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

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Abstract

Access to abortion may have profound economic impacts for women, yet little is known about how changes in abortion funding policies affect downstream social and economic outcomes. In this paper, we study the effects of abortion access on fertility and women's career outcomes. To establish causality, we leverage a policy change that in 2014 increased the eligibility age cutoff for free abortion in Israel. We use unique administrative data that allows us to track abortions, births, employment, earnings, and formal education for the universe of Israeli women over a seven-year period. We show that access to free abortion increases the abortion rate but does not increase conceptions. Instead, the result is driven by more abortions among poor women who live in religious communities in which abortion is socially stigmatized. This finding suggests that when abortion is free, poor women do not need to consult family members for financial support, which allows them to have an abortion in private. In the medium-run, access to free abortion delays parenthood, increases human capital investment, and shifts employment towards the white-collar sector, suggesting a large career opportunity cost of unplanned parenthood. Finally, by using observable information on the women we suggest alternative policies that improve targeting of financially constrained women.

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1 Introduction

Abortion is one of the most contentious social policies in the world, often embedded in highly charged moral debates. Yet, access to abortion may also have profound economic impacts for women. A robust body of evidence examines the impacts of abortion legalization across a number of settings,[1] but much less is known about how changes in *funding* policies affect downstream social and economic outcomes.[2] On the one hand, economists are concerned that making abortion free creates moral hazard: women will reduce their contraceptive use due to the lower cost of abortion (Levine and Staiger, 2002; Ananat et al., 2009). Conversely, such a policy may allow low-income and disadvantaged women to access legal abortion and avoid unwanted births. Given the large and persistent earnings drop for women after entering parenthood (Kleven et al., 2019b; Eckhoff Andresen and Havnes, 2019), it is important to understand whether expanding funding for abortion can help reduce this child penalty.

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, for women below 19 years of age, it has also been free. Yet, many young women over age 19 were unable to access abortion services due to a \$600 co-pay.[3,4] In response to advocacy efforts by local activists, the Israeli government expanded the existing subsidy in 2014, making older women eligible for free abortion.

To study the consequences of expanded funding for abortion, we ask three questions: Do abortion rates increase when they are offered for free? If so, *why*? And, what are the career repercussions of avoiding an *unplanned* pregnancy? 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, individual level administrative data on abortion an entire country is extremely rare, thus our data present a unique opportunity to examine abortion access. Using a difference-in-differences strategy, we compare the 'newly funded' women aged 20-21 (treated) to 'always funded' women aged 18-19 (control), before and after the 2014 policy reform. We find that, consistent with the existing literature, increased access to abortion increases the abortion rate.[5]

[1]There are numerous examples from different settings: abortion legalization in the U.S. 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), clinic closures (Myers and Ladd, 2017; Lu and Slusky, 2019; Fischer et al., 2018), changes in Spain's child-benefits González (2013), 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 come from studies using variation in Medicaid funding for abortion in the U.S. Kane and Staiger (1996); Levine et al. (1996); Cook et al. (1999); Bitler and Zavodny (2001).

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

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

[5]See for example: Akerlof et al. (1996); Ananat et al. (2009); Myers (2017); Lindo et al. (2019); Myers and

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 elmination of financial constraints. We find no change in the rate of conceptions, suggesting no evidence for moral hazard. To test the second mechanism, we split our population into women from low-and high-earning families and find that the effect is stronger among women from low-income families, although the difference is not statistically significant.

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.[6] Our data allow us to explicitly test this hypothesis among the Jewish population in Israel. We split our analysis by Jewish religiosity and find the data confirm the anecdotal evidence: the increase in the abortion rate 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 rates increase: making abortion free removed a binding financial constraint for women from social groups where abortion is stigmatized. In other words, making abortion free increases the *privacy* of the decision because it eliminates the (financial) need for women to discuss the decision with family or friends.

Next, we ask whether avoiding an unplanned pregnancy 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 avoids an unplanned pregnancy. We first show that, among policy compliers, having an abortion results in a decrease in parenthood and marriage rates in the three years following conception. Furthermore, we find an increase in educational attainment on a range of margins: college enrollment, teacher training, post-high-school professional training, and taking the Israeli matriculation exam. Finally, we find that avoiding an unplanned pregnancy results in a lower probability of being in the labor market, but that, conditional on working, women who avoid an unplanned pregnancy are more likely to work part-time and in better-paying sectors (e.g., public sector instead of restaurants).

