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

Nina Brooks Tom Zohar

June 10, 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 20-34 eligible and use unique administrative data that allows us to track abortions, births, employment, earnings, and formal education for the universe of Israeli women from 2009-2016. We examine the impact of the policy among young, unmarried women and show that access to free abortion increases abortion but does not increase conceptions. The effect is driven by low-income women from religious Jewish backgrounds in which abortion is socially stigmatized. This finding suggests that making abortion free increased the privacy of the decision. In the medium-term, the policy delayed parenthood and marriage, increased human capital investment, and shifted employment toward higher-paying and more flexible work arrangements, suggesting a large opportunity cost of undesired parenthood.

*Brooks: School of Public Policy, University of Connecticut. nina.brooks@uconn.edu. Zohar: Economics Department, CEMFI. tom.zohar@cemfi.es. We thank Ran Abramitzky, Boaz Abramson, Tslil Aloni, Hadar Avivi, Hernán Barahona, Eran Bendavid, Eric Brunner, Nina Buchmann, Raj Chetty, Cauê Dobbin, Pascaline Dupas, Liran Einav, Hedva Eyal, Diego Jiménez, Caitlin Myers, Sharon Orshalimy, Petra Persson, Yotam Shem-Tov, and Isaac Sorkin, as well as participants at APPAM, ASSA, the 2019 Chapman University Institute for Religion, Economics and Society Graduate Student Workshop, Stanford University Economics seminars, and the 2021 NBER Summer Institute for helpful comments. We gratefully acknowledge financial support from Leonard W. Ely and Shirley R. Ely Graduate Student Fellowship, the Stanford Earth Dean's Fellowship, the David & Lucile Packard SGF Fellowship, the Shultz Fellowship, the Freeman Spogli Institute for International Studies, and the Stanford Graduate Research Opportunity Grant for this work. All views and errors are our own. Corresponding author: Tom Zohar, tom.zohar@cemfi.es.

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 to 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. Thus, much less is known about how changes in *funding* policies affect the abortion decision and downstream social and economic outcomes for women.[2] Understanding the impacts of removing the monetary cost of abortion is particularly important as many states and countries are moving beyond legal rights to abortion and considering policies that reduce the monetary cost.[3]

In this paper, we shed light on these important policy issues 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 1a). However, many women were ineligible for these subsidies and unable to access abortion services due to the $600 co-pay.[4,5] In response to advocacy efforts by local activists, the Israeli government massively expanded the existing subsidy in 2014, making women aged 20-32 years of age eligible for free abortion.

To study the effects of expanded funding for abortion, we ask three questions: Does abortion increase when it is offered for free? If so, *why*? And, what are the downstream social and economic effects of avoiding an *undesired* birth?[6] To answer these questions, we link administrative data on the universe of individual pregnancies (abortions and births) in Israel from 2009 and 2016 to tax data on employment, earnings, and educational enrollment. Detailed,

[1]There are numerous examples from different settings, such as abortion legalization in the US: Angrist and Evans (1996); 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), 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]The limited evidence on funding expansion/restrictions comes exclusively from studies using variation in Medicaid funding for abortion in the United States (Kane and Staiger, 1996; Levine et al., 1996; Cook et al., 1999; Bitler and Zavodny, 2001).

[3]See for example California (Gutierez, 2021) and Maryland (Heyward, 2022) in the United States, which recently enacted policies to fund abortions, and state-level differences in the cost of an abortion in Australia (Livsey, 2017).

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

[5]Hedva Eyal, President of the Haifa Women's Coalition, Tel Aviv, Israel, April 2020.

[6]We refer to undesired births throughout. An undesired birth may be the result of either an unplanned pregnancy or a pregnancy in which a change in information later led the woman to want to avoid birth.

individual level administrative data on abortion for an entire country is extremely rare, thus our data present a unique opportunity to examine abortion access at the individual level. Using a difference-in-differences identification strategy, 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.[7] Specifically, the share of abortions out of total pregnancies increased by 5 percentage points among young, unmarried women.

We then explore the two primary hypotheses from the economics literature 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, resulting in more pregnancies and abortions (Levine and Staiger, 2002; Ananat et al., 2009). In our setting, we find no change in conceptions, suggesting 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 women in their early 20s who struggled to come up with the abortion co-pay, but particularly young women from religious backgrounds.[8] 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 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 where 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.

Next, we ask whether avoiding an undesired birth affects future fertility, marriage, education, and employment decisions. We use the sharp change in abortion access induced by the policy as an instrument for whether a woman can avoid an undesired birth. We first show that, among policy compliers, having an abortion results in a decrease in parenthood and marriage in the three years following conception. We also find an increase in college enrollment. Finally, we

[7]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).

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

find that avoiding an undesired birth results in a lower probability of being in the labor market, but that, conditional on working, women who avoid an undesired birth are more likely to work part-time and in better-paying sectors (e.g., public sector instead of 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 constraints to 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 towards part-time (but better paying) employment hints at a substitution towards more flexible employment arrangements that allow these women to complete their studies.

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 both in 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 the populations who may benefit the most from policies that reduce the monetary barrier of abortion access, policies that are actively discussed these days in several U.S. states. For example, in the US context state-level Medicaid funding for abortion has been shown to increase access to abortion (Cook et al., 1999; Levine et al., 1996), but whether subgroups, such as more religious or conservative women, specifically benefited or whether the increased access led to economic effects for women has not been investigated. Our findings shed some light on the mechanisms of these effects, particularly the role of *privacy* and *social stigma*, as well as potential economic consequences.

Our paper advances the literature in several ways. Abortion is notoriously difficult to study because reliable data on abortion is rare and often comes from surveys, where under-reporting is high. 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 answer critical questions in economics about whether and how abortion access matters for women's lives. For example, does abortion access impact fertility? Does providing abortion for free increase pregnancies due to moral hazard? Who benefits the most from relaxing constraints to abortion access? And, what are the economic effects of using abortion to avoid an undesired birth?

Our ability to use detailed individual-level data on abortions and births allows us to under-

stand the mechanisms that drive the effect of making abortion free on the abortion decision. 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 we find no evidence that providing abortion for free induced moral hazard, we suggest an alternative explanation: the role of social stigma, financial constraints, and privacy. This finding relates to Ananat et al. (2009), who find that in the U.S., abortion legalization had weaker effects in more conservative states. An important difference is that the Israeli policy we examine eliminated the entire monetary cost of obtaining an abortion, which enabled low-income women to make the abortion decision in private, without asking friends or family for financial support.

This finding on the importance of privacy in making reproductive decisions echoes that of Myers and Ladd (2020), who demonstrate how parental involvement laws for minors seeking an abortion in the United States increased 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 abortions and religiosity allows us to examine the role of privacy from a different angle and show how the abortion funding policy in Israel was primarily utilized 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, even when they are legal, can be large.

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 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 short-term, 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, less likely to marry young or divorce in the long-term.

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. (2019) and Foster et al. (2018), use the landmark Turnaway Study[9] 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. (2019) and Foster et al. (2018), although we examine different economic outcomes, namely early career and human capital investment. We are also 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 *added* penalty of an *undesired* birth. Evidence from the U.S. suggests that women who seek abortions have lower-income, are less likely to have health insurance, and generally 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 have shown how poor and religious women benefit the most from eliminating the cost of abortion.

The rest of this 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-difference 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 sub-population 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 Abortion in the Israeli Context

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. Here we describe the existing abortion law in Israel prior to the policy change, the cost of an abortion, and the 2014 policy change we use for identification.

[9]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 facility gestational limit (turnaway group).

2.1 Abortion Law in Israel

Abortion has been legal in Israel since 1977, conditional on approval from a committee and on an individual basis. The committee approves the 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 1a. The committee is composed of two medical professionals and a social worker, one of whom must be a woman. The criteria for approval imply that the only group of women who will not be automatically eligible for an abortion are married women between 18-40 years of age who have healthy pregnancies that were not the result of rape, incest, or infidelity.[10]

All legal abortions in Israel must go through this committee process, including when women opt to have the procedure performed by a private doctor of 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; indeed, our data show that 99% of applications are approved and 97% are acted upon.

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.[11] Anecdotal evidence suggests illegal abortion does exist in Israel but is dominated by 'high-end' (and high cost) providers operating outside the committee process, rather than the unsafe conditions that are more characteristic of illegal abortion in other settings (Newman, 2017; Oberman, 2020). Israeli law puts the liability of performing an illegal abortion on the doctor rather than the woman, but these are rarely prosecuted (Amir, 2015). Given the high accessibility, quality, and low, or in some cases free, cost, incentives to have an illegal abortion in Israel are low. Particularly as one can have the abortion performed by a doctor of their choosing and avoid 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

[10] These criteria are aligned with Israel's pro-natalist policies. See Appendix B.1 for more information on the origins of the abortion committee in Israel.

[11] Another alternative explanation for the high approval rate might be due to women that are likely to be denied will therefore travel to neighboring countries to perform the abortion (i.e. 'abortion tourism'). However, due to the Israeli political and geographic environment, traveling to neighboring countries is impossible, and even if it was, abortion laws among these countries are much more restrictive than in Israel. Therefore, we are not concerned about the possibility of 'abortion tourism' in our setting.

(Oberman, 2020).[12,13]

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 a large illegal market would complicate the interpretation of our results because any change in the abortion ratio could be due to shifts from the illegal to the legal abortion market and we address this concern directly in Section 5.3.

2.2 Cost & Financial Constraints

After receiving the approval from the committee, women have to pay an out-of-pocket (co-pay) cost for the abortion. The cost of an abortion varies from NIS 2,100 - 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, in our analytic sample of unmarried women aged 20-21 (treated) who conceived, the average monthly earnings is NIS 2,109 (USD 624) conditional on working that month, and the average woman in this population works 5.5 months per year.

