine the long-term effects of funding schemes and focus on women who abort at older ages. Overall, our analysis shows that covering the cost of abortion can be a powerful policy tool, that allows women to time parenthood and increase their early career investment, while granting them privacy in making personal reproductive decisions.

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Table 1: Effect of Removing Abortion Cost on Abortion Utilization

	DiD	DiD+Controls	LTT	DDD
Treatment Effect	4.63	3.19	3.00	3.93
	(1.35)	(1.58)	(1.33)	(1.71)
N	24,650	21,432	24,650	125,115

Notes: This table presents the primary difference-in-differences results of the 2014 policy on abortion utilization. Our baseline specification in Column (1) follows Equation 1 as described in Section 4.1 – where we compare outcomes before and after the policy change for women who were affected (20-21) and unaffected (18-19) by the expansion of the subsidy. Column (2) includes a set of pre-pregnancy non-parametric controls (ethnicity, religiosity level, education, family's yearly earnings). Column (3) controls for differential time pre-trend as described in Appendix C. Column (4) corresponds to a specification using the married population as a third difference (DDD). Standard errors clustered by age at conception in parentheses.

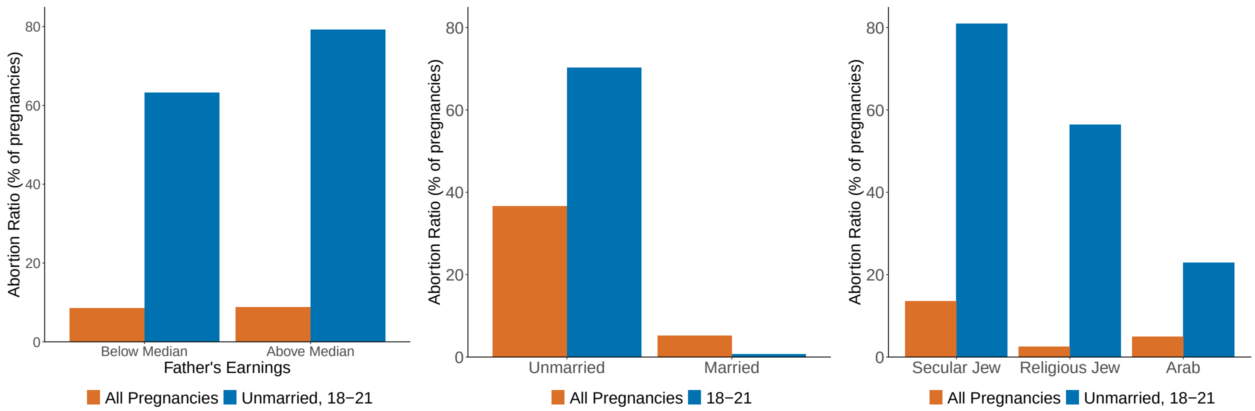
Table 2: Effect on Downstream Social and Economic Outcomes

	OLS	IV	RF	Mean	N
Is a parent	-0.703	-0.839	-0.117	60.9%	1,790
	(0.007)	(0.069)	(0.021)		
Married	-0.32	-1.148	-0.16	39.1%	1,790
	(0.019)	(0.447)	(0.041)		
Age at 1st Birth	3.234	2.838	0.566	20.53	1,578
	(0.051)	(0.436)	(0.128)		
Number of children	-1.39	-0.838	-0.117	1.03	1,790
	(0.078)	(0.5)	(0.091)		
BA Enrollment	0.009	0.844	0.013	5.4%	1,790
	(0.01)	(0.67)	(0.004)		
Working	0.17	-0.371	-0.052	77.7%	1,790
	(0.017)	(0.203)	(0.022)		
Employed by a firm	0.174	-0.414	-0.058	76.3%	1,790
	(0.017)	(0.218)	(0.024)		
Employed part-time	-0.119	0.398	0.055	77.4%	1,790
	(0.03)	(0.278)	(0.028)		
Self-employed	-0.004	0.043	0.006	1.4%	1,790
	(0.002)	(0.02)	(0.003)		
Earnings (NIS, Cond.)	10280.401	-20456.56	-2810.076	23,138	1,688
	(1651.6)	(13651.9)	(1443.5)		
Sector's Wage Premium	0.016	0.105	0.015	0.03	1,688
	(0.004)	(0.016)	(0.001)		

Notes: This table presents results for the effect of the 2014 policy on a range of human capital formation and labor market outcomes. The first column presents the naïve OLS, the second column presents results from the IV (Equation 2), and the third column presents results for the reduced from (Equation 4). The sample includes 18-21 year old, unmarried, socially and financially constrained women. Means are calculated using the pre-policy data. Standard errors clustered by age at conception in parentheses.