Taken together, these results suggest that when abortion is not free, young pregnant women enter into early unplanned parenthood and possibly unplanned marriages, which can be avoided when the constraints are removed. Subsequently, by avoiding early unplanned 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

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).

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

capital. 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.

In the final section of this paper, we use these insights to inform a discussion of optimal policy in this context. When implementing the abortion funding policy, the government faces an unobserved type problem – it is costly to tell which women are financially constrained and which are unconstrained; therefore, the government enacted a policy that tags likely financially constrained women on the basis of age. We consider an alternative policy that improves tagging precision by incorporating our empirical finding that the largest impacts are among socially and financially constrained women. Then we provide a framework to assess the trade-offs of different funding schemes in terms of excess cost and missed abortions.

Our paper contributes primarily to three bodies of literature. First, we revisit the moral hazard mechanism suggested in the economics of abortion literature with the "abortion as insurance" model (Kane and Staiger, 1996; Levine and Staiger, 2002, 2004; Levine, 2007; Ananat et al., 2009). Our detailed administrative data allows us to understand the mechanisms that drive the effect of making abortion free on the abortion decision. Given we find no evidence that providing abortion for free induces 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. However, an important difference between the Israeli policy we study and the policy in Ananat et al. (2009) is that abortion legalization in the U.S. did not affect the financial cost of an abortion. In other words, financially constrained women in conservative states still had to seek financial support to have an abortion; consequently, the abortion decision was not private, which may have resulted in lower utilization of abortion services in more conservative U.S. states after legalization.

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 a different context, Ashraf et al. (2014) find that women are less likely to seek family planning services if their husbands are involved. 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 religious Jewish backgrounds. Taken together, this evidence suggests that 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 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 debate by studying a setting in which only abortion access changes; while contraceptive access and other fertility policies are held constant.

More closely related to our study is the literature on the economic consequences of being denied an abortion. Miller et al. (2019) and Foster et al. (2018), who use the Turnaway study[7] 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 these studies, yet we examine the converse margin – the positive economic impacts from *expanding* abortion access. Specifically, we focus on a different economic margin (early career and human capital investment) and study a different policy (providing free abortions).

Third, our findings contribute 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* effect of an *unplanned* child. 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). 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 unplanned pregnancies than for pregnancies overall. Our findings suggest that, at least for young women, an unplanned pregnancy induces an additional penalty to their human capital investment.

Our work is also situated in research on the determinants of college investments for women and the importance of flexible or part-time employment in the development of women's careers (Goldin et al., 2006; Goldin, 2014), as well as the labor market effects of parental leave and other family support policies more generally (Lalive et al., 2014; Dahl et al., 2016; Gallen, 2019; Kleven et al., 2019a). By highlighting the role of abortion access in facilitating human capital investment for young women, coupled with studies showing that early shocks have particularly large negative effects on lifetime career outcomes (Gallen et al., 2021; Kahn, 2010; Oreopoulos et al., 2012), suggest that an important policy for preventing an early, negative shock to young women's careers could be covering abortion costs.

[7]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).

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 rates that occurs in response to the policy. In Section 5, we explore alternative explanations for the policy's effect on abortion rates and show that, while moral hazard does not explain the result, the increase in abortion rate occurs primarily in the sub-population of socially and financially constrained women. Section 6 presents our identification strategy that combines the difference-in-difference with an event study design and demonstrates the effect that abortion has on women's educational and labor market outcomes. Section 7 presents a framework to asses the trade-offs between the existing funding scheme and other alternatives. Section 8 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.

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 (see Table A1 for approval shares by each criterion). 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 that will not be officially 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.[8]

All legal abortions in Israel must go through this committee process, including when women opt to have the procedure performed by a private doctor outside of the public healthcare system. Although the committee process may seem obstructive, the committee itself effectively

[8]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.

serves as a rubber stamp and in practice many women that 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 (see Appendix B.1 for further discussion of the approval process).