Figure 1a shows the variation in abortion ratios[14] across income levels in Israel, prior to 2014, when the policy was implemented. Higher rates occur among women from higher-earning households. 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.

[12] An article in the Israeli newspaper Seven-Days ('Shivaa Yamim') suggested that there are 15,000 illegal abortion a year in Israel (Newman, 2017). However, after contacting the reporter and organization quoted in the article, we found no evidence for the original source of the survey, nor an individual that was willing to take responsibility and confirm the numbers.

[13] 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, we have explored the availability of abortion pills for purchase online and they do not appear to be widely available in Israel, which has also been affirmed by the anecdotal experience of various advocates we have spoken to.

[14] Throughout our analysis we will primarily calculate and refer to abortion ratios: the share of pregnancies (that is, the total number of legal abortions and live births) that end in abortion. This is in contrast to the more commonly used abortion rates, which refer to the number of abortions per 1,000 women of a given age. As we will discuss in the Data section (Section 3), we are actually able to calculate the abortion ratio because we observe all pregnancies. In cases where the abortion rate is used, it will be explicitly noted.

2.3 Social Stigma

Because of its cultural and religious heterogeneity, Israel is an interesting setting to study abortion.[15] Figure 1c demonstrates the substantial variation in baseline abortion ratios, which might suggest differing abortion views, sexual behavior, and use of contraception across groups.[16] The Jewish population consists of a wide mixture of religiosity levels, ranging from secular Jews (45%), traditional Jews (25%), religious Jews (16%), and Orthodox Jews (14%) (Central 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.[17] Our administrative data allow us to directly observe the religiosity of the Jewish population, enabling us to leverage the variation in Jewish religiosity and abortion views to unpack mechanisms of the change in abortion policy.

2.4 The 2014 Natural Experiment: Eliminating the Cost of Abortion

To prevent cost from being a barrier to abortion access the Israeli government enacted several policies over the past few decades.[18] Prior to 2014, women aged 19 or below could obtain an abortion free of charge. However, since co-pays are rare (and small) in the Israeli healthcare system, women aged 20 or above were often surprised to learn they needed to pay between \$600-\$1000 for the abortion procedure upon arriving at the clinic. According to Dr. Eyal, head of the Haifa Women's Coalition, a women's rights organization that also helps young women access reproductive health services, women from lower earning families frequently struggled to come up with money to cover the co-pay.[19] Religious women in particular faced difficulties because they could not ask friends or family members for financial support for an abortion. To help support women access abortion services, the Haifa Women's Coalition raises money to

[15] 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.

[16] The Israel National Health Interview Survey (2013-2015) asked respondents about their contraceptive use (Einav et al., 2017). Figure B4 shows the variation in responses by self-reported religiosity from this data.

[17] 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).

[18] Since 1977, abortion cost has been fully subsidized if the woman is 17 years old or below, if the pregnancy results from rape or incest, or if there is a medical risk for the woman or fetus (see Table 1a, column 3). This subsidy has been expanded several times: first in 2001 to include women up to age 18, then in 2008 to include women up to the age 19. Thus, for women whose abortion was approved by the committee for any of the eligible criteria, she would not have to pay if she was 19 or younger.

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

help support women who need financial help, but leaders noted that it was insufficient for the demand. Women's rights advocates lead by Dr. Eyal lobbied the Israeli Health Ministry and advocated for an expansion of the existing subsidies.[20,21]

In January 2014, the Israeli government massively expanded the subsidy for abortion to include all women up to 32 years of age, from the previous cutoff of 19 years of age (see Table 1b).[22] The government decided to use age as a proxy for financially constrained women, and due to budget constraints, capped the funding at 32 years of age (Amsterdamski et al., 29.04.21). 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 at 19 years old, as we describe below.[23]

3 Data

We utilize 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.

3.1 Data Sources

Our administrative data on abortion comes from the abortion committee, which includes every woman who applied to the committee between 2009-2016 and provides information about 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' identifiers), we use the 2016 civil registry data. Combined, the abortion committee data and the civil registry data allow us to identify all of the recorded pregnancies in Israel between January

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

[21]Hedva Eyal, President of the Haifa Women's Coalition, Tel Aviv, Israel, April 2020.

[22]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.

[23]There are both conceptual and empirical reasons to focus only on the younger age cutoff, as we describe in Section 3.2. Conceptually, younger women are more likely to have undesired births and they are making investments in human capital. Empirically, we show that the parallel trends assumption is violated around the older cutoff (32 years old) (Figure C1a). Nonetheless, we show results for specifications around the older age cutoff and using the entire population with two cutoffs designated in the appendix.

2009 and March 2016 with the exception of pregnancies terminated without the permission of the committee (illegal abortions) and miscarriages that occur early in pregnancy.[24] A benefit of this unique data on pregnancies is that it allows us to calculate the *abortion ratio*, defined as the share of pregnancies in a given population that end in abortion, rather than the abortion rate, defined as the number of abortions per 1,000 women of childbearing age. We use tax data composed of a monthly panel of labor market employment, earnings, and sector identifiers from 2005-2018. Finally, we use data from the education registry spanning 2005-2018 that includes data on whether and when a woman enrolled in higher education.

All four of these datasets are provided with a unique, de-identified woman identifier that we use to merge at the woman-pregnancy level and construct a repeated cross-section of pregnancies. We complement our primary datasets with survey data that assessed credit constraints and subjective economic well-being (akin to the Report on the Economic Well-Being of U.S. Households conducted by the U.S. Federal Reserve), which asks 'Can you afford an unexpected bill of $2,000 within a month?' (Peled, 2013).

3.2 Sample Definition: Unmarried 18-21 Year Olds (2009-2016)

Table 2 shows our sample of total observations (i.e., pregnancies) and total women, as we narrow down from the universe of pregnancies in Israel to our primary analytic population of unmarried 18-21 year olds who conceived between January 2009 and March 2016. Below we describe the rationale for each of these restrictions (marital status, age, and time period).

3.2.1 Restrict to Unmarried Women

We restrict our sample to unmarried women for three reasons: (1) all unmarried women are automatically approved by the committee for an abortion; (2) the structure of the 2014 policy; and (3) births among unmarried women are more likely to be undesired (the focus of our analysis).[25] With respect to the first reason, as shown in Table 1a, pregnancies that occur "out of marriage" are automatically approved by the committee. While we can observe women who had an out-of-marriage pregnancy in the abortion data if they were approved for the abortion according to this criteria, we do not observe out-of-marriage pregnancies among the women

[24] Third trimester abortions are captured in the abortion committee data, although late-term spontaneous pregnancy lost is not; however, third trimester abortions (defined as an abortion requested after 24 weeks of gestation) are required to go through a special committee and have been fully subsidized during the span of our data. Overall, these are rare (approximately 250 per year among the entire population of Israel) and we do not include them in our analysis.

[25] Although privacy and stigma could also be very important for married women seeking an abortion, particularly poor, religious married women, because these women are not automatically approved for an abortion and must meet one of the other criteria, there is likely selection in terms of who ends up getting approved.

who gave birth (e.g., pregnancies carried to term that were the result of infidelity).[26] Thus, the most direct way to ensure the comparability of the women who had abortions and those who gave birth is to restrict the sample to all unmarried women, where marital status is identified at the month of conception.

With regards to the second reason, given the prior criteria for government funding, the 2014 expansion changed the funding coverage primarily for unmarried women. As shown in Table 1a, there is substantial overlap between the *approval* criteria and the *funding* criteria and many of the criteria that must be met to have a legal abortion are the same that were fully funded prior to 2014. For example, a 21 year old married woman who is approved for an abortion due to a health risk in 2013 would also have had her abortion fully funded – none of this changes after 2014. In contrast, an unmarried 21 year old woman with no health risks would have had her abortion approved in 2013 but she would have to pay the full cost; after 2014 she would no longer have to pay.[27] In other words, prior to 2014, the only two populations who would be approved for an abortion, but would not receive funding were women whose pregnancies were out of marriage and women aged 40 and older because all other criteria for abortion approval were also fully funded prior to 2014 (Table 1a). Thus, the only population for whom the funding coverage changed in 2014 are the women with an out of marriage pregnancy, which further motivates the restriction to unmarried women.

Finally, a more intuitive reason to focus on unmarried women is that unmarried women are more likely to have unintended pregnancies and undesired births both in Israel and other settings (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), and, as discussed below, this is particularly so for younger women. Figure 1b illustrates that intuition: the abortion ratio among young unmarried women is 71.5%, while among young married women is 0.75%. To restrict our analysis to the population of unmarried women, we combine data on the timing of conception and timing of marriage to identify women who marital status at conception. By considering women who were unmarried at the time of conception, the sample is not affected by the endogenous decision to get married after becoming pregnant. This also allows us to later assess the policy's downstream impact on marriage (see Section E.2.1).

[26] We describe in Appendix B.1 how married women can get around the committee criteria. However, the procedure raises concerns about selection: many women might decide not to pursue the process because it is cumbersome.

[27] This might raise a concern that some women delayed their marriage life before 2013 to keep their free abortion eligibility. Nevertheless, it will be easier to invest more in contraception than delaying these life-changing events.

3.2.2 Restrict to 18-21 Year Olds

Next, 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 that is 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). Ultimately, after restricting our sample to unmarried, 18-21 year-old women, our sample is composed of 24,564 pregnancies across 20,621 women (Table 2).