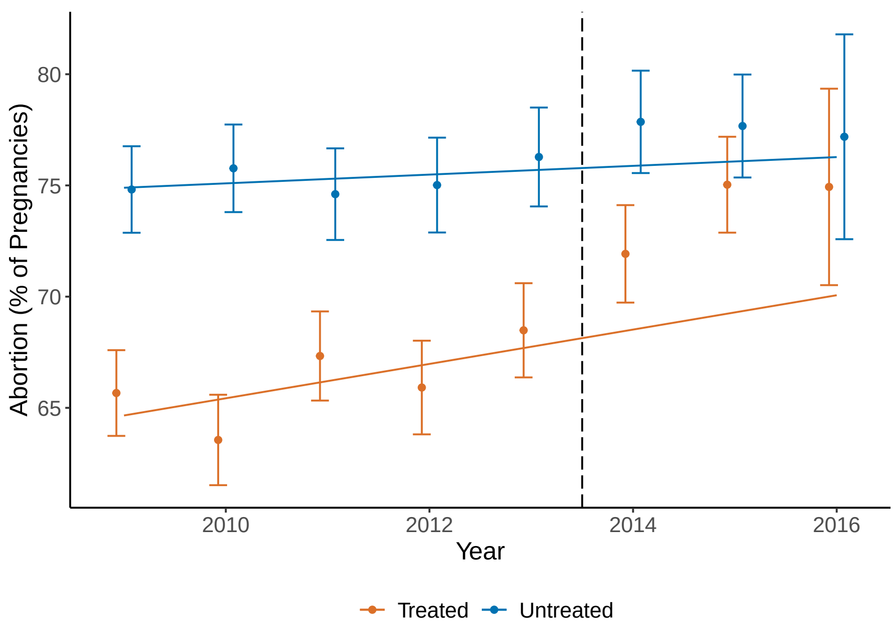
Figure 1: Baseline Abortion by Sub-Group

(a) Father Earnings (b) Marital Status (c) Ethnicity



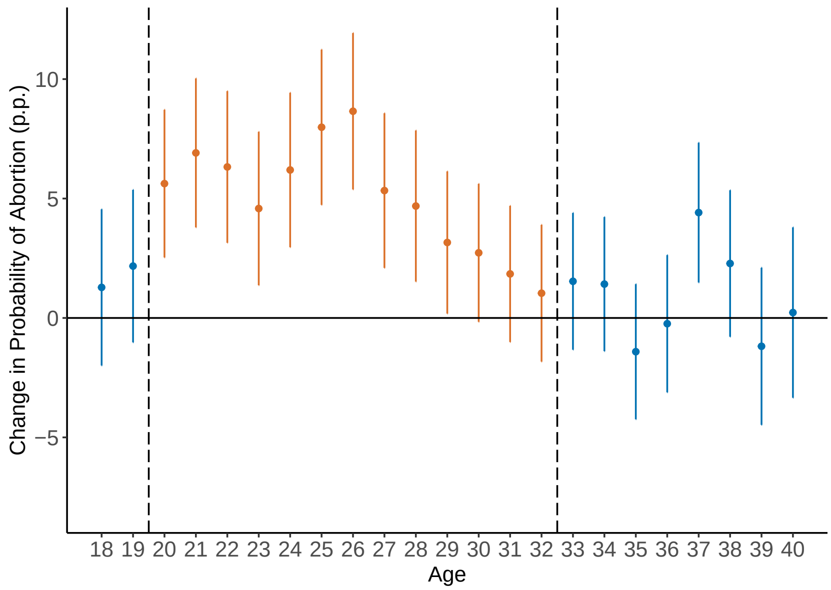
Notes: This figure presents abortion ratios (the % of pregnancies that end in abortion) among important sub-groups within Israel pre 2014 (the year of the policy change). Each panel shows the proportion of abortions out of all pregnancies (orange) and our sub-population of unmarried 18-21 year olds (blue). Panel (a) presents abortion by father's earnings. Panel (b) disaggregates by marital status (note that here the blue bar is restricted to only 18-21 year olds not unmarried 18-21 year olds). Panel (c) presents abortion by ethnicity.