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.[9] As we explain below (Section 3), our data on abortion come from the official abortion committee; thus, we do not capture illegal abortions. The existence of a large illegal market would complicate the interpretation of our results because any change in the abortion rate could be due to shifts from the illegal to the legal abortion market. We address this concern directly in Section 5.3. However, anecdotal evidence suggests that in Israel, incentives to obtain an abortion outside the legal system are low, especially among low-income women.[10]

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 a year.

Figure Id shows the variation in abortion rates across income levels in Israel; 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) (see Figure A1 for further breakdown by earning levels). Overall, these factors imply that the co-pay might be a binding financial constraint for young and lower earning Israeli women.

[9]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 harsher then in Israel. Therefore, we are not concerned the possibility of 'abortion tourism' is convoluting the interpretation of the abortions we observe.

[10]For more information about the illegal abortion market see Appendix B.2.

2.3 Social Stigma

Because of its cultural and religious heterogeneity, Israel is an interesting setting to study abortion.[11] Figure Ia demonstrates the substantial variation in baseline abortion rates, which might suggest different latent costs of abortion (or differing abortion views) across groups. The Jewish population consists of a wide mixture of religiosity levels, ranging from secular Jews (45%), traditional Jews (25%), religious Jews (16%), and Orthodox Jews (14%) (Central Beurue of Statistics (Israel), 2018).[12] 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.[13]

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.[14] 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.[15] 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 help support women who need financial help, but leaders noted that it was insufficient for the scale of the problem. Women's rights advocates lead by Dr. Eyal lobbied the Israeli Health

[11]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.3 for more information about abortion norms in Israel.

[12]Furthermore, the former-USSR immigrants are a unique population for studying the impacts of the 2014 policy due to their relatively lenient abortion views. For example - during the 1970s, four out of five pregnancies were aborted, giving the USSR the highest abortion rate in the world (Avdeev et al., 1995).

[13]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 Beurue of Statistics (Israel), 2018).

[14]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 A1, 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.

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

Ministry and advocated for an expansion of the existing subidies.[16,17]

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 I). 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 family or income policies 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.

3 Data

We obtained access to 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 first component provides data on every woman who applied to the abortion committee between 2009-2016, which includes information about the woman's pregnancy (such as the week of pregnancy at the time of application). The second component is the 2016 civil registry data, which we use 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). Combined, the abortion committee data and the civil registry data allow us to identify all of the recorded pregnancies in Israel between January 2009 and March 2016[18] with the exception of pregnancies terminated without the permission of the committee (illegal abortions) and miscarriages that occur early in pregnancy.[19]

Third, we use tax data composed of a monthly panel of labor market employment, earnings, and sector identifiers, from 2005-2018. Fourth, we use data from the education registry spanning 2005-2018 that includes data on whether women obtained a high-school diploma,

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

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

[18]The latest births we observe are in December 2016; thus, we observe conceptions only until March 2016.

[19]Third trimester miscarriages are captured in the abortion data; however, we drop these from our analysis.

took the Israeli-SAT, enrolled in higher education, or participated in some form of training (e.g., vocational training, practical engineering).

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 (Appendix C.1 explains the data structure in detail). We complement our primary datasets with a survey that assesses 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: Restrict to Unmarried, 18-21 Year Olds

Table II 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 (see Appendix C.1 for more details on the data construction). 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.[20]

We then further restrict our sample to unmarried women for three reasons: (1) all unmarried women are automatically approved for abortion; (2) the structure of the 2014 policy; and (3) pregnancies among unmarried women are more likely to be unplanned (the focus of our analysis). With respect to the first reason, as shown in Table A1, 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 (since the abortions are classified according to the approval criteria), we do not observe out-of-marriage pregnancies among the women who gave birth (e.g., any pregnancies carried to term that were the result of infidelity).[21] Thus, the most direct way to ensure the comparability of the populations of women who had abortions and those who gave birth is to restrict the sample to all unmarried women.