The conceptual reasons of our age selection are two fold. First, we are focused on identifying the effects of *undesired* births on career outcomes (Section 6), which are important if they result in a significant shock to a woman's economic trajectory. If an undesired birth reduces a woman's labor market prospects, exploring policies that prevent this initial shock is important. While we ultimately cannot observe which births are undesired, Buckles et al. (2019) show that young, unmarried women are more likely to have undesired births than their older counterparts. Indeed, the abortion ratio among our population of interest, unmarried 18-21 year olds, is 67.2% (Figure 1b). While this may sound high, Israel's legal abortion ratio among the entire population is 10%, which is relatively low compared global rates (see Figure (see Figure B2). Twenty-five percent of pregnancies are aborted worldwide, while in Europe the rate is 26% and in North America the rate is 16% (Guttmacher, 2018). Furthermore, it is important to remember that the abortion ratio tends to be higher among young women and particularly among unmarried women as noted above. In our data, the abortion ratio among all 18-21 year olds (married and unmarried) is 14.5%, which is about half the size of the abortion for women under the age of 20 in the United States, which was 29% in 2013 (Kost et al., 2017).

Second, this young age (18-21 years old) 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 22-24 year old women 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 year olds) are not.[28] Military service may pose a threat to our identification if it affects

[28]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

women's fertility decisions. Age-based differences, such as military service, are precisely why we take two differences and are addressed by the difference-in-difference framework.[29]

Finally, we focus our analysis on conceptions occurring between January 2009 and March 2016. We choose 2009 as the starting year because, prior to 2009 19 year olds were not universally funded. This is because many 19 year olds are still serving in the military at this age and the military covers the cost of all medical procedures, including abortion. In 2009, the Israeli government expanded the abortion subsidy to include all women up to the age of 19 (whereas previously only up to the age of 18 was covered).[30] 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.[31] We restrict our analysis to conceptions up until March 2016 for two reasons. First, the latest live births we observe occurred in December 2016, 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).

3.3 Key Variable Definition

Given our focus on young women, economic resources and family religiosity are central to the mechanisms we explore 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.[32] To classify religion and ethnicity for 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. For religiosity, we define

results (Figure C3).

[29]Two additional facts regarding military service in Israel may also help to alleviate concerns. First, most Israeli soldiers are stationed in 'open-bases' meaning their service allows them to return home every day as in a standard job. In contrast, women who serve in 'closed-bases' return home on weekends. Israeli military service, in other words, is unlike American military service, which requires soldiers to be stationed at remote bases. Second, only 20% of religious Jewish women serve in the military. This increases our confidence in our results, because religious Jewish (as well as Arab women that do not serve) are the main populations that are drive our results (as we will show in Section 5.2).

[30]We test for an effect this change in funding coverage for 19 year old in 2009 and find that the policy had a negligible and insignificant effect on abortion, which we interpret as a consequence of the existing funding coverage by the military.

[31]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 produce very similar results to our main effect. Thus, we chose to start from 2009 due to the confusing nature of the military coverage, which makes the interpretation of the staggered difference-in-difference results more complex.

[32]Father's earnings are more predictive of child's future ranking than household or mothers' earnings (see Table A2).

it 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 might change their religiosity level 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.

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 calendaric year. Following Abowd et al. (1999), we estimate the sector-level wage-premiums by running a log-wage regression on individual and firm fixed 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, where 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).[33] This part-time employment measure is defined for the total population of unmarried, 18-21 year women who conceived.

4 Effect of Abortion Subsidy on Abortion Ratio

4.1 Empirical Strategy

To identify the effect of the 2014 policy on abortion ratios, we use a DiD strategy that leverages the timing of the policy change (2014) and the age cutoff (19 years old, highlighted in Table 1b). Specifically, we estimate the following difference-in-difference model on a repeated cross-section of all conceptions that occurred in Israel between January 2009 and March 2016, based on the time (month-year) of conception:

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

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 being in effect ($\mathbb{1}\{t \geq$ Dec-2013$\}$)[34] and

[33] 2011 is the baseline year for our inflation correction.

[34] The policy expanding the abortion subsidy went into effect in January 2014. However, women who conceived

T_i indicates that woman i is eligible for the subsidy ($\mathbb{1}\{20 \leq age \leq 21\}$). The coefficient on the interaction between $Post$ and T is the standard difference-in-differences effect (δ). 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 effect (γ_{m_t}) are used to control for age-invariant time trends and seasonality that affect abortion ratios and fertility. Lastly, X_i represents a set of pre-pregnancy controls (ethnicity, education, yearly earnings, months worked) which we include non-parametrically in some specifications as a robustness test. Standard errors are clustered at the age-at-conception level.

Our difference-in-difference approach assumes parallel trends: the population of women eligible for the subsidy would have experienced similar changes in the abortion ratio over time as ineligible women in the absence of the 2014 subsidy. To assess the validity of this assumption, we plot in Figure 2 the mean abortion ratio by group and year, and the corresponding 95% confidence intervals around each point estimate. The fitted lines represent the estimated linear pre-trend for each group (and extrapolated post-policy). Intuitively, this serves as a visual illustration of the effect, taking into account differential pre-trends (a la Agha and Zeltzer (2022)). In general, the trend in the abortion ratio is quite parallel for the treated (20-21 year olds) and untreated (18-19 year olds) in the pre-period, although there is a slight narrowing of the difference for the years closer to 2014. In 2014 we observe a significant increase in the abortion ratio beyond the pre-trends for the treated 20-21 year olds, and no significant difference relative to pre-trends for the untreated 18-19 year-olds, as would be expected if the subsidy expansion affected the abortion ratio among newly eligible women.[35]

4.2 Results: Increase in the Abortion Ratio

We find that removing the abortion cost increased the abortion ratio by 4.6 percentage points relative to younger women who were already subsidized, as reported in Table 3. This is equiv-

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 even have been discovered until January 2014 when the policy was already in place, and thus should be considered treated.

[35]Appendix C presents alternative parallel trends assessments for the 18-21 year olds, as well as for two alternative populations: 30-35 year olds, where the 33 year old age cutoff was used to determine treatment; and the full sample of women aged 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)). We also present a generalized difference-in-difference, which interacts treatment with individual year fixed effects to test for pre-trends, shown in Figure C5, and finds no statistical difference in the abortion ratio 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 appendices as appropriate.

alent to an increase of approximately 7%, given the baseline abortion ratio of of 66% among unmarried women 18-21 years old who conceived. The 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, which is consistent with a lag in awareness of the policy.[36]

Our baseline specification follows Equation 1, as described in Section 4.1. Estimating it without controls ("DiD") we find a 4.6 percentage point increase in the abortion ratio. In Table 3 we perform several robustness tests. First, we test the robustness of the DiD to the inclusion of pre-pregnancy woman-level characteristics ("DiD + controls"), which reduces the estimated effect to 3.2 percentage points.[37] To address the potential risk to identification that women aged 18-19 were on a different (time) trend then women aged 20-21, 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 detail on this approach). This result is presented in the third column of Table 3 and is similar in magnitude to the "DiD + controls" specification. A potential concern with our DiD analysis is that other factors unrelated to the 2014 policy might have differentially affected the abortion decision of 20-21 year olds relative to the 18-19 year olds.[38] To address this concern, we also estimate a triple difference approach, using married women aged 18-21 as the third difference. The magnitude and significance of the effect in the triple difference (column 3) are comparable to the main effect (column 2) in Table 3, serving as stronger evidence for the exogeneity of the policy. Overall, our main finding is robust to these alternative specifications and tests.[39]

5 Potential Channels

In the previous section, we demonstrated that providing abortion free of charge increases the abortion ratio. It may be counterintuitive that eliminating such a small cost (the co-pay for

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

[37]Our results are qualitatively similar with and without controls. Nevertheless, recent work by Blandhol et al. (2022) suggests the LATE interpretation does not apply if we include controls in the subsequent IV estimation we do in Section 6. Therefore, we prefer for the sake of clarity to keep the 'no controls' specification as the baseline in our estimation procedure.

[38]One valid concern is that women in Israel serve two years in the Israeli Defense Force (IDF) between 18-20, which might suggest our control group is invalid. However we should note two contextual details that makes us less concerned about this. First, the IDF funds abortion for soldiers, which means our control group was funded in both cases. Second, while 60% of women serve in the IDF, only 20% of religious Jewish do, which are the population that are driving our results (as shown in Section 5).

[39]For the sake of completeness, Figure A1 presents these estimates while varying both the specification (DiD, DiD + controls, DDD, LTT) and the population group (18-21, 30-35, 16-40).

the abortion) relative to the cost of raising a child would result in such a large effect.[40] In this section we explore potential underlying mechanisms.

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). The canonical model in economics is the "abortion as insurance" model, in which 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 decision about contraception intensity, which implies an unplanned conception will happen with some probability (see Decision I and II in Figure 3).[41]

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 in the country (not only those who conceived), aged 18-21 and test whether the policy impacted the probability of conception. We use the same empirical design presented in Section 4; the results are presented in Figure 4. We find a small and insignificant effect across a range of specifications, suggesting no evidence for moral hazard in our setting.[42]

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 contraception use, including emergency contraception.[43] Although neither the availability nor the cost of emergency contraception changed in Israel during this time period, if the change in abortion policy led to an increase in risky sexual behavior, these potential pregnancies could have been prevented using emergency contraception. One could argue that if women are paying out of pocket for emergency contraception, it is not truly moral hazard in the canonical sense. Nonetheless, if this were occurring it would represent a change in sexual behavior that undermines our test.

The lack of evidence of moral hazard in this setting may be explained by the type of policy

[40]Note that child-rearing in Israel is orders of magnitude cheaper than in the U.S. because of Israel's pronatalist policies. For example, both education and healthcare are public and universal in Israel (see Appendix B.3 for more details).

[41]The full model adds incomplete information about the conditions under which the birth will occur. For the sake of simplicity, we abstract from the incomplete information channel (though implications are the same). We provide a description of the full model in Appendix D.1.

[42]Conducting a similar exercise in levels shows a *decrease* in births, reinforcing the lack of evidence for moral hazard (see Figure A2).