Figure 2: Parallel Trends Assessment (18-21)



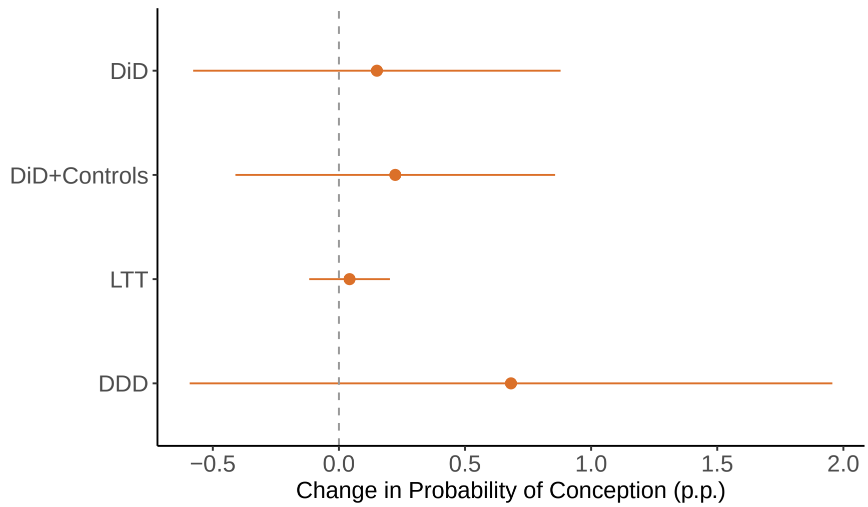
Notes: This figure presents the abortion ratio (the % of pregnancies that end in abortion) for treated (20-21) and control (18-19) women over time (2009-2016). The control population is presented in blue, and the treatment population is presented in orange. The dashed line indicates the timing of the 2014 policy change. Each dot represents the mean abortion ratio in a given year for the treatment and control groups of women, respectively, and the error bars 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).

Figure 3: Difference Between Pre and Post Policy Abortion by Age (Raw Data)



Notes: This figure presents the results of a before-and-after exercise in which we restrict the data to two years before and after the 2014 policy change (2012-2015) and estimate the post-policy difference in the abortion separately for each age (18-40). The point estimates can be interpreted as the percentage point difference in the probability of abortion for each age group following the introduction of the 2014 policy. The lines are 95% confidence intervals and the horizontal line marks 0. The ages that were eligible for the 2014 subsidy expansion are indicated in orange (treated), 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.

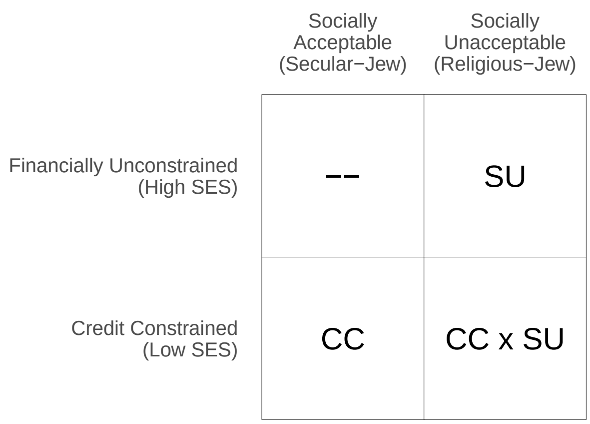
Figure 4: Change in Conceptions (No Evidence for Moral Hazard)