With regards to the second reason, given the prior criteria for government funding, the 2014 expansion changed the funding coverage primarily for unmarried women (Tables I and A1). As shown in Table A1, there is substantial overlap between the *approval* criteria and

[20]The military covered abortions even before 2009 for women who served, which means that some 19-year-olds that served in the military were covered. Therefore, starting the sample period before 2009 could contaminate our treatment because we cannot determine in our data who is in the military. We test for this policy change and find that, in practice, the policy had a negligible and insignificant effect, which we interpret as due to the already existing coverage by the military.

[21]We describe in Appendix B.1 how married woman 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.

the *funding* criteria. 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. 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.

Third, a more intuitive reason to focus on unmarried women is that unmarried women are more likely to have an unplanned pregnancy (Buckles et al., 2019), and, as discussed below, this is particularly so younger women. Figure Ib illustrates that intuition: the abortion rate among young unmarried women is 71.5%, while among young married women is 0.75%.

Finally, we restrict our analysis to the population of unmarried women who are 18 to 21 years old. Due to both conceptual and empirical reasons, we take a bandwidth of two years above and below the younger age cutoff (19 years old) for the subsidy. Conceptually, we are focused on identifying the effects of *unplanned* pregnancies on career outcomes (Section 6), which are important if they result in a significant shock to a woman's economic trajectory. If an unplanned pregnancy reduces a woman's labor market prospects, exploring policies that prevent this initial shock is important. While we ultimately cannot observe which pregnancies are planned, Buckles et al. (2019) show that young, unmarried women are more likely to have unplanned conceptions than their older counterparts. Also, 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 and technical training. In Israel, about 57% of Jewish women serve in the military between their 18th and 20th birthdays. This service delays entrance to higher education: only 5.6% of women aged 18-21 participate in higher education; in contrast, 18.2% of 22-24-year-old women do.[22]

There are also two empirical reasons for focusing on a small bandwidth around the age cutoff: 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 D.1 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

[22]Given that our control group is 18-19 years old, military service might pose a threat to our identification if military service affects women's fertility decisions. We note two facts that relieve these 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 don't serve) are the main populations that are drive our results (as we will show in Section 5.2).

(Table II).

4 Effect of Abortion Subsidy on Abortion Rate

4.1 Empirical Strategy

To identify the effect of the 2014 policy on abortion rates, we use a DiD strategy that leverages the timing of the policy change (2014) and the age cutoff (19 years old, highlighted in Table I). 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, organized 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 ($\mathbb{1}\{t \geq \text{Dec-2013}\}$)[23] and 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 rates and fertility. Lastly, X_i represents a set of pre-pregnancy controls (ethnicity, education, yearly earnings, months worked, and total number of children). Standard errors are clustered at the age-at-conception level.

Our difference-in-differences approach assumes parallel trends: women eligible for the subsidy would have experienced similar changes in their abortion rate over time as ineligible women in the absence of the 2014 subsidy. To assess the validity of this assumption, we interact the treatment status (T_i) with a dummy for each year in our sample (for years $k \in \{2009, 2016\}$):

$$abort_{it} = \sum_{k=2009}^{2016} \delta_k \times \mathbb{1}\{t = k\} \cdot T_i + \gamma_{a_i} + \gamma_{y_t} + \epsilon_{it} \tag{2}$$

Figure II plots the estimates of δ_k from Equation 2. The estimates represent the difference in abortion rate between treated (aged 20-21) and untreated (aged 18-19) women over time (2009-2016), with the 2013 difference dropped as the reference year.The shaded regions mark the 90% and 95% confidence intervals around the point estimate, respectively. We see no

[23] See Appendix C.2 for an explanation of why we push it back to December 2013.

statistical difference in the abortion rate between treated and control women before the policy change (which supports the parallel trends assumption); after 2014, the abortion rate among treated women increased.[24]

4.2 Results: Increase in the Abortion Rate

We find that removing the abortion cost increased the abortion rate by 4.6 percentage points relative to younger women who were already subsidized, as reported in the top left of Figure II. This is equivalent to an increase of approximately 7%, given the baseline abortion rate 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. As discussed further in Section B.3, this is consistent with Figure B3, which suggests there was a lag in awareness of the policy. Therefore, we believe the 2014 effect is muted, and so the medium-run effect is closer to the levels in 2015 and 2016.[25]