[43]Emergency contraception, or Plan B, has been available in Israel since 2002 (Efrati, 2019). It is available over the counter from pharmacies and in 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 in order to purchase (Joanne Zack Pakes et al., 2015)

change we analyzed. The "abortion as insurance" literature suggests moral hazard depends on the context. 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., shift from abortions available only to those with medical problems to abortion available on demand) result in moral hazard, while large changes, such as legalization, did result in moral hazard. In contrast, 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, suggesting moral hazard is present in response to a large policy change, such as legalization, in the U.S. context. Our finding suggests no evidence of moral hazard in a context where abortion is already legal and a more moderate policy change, providing abortion free of charge, was implemented.[44]

5.2 The Role of Social Stigma and Financial Constraints

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 years of age) implies that they are largely financially dependent on their family's resources. To proxy for family resources we split our population into two groups, women from low-earning and high-earning families (SES) using their parental income. 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 A3a).

Therefore, we propose a more nuanced explanation, motivated by the Haifa Women's Coalition's experience that women from religious backgrounds in particular struggled to pay for the abortion.[45] 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;[46] thus, when abortion is provided for free, these constraints are relaxed. This theory is supported by the descriptive evidence presented in Section 2, which shows that baseline abortion ratios are highest among women from higher-earning households (Figure 1a) and lowest among women from more religious backgrounds (Figure 1c).

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

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

[46] 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, suggesting that financial constraints, or at least perceived constraints, are binding at these cost levels for young Israeli women.

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 (Figure A3b), providing further support of this hypothesis.

We modify the 'abortion as insurance' model, presented in Figure 3, to include these social and financial constraints (formally defined in Appendix D). The updated model implies two testable hypotheses: first, baseline abortion ratios should be the lowest among the more socially and financially constrained women; and second, the effect of the policy, which removed those constraints, should be higher for women who are more socially and financially constrained.

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 social unacceptability of the abortion SU (which could also mean personal opposition to abortion). Finally, a woman who is from the same social group but is financially constrained (bottom right) will face both the credit constraints and the social cost $CC \times SU$.

To test for evidence of this mechanism, we proxy for financial constraints using the woman's father's earnings (split according to above median, or "High SES", and below median, or "Low SES", yearly earnings in Figure 5b). 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 the religious Jews as not accepting of abortion.[47] 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.[48] Figure 5d presents the estimates of the effect in percentage change relative to the

[47]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; in the Jewish population we can observe religiosity. Additionally, the 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.

[48]For robustness we run all four groups in one regression with interaction term for each and find qualitatively similar results.

baseline (see Figure 5c for the effect in percentage points). We can see that the effect of the policy on the abortion ratio follows the logic presented in Figure 5a: the highest (and only significant) effect is concentrated among poor, religious Jewish women.[49,50] The magnitude of the effect is also quite large – among the poor, religious women, removing the cost of abortion increased the abortion ratio by 13.4 percentage points, which is equivalent to a 25.1% increase relative to the baseline mean among this population. In contrast, the percentage change among the entire population of young, unmarried women is 7% (Figure A1).

These results suggest that the combined relaxation of social and financial constraints is the primary driver of the increase in the abortion ratio ($CC \times SU$ constraints).[51] This finding differs from that of Ananat et al. (2009), who find that legalizing abortion in the U.S. had a larger impact on abortion in liberal states than in conservative states. They explain that the difference is due 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 our results 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,[52] the social component of the latent cost did not change with legalization in the US. Although abortion legalization in the U.S. reduced the cost of an abortion (via reduced 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 U.S. 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 in more conservative U.S. states after legalization. But in the Israeli case, the 2014 policy enabled women who could

[49]One concern is that the poor-religious population is poorer than the poor-secular population and, thus, faces greater financial constraints. Figure A5 suggests otherwise: her parents' earnings are similar across both groups (Panels c and d).

[50]Another alternative 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, since some women are discharged around age 20 which might be attenuate the results for them. To test that we ran a simulation in which we assume the true treatment effect is the same across groups and check how much after turning 20 these women need to be released in order to explain the heterogeneity. We find the stark difference in the effects requires 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.

[51]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.

[52]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.

not afford an abortion to avoid asking for financial help, which increased the *privacy* of the decision and helped them avoid the social cost.[53] 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 and conservative or religious women in other settings.[54]

5.3 Alternative Explanations

Finally, we explore two additional explanations of what could be driving the increase in the abortion ratio: (1) a change in personal moral views regarding abortions and (2) substitution from the illegal to legal market for abortion. We find minimal evidence for either.[55]

To explore the first explanation, we consider whether a shift in personal moral views about abortions was precipitated by the policy. 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 abortion ratio to increase among women who were unaffected by the policy change.[56] Figure A4, which presents a first-differences exercise by age, shows no significant change in the abortion ratio among age groups ineligible for the subsidies.

Finally, we explore the second 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 the interpretation of our results in two ways. First, the increase in abortion ratios that we observe might not be an absolute increase but a substitution away from the illegal

[53]To further support the importance of privacy, we ran a complementary analysis on a 2004 policy that eliminated the requirement that 18 year olds obtain and documentation from their HMO 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 and may be concerned about the abortion record being shared. This small policy change resulted in a 1% increase in the abortion ratio (see Appendix F for more details).

[54]For example, the Medicaid-eligible population in the U.S. may be somewhat similar to the low-SES population in our study. Since 1976, the Hyde Amendment has banned the use of Medicaid funds to pay for abortions, except in cases of rape, incest, or life endangerment, although states may choose to allocate their state Medicaid budget for abortion coverage. Thus, in the U.S., most low-income women on Medicaid would be 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.

[55]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 strong evidence that supports it. If the price effect is occurring, we should see some effect among secular women because of the lower latent cost of abortions. However, as suggested in Figure 5d, there is no effect among constrained or unconstrained women who live in a socially accepting society.

[56]Note that this puts SUTVA (the stable unit treatment value assumption) at risk. That is, the 18-19 year-olds, too, are potentially affected by the treatment (through changing moral views) and, hence, they are not an untreated control group. Given the small and insignificant shift in the untreated groups shown in Figure A4, 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). Consequentially, our identification strategy will still hold.

to the legal market in response to the subsidy. Second, responding 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.[57] 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 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.[58] The results in Figure A2 show an increase of five abortions per age-month and a corresponding 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 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 abortion and thus indicative of some spillover from the illegal market to the legal. Additionally, our test above for moral hazard (Figure 4) also 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 see no change in conceptions. Ultimately, while we cannot neither fully rule out spillover nor a price response from the illegal market, we do not believe the presence of either is sufficiently large to undermine our main results.

6 Downstream Social and Economic Effects

After establishing that making abortion free increased the abortion ratio, we shift to examining the effect this change had on women's medium-term fertility, marital status, human capital

[57] Another possibility is to test for the likelihood that such women would end up in a hospital due to complications. This is a valid strategy, but we do not have data on hospital complications. Additionally, many illegal abortions in Israel 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.

[58] 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). 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.

investment, and labor market outcomes. We focus our analysis in this section on the socially and financially constrained women, given the results in Section 5.2. Nevertheless, the results for the entire population are qualitatively consistent and presented in Appendix Table A3.

6.1 Empirical Strategy

A possible (naive) strategy is to compare post-conception outcomes of women who aborted to women who gave birth with a standard OLS. Consider this naive OLS estimation for the effect of having an abortion on employment (or any other socioeconomic outcome):

$$y_i^{Post} = \theta^{OLS} \cdot abort_i + \rho^{OLS} \cdot y_i^{Pre} + \gamma_{a_i} + \gamma_{c_i} + \epsilon_i^{OLS} \tag{2}$$

where y_i^{Post} is the mean outcome of woman i in the year of and through three year 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.[59]

This approach is problematic because women who have abortions are systematically different from those who give birth, and this difference affects labor market outcomes (see Column 9 in Table A1).[60] Beyond selection, the naive OLS approach provides an estimate for the *average* woman who decided to abort her pregnancy, but from a policy perspective it may be more interesting to understand the downstream consequences of avoiding undesired parenthood among women who were *constrained* from having an abortion.

The ideal experiment for studying an undesired birth impact would be to randomly assign whether women have children and compare labor market outcomes between women who ended up with kids to those who did not. In the absence of such an experiment, we use the 2014 policy, which eliminated the monetary cost of abortion, as a natural experiment to estimate the effect of avoiding an undesired birth on education and labor market outcomes, focusing explicitly on the sub-population of constrained women (our compliers) who grew up in poor and religious backgrounds. 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).[61]

Formally, we estimate the following:

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

[60] Specifically, they suggest negative selection on earnings and education.

[61] Our detailed panel data on fertility, employment, and education allows us to go beyond average effects post-conception and examine the temporal dynamics of these effects, which can reveal a more nuanced story, which we present in Appendix E.

$$\text{2nd Stage: } y_i^{Post} = \theta^{IV} \cdot \widehat{abort}_i + \rho^{IV} \cdot y_{c_i}^{Pre} + \gamma_{a_i} + \gamma_{c_i} + \epsilon_i^{IV} \tag{3}$$

$$\text{1st Stage: } abort_i = \delta \cdot Post \cdot T_i + \rho^{abort} \cdot y_{c_i}^{Pre} + \gamma_{a_i} + \gamma_{c_i} + \epsilon_i^{abort} \tag{4}$$

$$\text{Reduced Form: } y_i^{Post} = \theta^{RF} \cdot Post \cdot T_i + \rho^{RF} \cdot y_{c_i}^{Pre} + \gamma_{a_i} + \gamma_{c_i} + \epsilon_i^{RF} \tag{5}$$

As in Equation 1, *Post* is an indicator for the policy being in effect ($\mathbb{1}\{c_i \geq \text{Dec-2013}\}$) and T_i indicates woman i is eligible for the subsidy ($\mathbb{1}\{20 \leq age_{c_i}\}$). The rest of the terms are defined as in Equation 2.