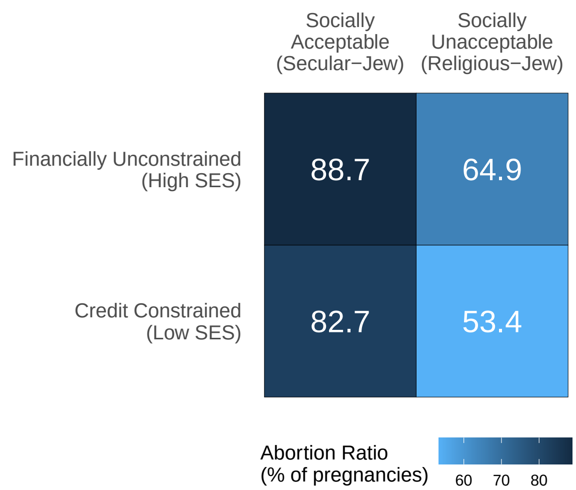
Notes: This figure presents the difference-in-difference results for the effect of the 2014 policy on conceptions probabilities from the population of 18-21 years old unmarried women. Each row presents the results from a different specification, where the dot represents the treatment effect and the lines mark the 95% confidence interval around the point estimate. DiD represents our baseline specification following Equation 1 as described in Section 4.1 – where we compare outcomes before and after the policy change for women who were affected (20-21) and unaffected (18-19) by the expansion of the subsidy. $DiD + Controls$ includes a set of pre-pregnancy non-parametric controls (ethnicity, education, yearly earnings, months worked). LTT controls for differential time pre-trend as described in Appendix C. DDD corresponds to a specification using the married population as a third difference. The dashed vertical line is at 0, indicating an insignificant result (at the 5% level). The sample includes all unmarried women in the country aged 18-21 from 2009-2016. The estimates are percentage point changes that can be interpreted as the relative change per 100 women.

Figure 5: Effect is Strongest Among the Socially and Financially Constrained Women

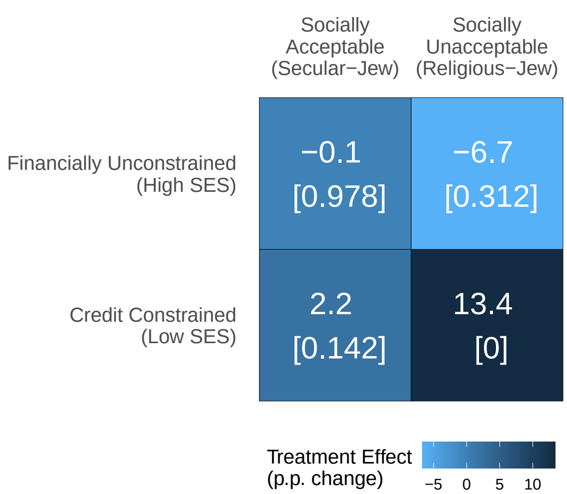
(a) Qualitative Predictions from the Model



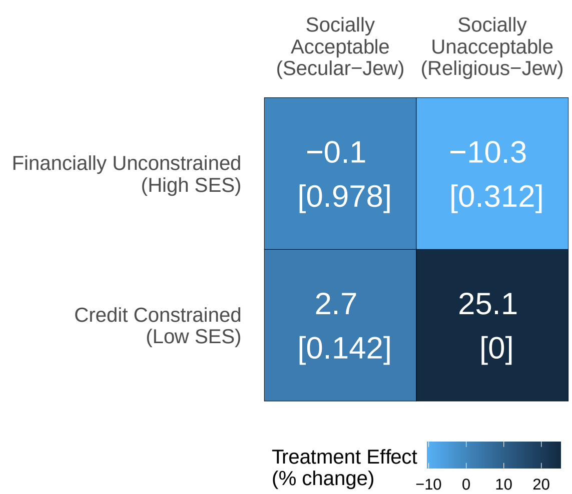
(b) Baseline Abortion



(c) Policy Effect (p.p.)