Our baseline specification follows Equation 1, as described in Section 4.1. Estimating it without controls ("DiD") we find a 4.6 percentage points increase in the abortion rate. In Table III we perform several robustness tests. First, we test the robustness of the DiD to the inclusion of increasing numbers of controls ("DiD + controls"), which reduces the estimated effect to 3.2 percentage points.[26] 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.[27] To address this concern, we also estimate a triple difference approach, using married women aged 18-21 as a third difference. The magnitude and significance of the effect in the triple difference (column 3) are comparable to the main

[24]Appendix D.1 discusses the parallel trends assumption 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 D1). 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 D2). 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.

[25]Section B.3 presents an analysis of Google search trends of the Hebrew word for abortion, "hapala"). A closer look at Figure II shows an increase in our standard errors in 2016. This is a consequence of our data structure, which allows to observe conceptions only until March 2016 (since the latest births we see occurred in December 2016).

[26]Our results in the next sections of our analysis are qualitatively agnostic on whether to include those controls. Nevertheless, given the small cuts of data we are about to perform, we prefer for the sake of clarity to keep the 'no controls' specification as the baseline in our estimation procedure.

[27]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, only 60% of women serve in the IDF, and essentially no religious or Arab women serve, which are the population that are driving our results (as shown in Section 5).

effect (column 2) in Table III, serving as stronger evidence for the exogeneity of the policy (Appendix D.2 presents an additional robustness using linear time trends).[28]

5 Potential Channels

In the last section, we demonstrated that providing abortion free of charge increases the abortion rate. It may be counterintuitive that eliminating such a small cost (the co-pay for the abortion) relative to the cost of raising an unplanned child would result in such a large effect.[29] In this section we explore the potential underlying mechanisms.

5.1 Effect on Conceptions: Test for Moral Hazard

It has been widely shown that reducing the cost of abortion increases the abortion rate (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 IV).[30]

Based on this model, a reduction in the abortion cost (monetary, physical, or psychological) will translate, by backward induction, to a lower contraception decision, 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 III. We find a small and insignificant across a range of specifications, suggesting no evidence for the existence of moral-hazard in our settings.[31]

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

[28]For the sake of completeness, Figure A2 presents these estimates while varying both the specification (DiD, DiD + controls, DDD, LTT) and the population group (18-21, 30-35, 16-40).

[29]Note that child-rearing in Israel is orders of magnitude cheaper than in the U.S. because of Israel's pro-natal policies. For example, both education and healthcare are public and universal in Israel (see Appendix B.4 for more details).

[30]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 E.1.

[31]Conducting a similar exercise in levels shows a *decrease* in births, reinforcing the lack of evidence for moral hazard (see Figure A5).

Eastern Europe in the late 1980s and early 1990s and conclude that moderate changes (e.g., shift from abortions available to those with medical problems to abortion on demand) result in moral hazard (an increase in abortions with no change in births), while large changes, such as legalization, show a decrease in births proportional to the increase in abortions. In contrast, Ananat et al. (2009) use the state and time variation in abortion legalization in the U.S. and find a bigger increase in abortion rate relative to the decrease in births, suggesting moral hazard is present in response to large policy changes, such as legalization. Our finding suggests no evidence for moral hazard in a case where abortion is already legal and a more moderate change, providing abortion free of charge, was implemented.

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 study (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. We find that the effect is stronger among women from low-earning families, although the difference is 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. We hypothesize that the *marginal* abortion decision is impacted by a combination of social views and financial constraints (henceforth "social and financial constraints"), which are relaxed when abortion is free.[32] This theory is supported by the descriptive evidence presented in Section 2, which shows that baseline abortion rates are highest among women from higher-earning households (Figure Id) and lowest among women from more religious backgrounds (Figure Ia).[33] Thus, for lower income women coming from religious backgrounds, asking friends or family to help financially to obtain an abortion if they cannot afford it on their own may not be an option.[34]

We modify the 'abortion as insurance' model, presented in Figure IV, to include these social

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

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

[34] 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 (see further breakdown by household earnings in Figure A1), suggesting that financial constraints, or at least perceived constraints, are binding at these cost levels for young Israeli women.