Ex-ante, one should expect two types of differences between the OLS and the IV estimates. First, the OLS suffers from the standard selection problem (described above). Second, the compliers are women who could not afford \$600-\$1,000 to perform the abortion. Therefore, the IV is estimating a local average treatment effect (LATE) of removing the financial constraints of having the abortion among a disadvantaged population. The exclusion restriction in our case implies that the only channel through which the 2014 subsidy policy affects labor market outcomes is by changing the probability of having a child. While this assumption is not directly testable, we argue that it is plausible because the policy only changed the cost of having an abortion without changing the expected benefits of having an abortion or any other fertility-related policies.[62]

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 the increase in abortion allowed women to delay parenthood. For this purpose we define a binary parenthood outcome in any of the subsequent three year ($\mathbb{1}\{\text{Is a parent}\}$) and estimate Equations 2 - 5.

We present the OLS, IV, and RF results in Table 4 across a range of demographic outcomes. The results supports our prior – 11.7 p.p. decrease in parenthood in the subsequent three years following the pregnancy they aborted (relative to a baseline share of 61%). Similarly, conditional on giving birth in the subsequent four years, women's age at first birth increased on average by 0.56 years. However, it is important to note this result is a censored average, three years after year of conception. Perhaps as a result of the censoring, we do not find a significant effect on the total number of children among the socially and financially constrained women.

[62] One potential violation of the exclusion restriction is via the always takers that are now getting a lump-sum transfer at the size of the abortion subsidy. While we cannot test directly for this assumption, we show in Table A3 the effects are consistent (and if anything stronger) in the socially and financially constrained population - where we have a higher rate of compliers, suggesting the estimated effects are driven by the share of compliers.

Avoiding an undesired birth might also reduce undesired marriage, because women who have an undesired pregnancy may end up marrying the father. The data seems to support that claim: 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 years following the index pregnancy (relative to a baseline of 39%). These results suggest that the removal of financial constraints allowed the constrained women to avoid undesired parenthood *and* a subsequent undesired marriage.[63]

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 - 5 on university enrollment (see Table 4). Our reduced from specification finds a 1.3 percentage point increase in the probability of university enrollment (relative to a baseline of 5.4%), which continues to increase over the three years following the potential undesired birth (Figure E2c). This result, coupled with the delay in parenthood and marriage, is consistent with the logic in Goldin et al. (2006), who argue that the reversal of the gender gap in college graduation appears to be 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.

Does this investment in human capital translate into higher earnings? Our results in Table 4 find a statistically insignificant decrease in yearly earnings. However, in Appendix E we explore a dynamic empirical strategy and find a *temporary* increase in yearly earnings unconditional on working (see Figure E2d). Specifically, the yearly earnings (unconditional on working) of socially and financially constrained women increased in the year of potential birth by $1,020 due to the policy (out of a baseline at year of potential birth of $5,806). Appendix Figure E3 presents a very similar temporary increase in total months worked in the year of potential 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 from abortion, the counterfactual woman would have given birth to a child, and so she would likely have taken maternity leave or reduced months worked. Now, the same women are unconstrained, able to avoid an undesired birth, and, as a result, are not taking time off. Since these women are more likely to go to college, they substitute away from time spent working.

Next we explore whether avoiding an undesired birth increases or decreases employment. The 'child-penalty' literature suggests a large and persistent decrease in employment after

[63]This finding is consistent with recent work by Gershoni and Low (2021) which finds a substantial increase in average age at first marriage following Israel's 1994 adoption of free in vitro fertilization.

becoming a mother, but is that the case for *undesired* parenthood as well? To answer that we need first to understand what happens to young unmarried religious women in Israel who conceive. According to the International Social Survey Programme (ISSP), 80% of Israelis believe a 'women with children under school age should work outside the home', the highest share in the OECD (Kleven et al., 2019a). Our data supports this claim in our sub-population – 77.7% of our young unmarried socially and financially constrained women who conceived are working in the year of conception (see Table 4).[64] Therefore, avoiding the need to provide for a newborn child as a young unmarried woman could result in a temporary *decrease* in employment. Indeed, we find a 5.8 percentage points decrease in employment in the short to medium term.

We also observe a shift from full-time to part-time and self employment due to the increase in abortion access.[65] Specifically, we find an increase of 5.5 percentage points in the share of socially and financially constrained 18-21 year old unmarried women who conceived and worked part-time (conditional on working) due to the elimination of the abortion cost. These results are consistent with substitution towards human capital investment: the counterfactual women who could not have had the abortion before the policy, worked full-time to provide for their baby. When abortion is provided for free, they are more likely to invest in their human capital, but because they have less time while completing their education, they shift to part-time and self-employment because of the flexibility it affords.[66]

Finally, we ask whether the human capital investment translates into employment in better-paying jobs. To answer this question, we estimate Equations 2 - 5 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 4 suggest an increase of 0.015 log-points (out of a baseline of 0.03 log-points) in the wage-premium of the sector in which these women work. Although our three years of data following the 2014 policy somewhat limit our ability to examine the policy's full labor-market consequences, our results suggest the removal of abortion cost resulted in an investment in human capital and a shift towards better paying jobs over the short-to-medium term.

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

[65]We define part-time work if a woman earns less than the minimum monthly earnings, mandated by law (conditional on working) – 3,890 NIS a month (USD $1,090) from the Social Security website. This serves as a lower bound on part-time employment.

[66]See Goldin (2014) for a discussion on the importance of flexible employment in closing the gender wage gap.

7 Conclusion

In this paper, we study the economic consequences of expanding access to free abortion services. Abortion access is currently being discussed across the world, often in highly charged moral and ethical debates, many of which focus on whether abortions should be *legal* (e.g., recent examples from Argentina, Mexico, and U.S. states such as Texas and Mississippi). However, in settings where abortion is already legal, the financial cost can impose barriers to access. Increasingly policymakers are turning to this question of removing or relaxing financial barriers (Gutierez, 2021; Heyward, 2022; Livsey, 2017), thus understanding the impacts of removing such barriers is particularly timely. We take advantage of a change in 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 abortions increases the abortion ratio. The magnitude of the effect is meaningful: the 4.6 percentage point increase represents a 7%, given the baseline abortion ratio of of 66% among unmarried women 18-21 years old who conceived.

Our analysis investigates two primary mechanisms that explain this increase. First, we examine whether the policy induced a moral hazard response, where women reduce contraception use because abortion has become 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 the abortion ratio 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 career. 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, assume more flexible employment arrangements while completing their studies, and work in sectors with a higher wage-premium.

Our findings are relevant for other settings in which abortion is legal but may be costly, although more research is warranted. 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 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 the abortion ratio, but the mechanisms and particularly the role of *privacy* and

social stigma have not been investigated in this setting.[67] Additionally, a limitation of our analysis on career consequences of the Israeli policy is the limited to a relatively short time-span that follows the policy change and is restricted to young, unmarried women aged 18-21. A promising future avenue of research would examine the long-term effects of funding schemes and focus on women who abort at older ages. Overall, our analysis shows that providing free abortion can be a powerful policy tool, allowing women to *time* parenthood and increase their early career investment, while granting them the privacy to make personal reproductive decisions.

[67]Cook et al. (1999) shows the importance of funding – interruptions in funding resulted in a third of pregnancies that would have been aborted carried to term instead. Similarly, Levine et al. (1996) shows that restrictions on Medicaid funding resulted in a reduction in the abortion ratio.

References

John Abowd, Francis Kramarz, and David Margolis. High Wage Workers and High Wage Firms. *Econometrica*, 67(2):251–333, 1999.

Leila Agha and Dan Zeltzer. Drug Diffusion through Peer Networks: The Influence of Industry Payments. *American Economic Journal: Economic Policy*, 14(2):1–33, May 2022. ISSN 1945-7731. doi: 10.1257/pol.20200044.

George A. Akerlof, Janet L. Yellen, and Michael L. Katz. An Analysis of Out-of-Wedlock Childbearing in the United States. *Q J Econ*, 111(2):277–317, May 1996. ISSN 0033-5533. doi: 10.2307/2946680.

Delila Amir. *The silencing of abortin in Israel: A feminist and international perspective, institutional and personal dilemmas*. Sociology and Anthropology series. Israel, 2015.

Shaul Amsterdamski, Liel Kyzer, and Riad Agberia. Radio conversation, 29.04.21.

Elizabeth Oltmans Ananat and Daniel M. Hungerman. The Power of the Pill for the Next Generation: Oral Contraception's Effects on Fertility, Abortion, and Maternal and Child Characteristics. *Rev Econ Stat*, 94(1):37–51, February 2012. ISSN 0034-6535. doi: 10.1162/REST_a_00230.

Elizabeth Oltmans Ananat, Jonathan Gruber, and Phillip Levine. Abortion Legalization and Life-Cycle Fertility. *The Journal of Human Resources*, 42(2):375–397, 2007. ISSN 0022-166X.

Elizabeth Oltmans Ananat, Jonathan Gruber, Phillip B Levine, and Douglas Staiger. Abortion and Selection. *The Review of Economics and Statistics*, 91(1):124–136, January 2009. ISSN 0034-6535. doi: 10.1162/rest.91.1.124.

Joshua Angrist and William Evans. Schooling and Labor Market Consequences of the 1970 State Abortion Reforms. NBER Working Paper 5406, National Bureau of Economic Research, Inc, January 1996.

S Anukriti, Catalina Herrera-Almanza, and Mahesh Karra. Bring a Friend: Strengthening Women's Social Networks and Reproductive Autonomy in India. page 63, January 2022.

Nava Ashraf, Erica Field, and Jean Lee. Household Bargaining and Excess Fertility: An Experimental Study in Zambia. *American Economic Review*, 104(7):2210–2237, July 2014. ISSN 0002-8282. doi: 10.1257/aer.104.7.2210.

Martha Bailey. More power to the pill - Impact of contraceptive freedom on women's life cycle labor supply. *The Quarterly Journal of Economics*, 2006.