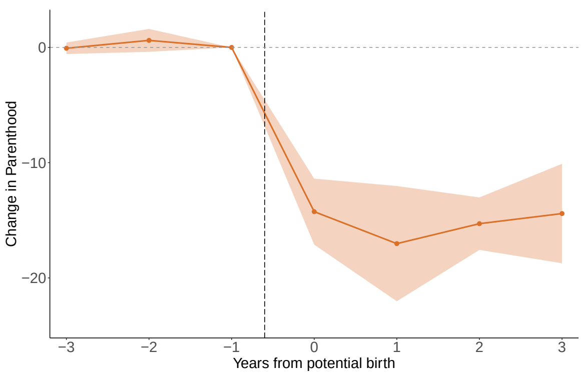
(d) Policy Effect (% Change)



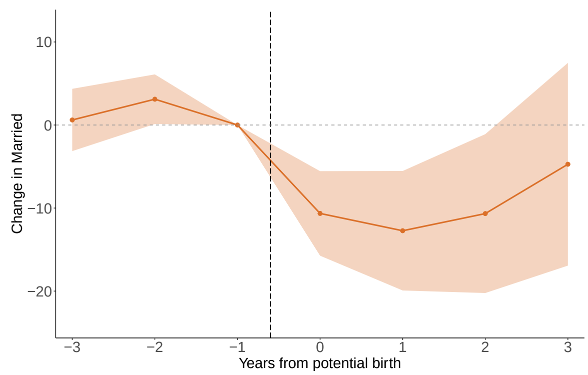
Notes: This figure presents the heterogeneous effect of the abortion funding policy on abortion while splitting the population across two dimensions: religiosity and SES background (based on father's earnings). Panel (a) presents the theoretical prediction based on social and financial constraints (see Section 5.2); (b) presents baseline abortion (as a % of pregnancies) within each group; Panel (c) presents the effect of the policy on abortion by each group in p.p. with p-values in brackets; Panel (d) presents the effect of the policy on abortion by each group in percent increase relative to baseline abortion ratio with p-values in brackets. Darker blue shading corresponds to higher values, while lighter blue represents smaller values.

Figure 6: Abortion Access Decreases Entrance to Parenthood and Increased Human-Capital Investment

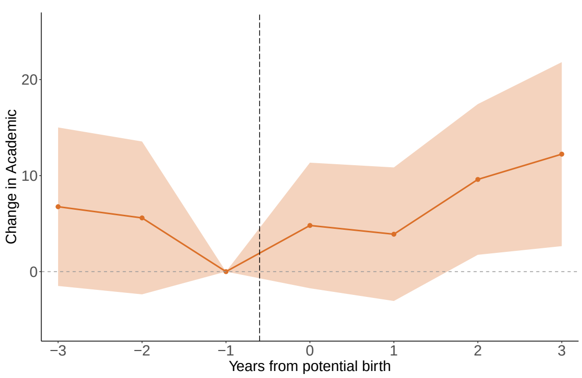
(a) Parenthood



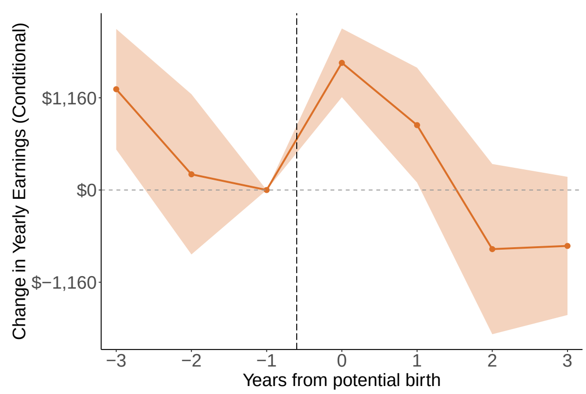
(b) Marriage



(c) University Enrollment



(d) Yearly Earnings



Notes: This figure presents the event study-DiD results for four outcomes: Panel (a) presents the results for the probability a woman is a parent, Panel (b) presents the results on the probability a woman is married, Panel (c) presents the results for the probability a woman is enrolled in an academic, 4-year university program, and Panel (d) presents the results for the woman's yearly earnings, conditional on working. Each panel presents the results for the reduced form effect of the 2014 policy relative to year of potential birth as described in Equation 6. 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 year). The shaded regions mark 95% confidence intervals around each point estimate. The dashed vertical line is at 0, indicating an insignificant result (at the 5% level). The sample consists of unmarried women 18-21 year olds who conceived between 2009-2016, and are socially and financially constrained (religious and low-SES, see Section 5.2).