and financial constraints (formally defined in Appendix E). The updated model implies two testable hypotheses: first, baseline abortion rates should be the lowest for 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 this hypothesis, consider the two-by-two table in Figure Va, in which we split our population across two dimensions: social views about abortions and financial constraints. A woman who is financially unconstrained and come 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 combined parental earnings and splitting them below and above median parental yearly earnings ("Low SES", "High SES" in Figure Vb). 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 socially accepting and the religious Jews as non socially accepting.[35] As shown in Figure Vb, the baseline abortion rate follows the logic in Figure Va: the highest abortion rate occurs among financially unconstrained women from secular backgrounds, while the lowest occurs among the socially and financially constrained women who face both $CC \times SS$ 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 Vb. 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.[36] Figure Vd presents the estimates of the effect in percentage change relative to the baseline rate (see Figure Vc for the effect in percentage points). We can see that the effect on the abortion rate follows the logic presented in Figure Va: the highest (and only

[35]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.

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

significant) effect is concentrated among poor, religious women.[37,38]

These results suggest that the combined relaxation of social and financial constraints is the primary driver of the increase in the abortion rate ($CC \times SS$ constraints). 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. This finding differs from that of Ananat et al. (2009), who find that legalizing abortion in the U.S. had a larger effect on abortion rates in liberal states than in conservative states. They conclude 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 to abortion.

One explanation for the differences between the Ananat et al. (2009) results and ours lies in the framing of the latent cost of abortion and the type of policy change. If we consider the latent cost to be composed of two components: a personal moral objection to abortion and a social cost, both were present in the U.S. setting when abortion was legalized. However, legalization did not affect the cost of abortion, which may still have been out of reach for many low-income women.[39] In the US case, in more conservative states women both had a high personal moral objection to abortion and faced a social cost for seeking support to pay for one. However, the Israeli policy enables women who cannot afford a wanted abortion to avoid asking for financial help, which increases the *privacy* of the decision and helps them avoid the social cost.[40]

[37]One concern is that the poor-religious population is poorer than the poor-secular population and, thus, faces more financial constraints. Figure A4 suggests otherwise: both the woman's and her parents' earnings are similar across both groups.

[38]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 will require that women are discharged more than a year after they turn 20, which is not possible in the IDF.

[39]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.

[40]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.

5.3 Alternative Explanations

Finally, we explore two additional explanations of what could be driving the increase in the abortion rate: (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.[41]

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 rate to increase among women who were unaffected by the policy change.[42] Figure I2a, which presents a first-differences exercise by age, shows no significant change in the abortion rate among age groups ineligible for the subsidies. As an additional test, we consider the policy's effects on Israeli-Arabs, whose personal and moral views are perhaps less influenced by the decisions of the Jewish government. Nevertheless, Figure A3b shows an effect with similar magnitude (yet insignificant) for the Israeli-Arab as for the Religious-Jewish population.

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 that we observe might be not an absolute increase but a substitution away from the illegal to the legal market in response to the subsidy. Second, responding to the subsidy, the illegal market could reduce 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.[43] If the entire effect of the policy is due to a shift from illegal to legal abortions, we should observe no change in the birth rate. 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 the birth rate that is greater than the increase in the legal abortions we observe.

To test this hypothesis we collapse our dataset to the year-month-age level and run the

[41] Another possible explanation is a standard price-theory effect (i.e., a reduction of the price of apples good will result in an increase in the purchase of apples regardless of one's budget constraint). 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 Vd, there is no effect among constrained or unconstrained women who live in a socially accepting society.

[42] 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. If this is the case, we can assume a weaker assumption: whatever shift in moral views that happened it was constant between the two groups (18-19 and 20-21). Consequentially, our identification strategy will still hold. Given the small and insignificant shift in the untreated groups shown in Figure I2a, we are not very concerned about this possibility.

[43] 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.

same DiD specification in Section 4.[44,45] The results in Figure A5 show an increase of five abortions per age-month and a corresponding proportional decrease of eight births per age-month. Thus, the increase in the abortion rate 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.