Martha J. Bailey, Brad Hershbein, and Amalia R. Miller. The Opt-In Revolution? Contraception and the Gender Gap in Wages. *Am Econ J Appl Econ*, 4(3):225–254, July 2012. ISSN 1945-7782. doi: 10.1257/app.4.3.225.

Akinrinola Bankole, Susheela Singh, and Taylor Haas. Characteristics of Women Who Obtain Induced Abortion: A Worldwide Review. *International Family Planning Perspectives*, 25(2): 68–77, 1999. ISSN 0190-3187. doi: 10.2307/2991944.

Yitzhack Barilovich. Abortion Payment for Women Younger than 18. Technical report, Ministry of Health, 2004.

M Antonia Biggs, Heather Gould, and Diana Greene Foster. Understanding why women seek abortions in the US. *BMC Women's Health*, 13(1):29, December 2013. ISSN 1472-6874. doi: 10.1186/1472-6874-13-29.

Marianne Bitler and Madeline Zavodny. The effect of abortion restrictions on the timing of abortions. *Journal of Health Economics*, 20(6):1011–1032, 2001.

Christine Blandhol, John Bonney, Magne Mogstad, and Alexander Torgovitsky. When is TSLS Actually LATE? Working Paper 29709, National Bureau of Economic Research, January 2022.

Kasey Buckles, Melanie E Guldi, and Lucie Schmidt. Fertility Trends in the United States, 1980-2017: The Role of Unintended Births. Working Paper 25521, National Bureau of Economic Research, January 2019.

Central Bureau of Statistics (Israel). Religion and Self-Definition of Extent of Religiosity Selected Data from the Society in Israel. Technical Report 10, June 2018.

Philip J. Cook, Allan M. Parnell, Michael J. Moore, and Deanna Pagnini. The effects of short-term variation in abortion funding on pregnancy outcomes. *Journal of Health Economics*, 18 (2):241–257, April 1999. ISSN 0167-6296. doi: 10.1016/S0167-6296(98)00048-4.

John J. Donohue and Steven D. Levitt. The Impact of Legalized Abortion on Crime. *Q J Econ*, 116(2):379–420, May 2001. ISSN 0033-5533. doi: 10.1162/00335530151144050.

John J. Donohue, Jeffrey Grogger, and Steven D. Levitt. The impact of legalized abortion on teen childbearing. *American Law and Economics Review*, page ahp006, 2009.

Martin Eckhoff Andresen and Tarjei Havnes. Child care, parental labor supply and tax revenue. *Labour Economics*, 61:101762, December 2019.

Ido Efrati. 14 years old can buy Plan B without prescription. *Haaretz*, December 2019.

Teana Einav, Dolev Korlinsky, and Anika Yifrah. Israel National Health Interview Survey 2013-2015. Technical report, ICDC, 2017.

Stefanie Fischer, Heather Royer, and Corey White. The impacts of reduced access to abortion and family planning services on abortions, births, and contraceptive purchases. *Journal of Public Economics*, 167(C):43–68, 2018.

Diana Greene Foster, M. Antonia Biggs, Lauren Ralph, Caitlin Gerdts, Sarah Roberts, and M. Maria Glymour. Socioeconomic Outcomes of Women Who Receive and Women Who Are Denied Wanted Abortions in the United States. *American Journal of Public Health*, 108(3): 407–413, March 2018. ISSN 0090-0036, 1541-0048. doi: 10.2105/AJPH.2017.304247.

Itay Gal. Abortion Pill Was Approved for Use Three Months Past Conception. https://www.ynet.co.il/articles/0,7340,L-4785472,00.html, March 2016.

Naomi Gershoni and Corinne Low. Older Yet Fairer: How Extended Reproductive Time Horizons Reshaped Marriage Patterns in Israel. *American Economic Journal: Applied Economics*, 13(1):198–234, January 2021. ISSN 1945-7782, 1945-7790. doi: 10.1257/app.20180780.

Claudia Goldin. A Grand Gender Convergence: Its Last Chapter. *American Economic Review*, 104(4):1091–1119, 2014.

Claudia Goldin and Lawrence F. Katz. The Power of the Pill: Oral Contraceptives and Women's Career and Marriage Decisions. *Journal of Political Economy*, 110(4):730–770, August 2002. ISSN 0022-3808, 1537-534X. doi: 10.1086/340778.

Claudia Goldin, Lawrence F Katz, and Ilyana Kuziemko. The Homecoming of American College Women: The Reversal of the College Gender Gap. *Journal of Economic Perspectives*, page 28, 2006.

Libertad González, Sergi Jiménez-Martín, Natalia Nollenberger, and Judit Vall Castello. The Effect of Access to Legal Abortion on Fertility, Marriage, and Long-term Outcomes for Women. 2022.

Melody Gutierez. California lawmakers prepare to protect abortion access, starting with eliminating copays. *Los Angeles Times*, December 2021.

Guttmacher. Induced Abortion Worldwide. Technical report, Guttmacher Institute, March 2018.

Guttmacher Institute. Medicaid Funding of Abortion. https://www.guttmacher.org/evidence-you-can-use/medicaid-funding-abortion#, January 2020.

Giulia Heyward. Maryland Lawmakers Expand Abortion Access, Overriding Governor's Veto. *The New York Times*, April 2022. ISSN 0362-4331.

Israel Defense Forces (IDF), Medical Corps, Adi Kuperman-Shani, Tarif Bader, Elon Glassberg, and Vered Klaitman. Policy for reducing unplanned pregnancies and repeat unplanned pregnancies rates in Israeli Defense Force. *Isr J Health Policy Res*, 8(1):21, December 2019. ISSN 2045-4015. doi: 10.1186/s13584-019-0292-x.

Jenna Jerman, Rachel K Jones, and Tsuyoshi Onda. Characteristics of U.S. Abortion Patients in 2014 and Changes Since 2008. Technical report, Guttmacher Institute, New York, May 2016.

Joanne Zack Pakes, Amos Ber, Daniel Seidman, Sharon Sela Ktzav, and Ruth Geist. Country-by-Country Information on Emergency Contraception. https://www.ec-ec.org/emergency-contraception-in-europe/country-by-country-information-2/, 2015.

Kelly Jones and Mayra Pineda-Torres. TRAP'd Teens: Impacts of Abortion Provider Regulations on Fertility & Education. page 73, 2021.

Thomas J Kane and Douglas Staiger. Teen motherhood and abortion access. *The Quarterly Journal of Economics*, 111(2):467–506, 1996.

Megan L. Kavanaugh and Jenna Jerman. Contraceptive method use in the United States: Trends and characteristics between 2008, 2012 and 2014. *Contraception*, 97(1):14–21, January 2018. ISSN 1879-0518. doi: 10.1016/j.contraception.2017.10.003.

Yaron Kelner. Basket of medicine 2014: free abortions withour medical reasoning. *Ynet*, December 2013.

Henrik Kleven, Camille Landais, Johanna Posch, Andreas Steinhauer, and Josef Zweimüller. Child Penalties across Countries: Evidence and Explanations. *AEA Papers and Proceedings*, 109:122–126, May 2019a.

Henrik Kleven, Camille Landais, and Jakob Egholt Søgaard. Children and Gender Inequality: Evidence from Denmark. *American Economic Journal: Applied Economics*, 11(4):181–209, October 2019b.

Kathryn Kost, Isaac Maddow-Zimet, and Alex Arpaia. Pregnancies, Births and Abortions Among Adolescents and Young Women In the United States, 2013: National and State Trends by Age, Race and Ethnicity. Technical report, Guttmacher Institute, September 2017.

Phillip Levine and Douglas Staiger. Abortion as Insurance. Technical Report w8813, National Bureau of Economic Research, Cambridge, MA, February 2002.

Phillip B. Levine. *Sex and Consequences: Abortion, Public Policy, and the Economics of Fertility*. Princeton University Press, July 2007. ISBN 978-0-691-13045-3.

Phillip B. Levine and Douglas Staiger. Abortion Policy and Fertility Outcomes: The Eastern European Experience. *The Journal of Law and Economics*, 47(1):223–243, April 2004. ISSN 0022-2186. doi: 10.1086/380475.

Phillip B. Levine, Amy B. Trainor, and David J. Zimmerman. The effect of Medicaid abortion funding restrictions on abortions, pregnancies and births. *Journal of Health Economics*, 15: 555–578, 1996.

Melanie Lidman. As ultra-Orthodox women bring home the bacon, don't say the F-word. *The Times of Israel*, January 2016.

Jason Lindo, Caitlin Myers, Andrea Schlosser, and Scott Cunningham. How Far Is Too Far? New Evidence on Abortion Clinic Closures, Access, and Abortions. *J. Human Resources*, pages 1217–9254R3, May 2019. ISSN 0022-166X, 1548-8004. doi: 10.3368/jhr.55.4.1217-9254R3.

Jason M Lindo, Mayra Pineda-Torres, David Pritchard, and Hedieh Tajali. Legal Access to Reproductive Control Technology, Women's Education, and Earnings Approaching Retirement. page 9, 2020.

Anna Livsey. How much do abortions cost across Australia? – explainer. *The Guardian*, August 2017. ISSN 0261-3077.

Yao Lu and David J. G. Slusky. The Impact of Women's Health Clinic Closures on Fertility. *American Journal of Health Economics*, 5(3):334–359, July 2019. ISSN 2332-3493. doi: 10.1162/ajhe_a_00123.

Ofer Malamud, Cristian Pop-Eleches, and Miguel Urquiola. Interactions Between Family and School Environments: Evidence on Dynamic Complementarities? Working Paper 22112, National Bureau of Economic Research, March 2016.

Sarah Miller, Laura R Wherry, and Diana Greene Foster. The Economic Consequences of Being Denied an Abortion. page 40, 2019.

Caitlin Myers and Daniel Ladd. Did parental involvement laws grow teeth? The effects of state restrictions on minors' access to abortion. *Journal of Health Economics*, 71:102302, May 2020. ISSN 01676296. doi: 10.1016/j.jhealeco.2020.102302.

Caitlin Knowles Myers. The Power of Abortion Policy: Reexamining the Effects of Young Women's Access to Reproductive Control. *Journal of Political Economy*, 125(6):2178–2224, December 2017. ISSN 0022-3808, 1537-534X. doi: 10.1086/694293.

Caitlin Knowles Myers and Daniel Ladd. Did Parental Involvement Laws Grow Teeth? The Effects of State Restrictions on Minors' Access to Abortion. SSRN Scholarly Paper ID 3029823, Social Science Research Network, Rochester, NY, August 2017.

Marissa Newman. 15,000 illegal abortions performed in Israel each year, activists claim. *The Time of Israel*, January 2017.

Michelle Oberman. Abortion Talmud: Interrogating the Nature and Purpose of Abortion Laws in the 21st Century. 2020.

Micki Peled. 70% of the Israeli Population Cannot Efford a surprise 8,000 NIS Expanses. *Calcalist*, August 2013.

Cristian Pop-Eleches. The impact of an abortion ban on socioeconomic outcomes of children: Evidence from Romania. *Journal of Political Economy*, 114(4):744–773, 2006.

Cristian Pop-Eleches. Abortion and Child Cognitive Outcomes. Technical report, mimeo, Columbia University, 2009.

Cristian Pop-Eleches. The supply of birth control methods, education, and fertility evidence from Romania. *Journal of Human Resources*, 45(4):971–997, 2010.

Troy Quast, Fidel Gonzalez, and Robert Ziemba. Abortion Facility Closings and Abortion Rates in Texas. *INQUIRY*, 54:0046958017700944, January 2017. ISSN 0046-9580. doi: 10.1177/0046958017700944.

Misgav Rottenstreich, Limor Loitner, Shir Dar, Ron Kedem, Noam Smorgick, and Zvi Vaknin. Unintended pregnancies among women serving in the Israeli military. *Contraception*, 96(1): 62–65, July 2017. ISSN 0010-7824, 1879-0518. doi: 10.1016/j.contraception.2017.03.006.

Misgav Rottenstreich, Hen Y. Sela, Limor Loitner, Noam Smorgick, and Zvi Vaknin. Recurrent unintended pregnancies among young unmarried women serving in the Israeli military. *Isr J Health Policy Res*, 7(1):42, December 2018. ISSN 2045-4015. doi: 10.1186/s13584-018-0239-7.

Gilla K. Shapiro. Abortion law in Muslim-majority countries: An overview of the Islamic discourse with policy implications. *Health Policy Plan*, 29(4):483–494, July 2014. ISSN 0268-1080. doi: 10.1093/heapol/czt040.

Fabienne Sikron, Rachel Wilf-Miron, and Avi Israeli. [Adolescent pregnancy in Israel: a methodology for rate estimation and analysis of characteristics and trends]. *Harefuah*, 142(2):131–6, 158, 157, February 2003. ISSN 0017-7768.

Herdis Steingrimsdottir. Reproductive rights and the career plans of U.S. college freshmen. *Labour Economics*, 43:29–41, December 2016. ISSN 0927-5371. doi: 10.1016/j.labeco.2016.07.001.

Joanna Venator and Jason Fletcher. Undue Burden Beyond Texas: An Analysis of Abortion Clinic Closures, Births, And Abortions in Wisconsin. Working Paper 26362, National Bureau of Economic Research, October 2019.

Table 1: Eligibility Criteria for Abortions and Subsidies

(a) Abortion Eligibility Criteria and Pre-2014 Subsidies

Eligibility Criteria for Abortion	**Share of Approvals by Criteria**	**Free Pre-2014**
Out of Marriage	50.3%	X
or illegal Act		✓
Risk for Woman	40.6%	✓
or Fetus		✓
Age $<$ 18 or	9%	✓
Age $\geq$ 40		X

Notes: This table shows the eligibility criteria (column 1) to obtain a legal abortion in Israel and the proportion of applications that are approved by the committee for each criteria (column 2). In the third column, we show the eligibility criteria for a subsidized abortion pre-2014. While "out-of-marriage" and illegal act are both under the same eligibility criteria, only abortions approved due to an illegal act were subsidized prior to 2014.

(b) 2014 Change in Abortion Subsidy (Identification Strategy)

Age	**Free?**	
	Pre-2014	**Post-2014**
Age $\leq$ 19	✓	✓
19 $<$ Age $<$ 33	X	✓
Age $\geq$ 33	X	X

Notes: This table highlights the change in eligibility for a fully subsidized abortion following the 2014 policy, which serves as a natural experiment for this paper. Women aged 19 and under were already fully subsidized by the government and therefore unaffected by the change and women age 33 and older were not included in the subsidy expansion and thus never treated. This change in funding applies to women aged 20-32 regardless of what criteria their abortion was approved under, but as can be seen in Table 1a, of the potential criteria (out-of-marriage pregnancy, a pregnancy that is the result of an illegal act, and a pregnancy in which there is a health risk for woman or fetus) that apply to women aged 20-32, the out-of-marriage criterion is the only one not eligible for a subsidy prior to 2014.

Table 2: Sample Construction

Panel A: Primary Analytic Dataset (Conception Panel)		
	Observations	Women
Pregnancy Panel	4,273,610	1,636,580
Conceptions b/w 2009-2016	1,380,674	807,985
Unmarried women	170,605	125,253
Unmarried 18-21 year olds	24,564	20,621
Panel B: Labor Market Dataset		
	Observations	Women
Income Panel	48,591,970	1,636,580
Conceptions b/w 2009-2016	30,935,956	807,985
Unmarried women	2,570,035	125,253
Unmarried 18-21 year olds	402,607	20,621

Notes: This table shows our sample construction, in terms of both total observations (pregnancies) and total women, described in Section 3. Panel A reports these sample sizes for the primary analytic sample – the conception panel, where each row adds data restrictions. Panel B reports these sample statistics for the labor market panel. In both cases, we began with the initial sample of all pregnancies, which we trimmed to conceptions that occurred between January 2009 - March 2016 to women aged 16-40. Then we further restrict our sample to the population of unmarried 18-21 year-olds.

Table 3: Effect of Removing Abortion Cost on Abortion Ratio

	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

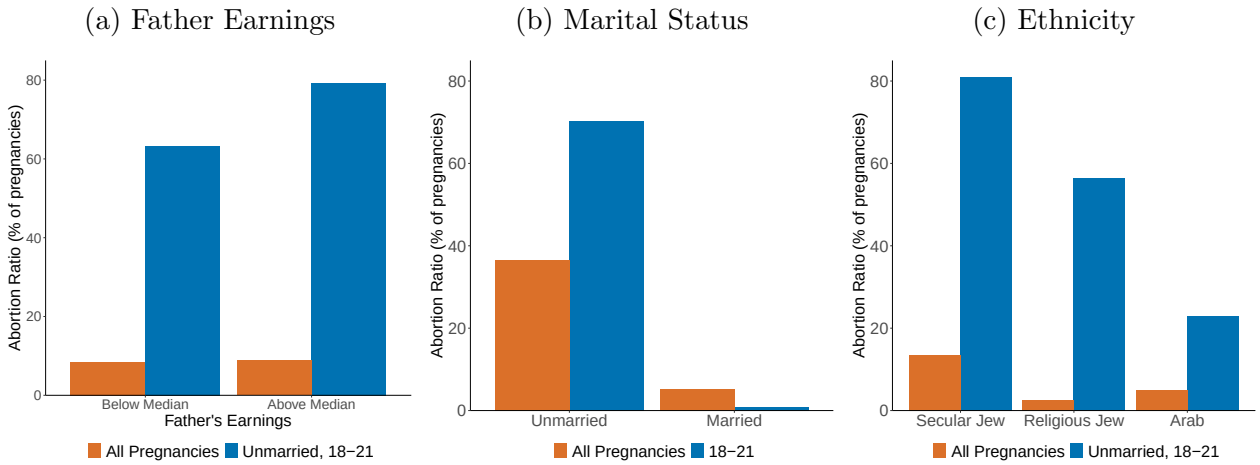
Notes: This table presents the estimates on the abortion ratio due to the 2014 policy making abortions free. Our baseline specification in Column (1) follows Equation 1 as described in Section 4.1 – where we compare outcomes before and after the policy change for women who were affected (20-21) and unaffected (18-19) by the expansion of the subsidy. Column (2) includes a set of pre-pregnancy non-parametric controls (ethnicity, education, yearly earnings, months worked). 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 4: 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)		

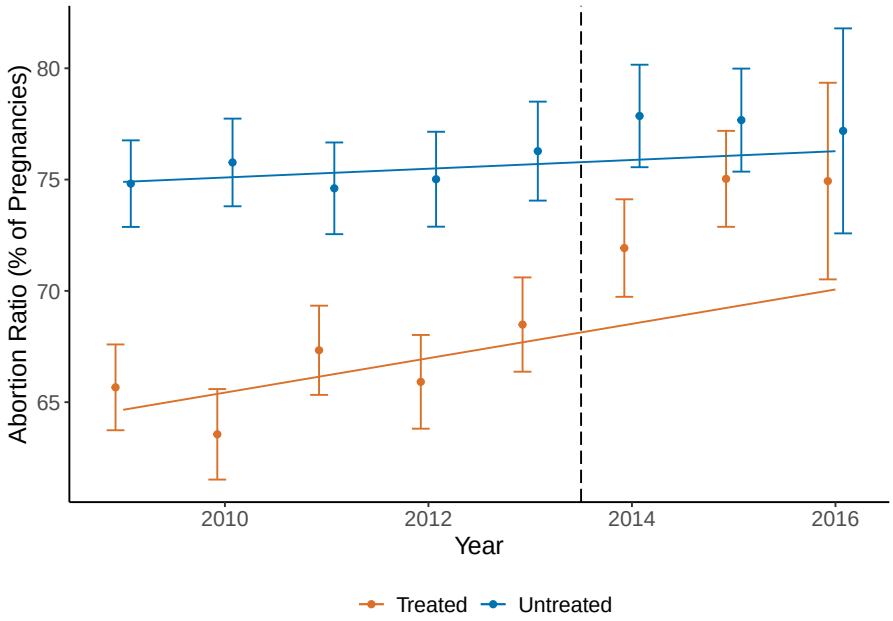
Notes: This table presents results for the effect of the 2014 policy on a range of human capital formation and labor market outcomes. The first column presents the naive OLS (Equation 2), the second column presents results from the IV (Equation 3), and the third column presents results for the reduced from (Equation 5). 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: Abortion Ratio by Sub-Group