6 Labor Market Consequences

After establishing that making abortion free increased the abortion rate, particularly among socially and financially constrained women, we shift to examining the effect this change had on women's medium-term fertility, human capital investment, and career decisions.

A possible (naive) strategy is to compare post-conception outcomes of women who aborted to women who gave birth with a standard OLS. 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 A2). 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 unplanned parenthood among women who were *constrained* from having an abortion. The 2014 abortion subsidy expansion allows us to address both of these issues, focusing explicitly on the sub-population of constrained women (our compliers) who from poor and religious backgrounds.[46]

6.1 Empirical Strategy

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. For example, a dynamic approach allows us to see how long women delayed parenthood after having an abortion. Therefore, we combine the variation induced by the 2014 policy with an event study relative to the timing of (potential) birth to develop

[44] 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. This is so because the population is comprised of conceptions (abortions + births).

[45] For example, a row in this collapsed dataset will be: 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 doing this collapsed exercise doesn't allow us to control for individual-level observable characteristics.

[46] As shown in Appendix H the results are qualitatively similar to the entire young, unmarried population.

a dynamic picture of the career consequences of avoiding an unplanned parenthood.[47] We complement this empirical approach with an instrumental variable approach that helps make sense of the magnitudes in Appendix F.

In order to better understand the interaction we implement between the difference-in-difference and the event study, we begin by briefly presenting the standard event study design relative to the timing of potential birth known as the 'child-penalty' (Kleven et al., 2019b).[48] The analysis is estimated on a yearly-level panel for all women who conceived within our sample period. For each woman, we denote the year in which she has her first child by c_i and index as $j = t - c_i$ for $j \in \{-3, 3\}$ years relative to the conception. This standard event study is estimated with the following regression:

$$y_{it} = \sum_{j \neq -1} \alpha_j \cdot \mathbb{1}\{j = t - c_i\} + \gamma_{a_{it}} + \gamma_t + \epsilon_{it} \tag{3}$$

where y_{it} denotes an outcome for woman i in year t and at event $j = t - c_i$ and $\mathbb{1}\{j = t - c_i\}$ represent the full set of event time dummies, $\gamma_{a_{it}}$ are age dummies, and γ_t are year dummies.[49] Event time dummy at $j = -1$ is omitted as the reference category, implying that the event time coefficients measure the impact of children relative to the year just before the first (potential) childbirth. By including a full set of age dummies, Equation 3 controls nonparametrically for underlying age-specific effects, and by including a full set of year dummies, we control nonparametrically for time trends, such as wage inflation and business cycles. It is possible to identify the effects of all three sets of dummies because, conditional on age and year, there is variation in event time which is driven by variation in the age at which individuals have their first child.

Next, we incorporate the policy variation (Equation 1) into this event study (Equation 3) Figure VI illustrates the intuition behind the identification of this combined event study and difference-in-difference approach. Panels VIa and VIb show the event study of a given outcome y (e.g., employment) relative to the timing of a potential birth for a population of women who conceived (pooling abortion and births), split by conceptions that happened before the policy change (blue) and conceptions that happened after the policy change (orange). Panel VIa illustrates the event study for our treated group (age ≥ 20), while Panel VIb illustrates the

[47] Potential in the sense of the potential time a birth would have taken place if the aborted pregnancy had been carried to term. We choose this event-time as opposed to conception time to be consistent with the 'child penalty' literature that shows the effect on earnings comes in the year of birth.

[48] See baseline estimates of the child-penalty for young unmarried women in Appendix G.2, and comparison of women who abort to women who gave birth in Appendix G.3.

[49] Unlike the age and time dummies estimated in Sections 4.1 and F, which were the age at conception and year of conceptions, these are the age and year in which we observe women in the income and education panel.

corresponding event study for our control group (age < 20). The differences between the lines in VIa and VIb are the first differences (post − pre) illustrated in Panel VIc. The difference between the first differences by treatment status is the event study difference-in-differences estimator illustrated in Panel VId.

Figure VI also illustrates the value of this combined approach. In the absence of any contemporaneous changes, the difference between the graphs in Panel VIa should be sufficient (i.e., the orange line on Panel VIc). However, as Panel VIb illustrates, the outcome could also be changing in the control group over time, perhaps due to a constant shift in attitudes (i.e., time trends). For example, if the outcome is employment and women are increasingly more expected to work post-birth, we will see the shift observed in Panel VIb regardless of the policy change. Therefore, to net out such effects, we take a third difference.