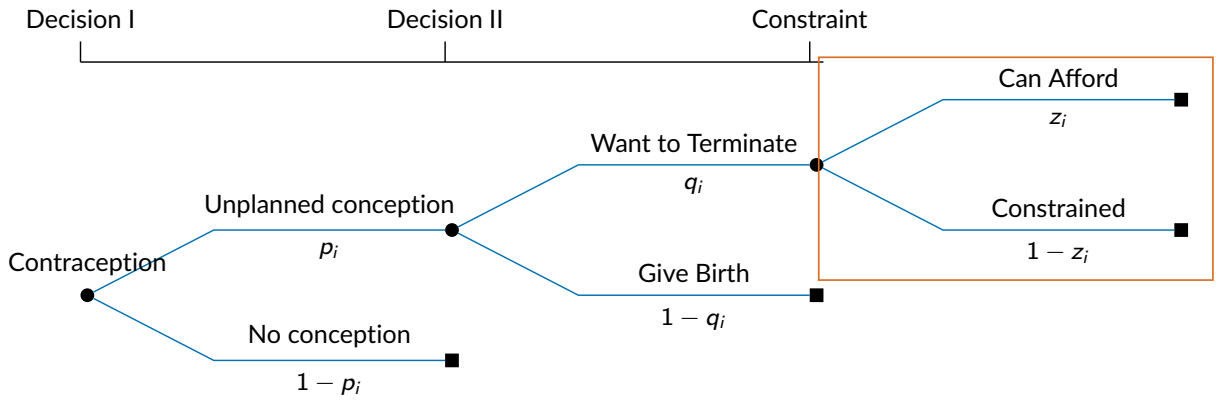
Notes: This figure presents abortions (as % of pregnancies) among important sub-groups within Israel pre 2014 (the year of the policy change). Each panel shows the proportion of abortions out of all pregnancies (orange) and our sub-population of unmarried 18-21 year olds (blue). Panel (a) presents the abortion ratio by father's earnings. Panel (b) presents the abortion ratio 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 the abortion ratio by ethnicity.

Figure 2: Parallel Trends Assessment (18-21)



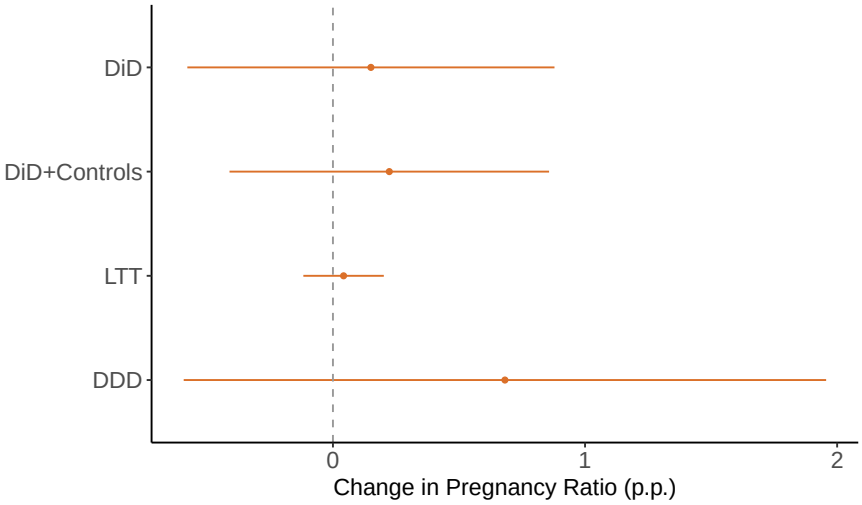
Notes: This figure presents the abortion ratio for treated (20-21) and control (18-19) women over time (2009-2016). The control population is presented in blue, and the treatment population is presented in orange. The dashed line indicates the timing of the 2014 policy change. Each dot represents the 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: Decision Tree



Notes: This figure illustrates our extension of the 'abortion as insurance' model: a two-step decision process as described in Section 5.1 and 5.2. In the model, a woman first makes a decision on contraception intensity p_i ($p_i \in [0, 1]$), which implies a conception will happen with probability $1 - p_i$. Once a conception is realized, the woman decides whether she wants to terminate ($q_i = 1$) or give birth ($q_i = 0$) (explained formally in Appendix D.2). The modified model adds another layer to capture the combination of credit constraints and social stigma cost. This feature of the model captures the fact that credit-constrained young women from traditional households cannot rely on their social network as a safety net for the monetary cost of the abortion.

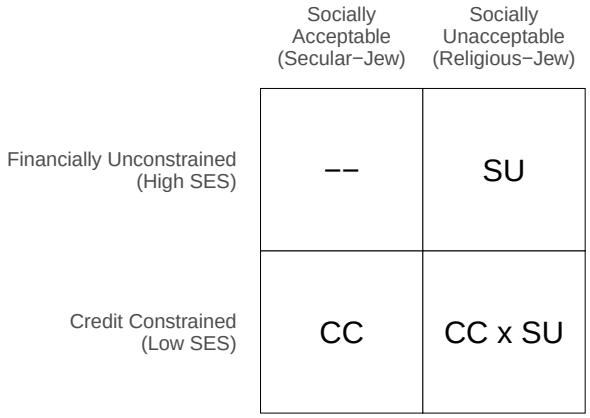
Figure 4: Change in Conceptions (No Evidence for Moral Hazard)



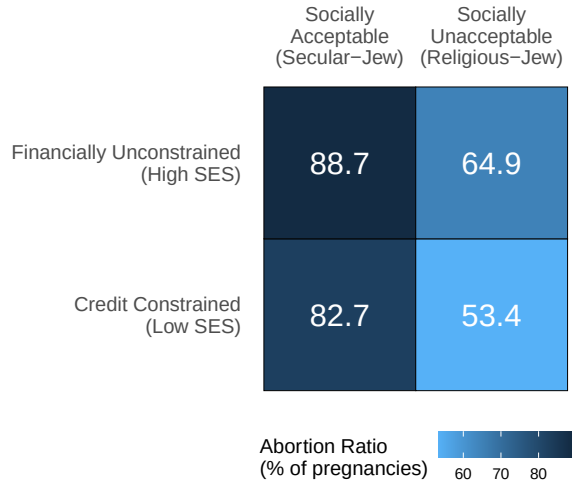
Notes: This figure presents the difference-in-difference results for the effect of the 2014 policy on conceptions probabilities from the population of 18-21 years old unmarried women. Each row presents the results from a different specification, where the dot represents the treatment effect and the lines 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

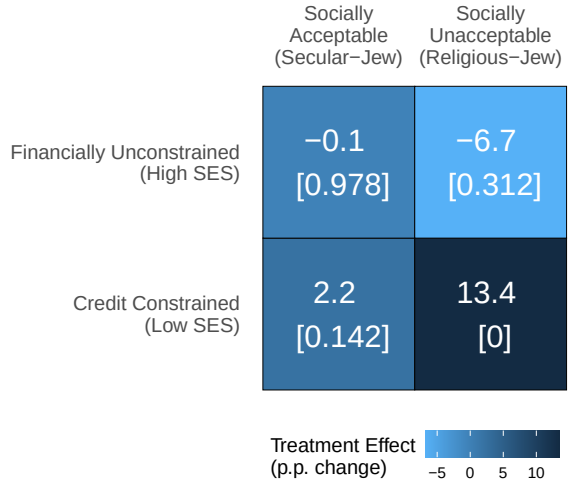
(a) Qualitative Predictions from the Model



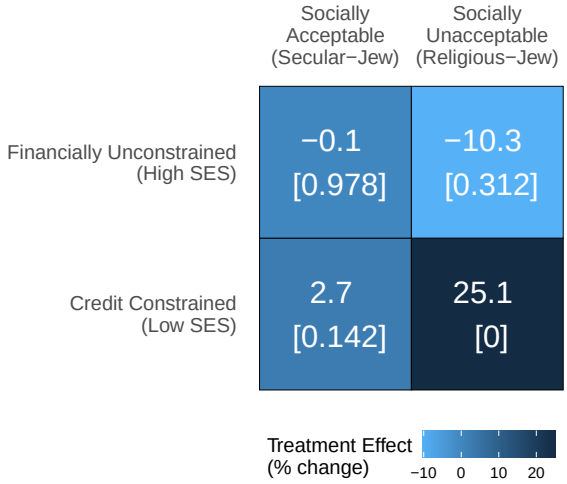
(b) Baseline Ratios



(c) Policy Effect (p.p.)



(d) Policy Effect (% Change)



Notes: This figure presents the heterogeneous effect of the abortion funding policy on abortion ratio while splitting the population across two dimensions: religiosity level and SES background (based on parental earnings). Panel (a) presents the theoretical prediction based on social and financial constraints (see Section 5.2); (b) presents the baseline abortion ratio within each group; Panel (c) presents the effect of the policy on abortion ratio by each group in p.p. with p-values in brackets; Panel (d) presents the effect of the policy on abortion ratio 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.