Given that the event study itself serves as a first difference from the dropped year (-1), this is essentially a triple difference estimator. In other words, these coefficients are conceptually equivalent to running separate triple differences for each year since conception relative to year -1. Though, they are not numerically equivalent since we put everything in one regression to get additional power to identify the year and age fixed effects.[50] Formally, we estimate:

$$\begin{aligned} y_{it} = {} & \\ & \sum_{j\neq -1} \alpha_j^{Post\times T} \cdot \mathbb{1}\{j = t - c_i\} \cdot Post_{c_i} \cdot T_i \\ & + \sum_{j\neq -1} \alpha_j^{Post} \cdot \mathbb{1}\{j = t - c_i\} \cdot Post_{c_i} \\ & + \sum_{j\neq -1} \alpha_j^{T} \cdot \mathbb{1}\{j = t - c_i\} \cdot T_i \\ & + \sum_{j\neq -1} \alpha_j^{KLS} \cdot \mathbb{1}\{j = t - c_i\} + \gamma_{a_{it}} + \gamma_t \\ & + Post_{c_i} \cdot T_i + Post_{c_i} + T_i + \epsilon_{it} \end{aligned} \tag{4}$$

Similarly to Equation 1, $Post$ is an indicator that the policy is in effect ($\mathbb{1}\{c_i \geq$ 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 3.[51]

Here our parameters of interest are the $\alpha_j^{Post\times T}$ from the triple interaction of each event period with the double difference: $\mathbb{1}\{j = t - c_i\} \cdot Post_{c_i} \cdot T_i$. We interpret these estimates as the

[50] The implicit assumption is that age and year effects are fixed across the control and treatment group, before and after the policy change. Furthermore, note that age and year fixed effects are defined in a given calendaric year, while $Post_{c_i}$ and T_i are defined relative to *conception* time, making them both separately identified.

[51] Given the yearly structure of the data we cannot include month fixed-effects, however, for robustness we include month-of-conception fixed effects which does not affect our results.

additional effect from avoiding an unplanned birth due to the policy among the women who conceived.[52] This triple difference estimator represents the reduced form, or the intention-to-treat (ITT) effect of the policy. Given the policy change happened in 2014 and our tax and education data span until 2018, we take a time span of three years prior to three years post potential birth ($j \in \{-3, 3\}$), relative to one year before potential birth (the dropped category).

6.2 Effect on Human Capital Investment and Labor Force Participation

6.2.1 Parenthood and Marriage

Why would access to free abortion affect women's career outcomes? It is well documented that parenthood imposes a 'penalty' on women's careers (e.g., Angelov et al. (2016); Kleven et al. (2019b,a); Eckhoff Andresen and Havnes (2019)). Therefore, we first establish that the increase in abortion due to the policy allowed women to avoid an unplanned birth, and thus delay parenthood in the medium-term. For this purpose we define a binary parenthood outcome ($\mathbb{1}\{\text{Is a parent}\}$) and estimate Equation 4 on the sample restricted to socially and financially constrained women (as defined in Section 5.2).

Figure VIIa shows a persistent decrease in the probability of entering into parenthood in the three years following potential birth. More specifically, among socially and financially constrained women, the 2014 policy reduced the probability of being a parent by 11-14 percentage points in the three years following the index pregnancy (relative to a baseline parenthood rate of 43% in the year of potential birth). In Appendix F we take stock of these effects by running the IV equivalent of this strategy. We find that 88% of the compliers did not enter parenthood in the subsequent four years following the conception they aborted.

Avoiding an unplanned birth might also reduce unplanned marriage, because women who have an unplanned pregnancy may marry the father. The data seems to support that claim: Figure VIIb presents a persistent decrease in the probability of getting married in the three years following potential birth. More specifically, in the population of socially and financially constrained unmarried 18-21 year-olds, the 2014 policy reduced the probability of getting married by 11 percentage points in the years following the index pregnancy (relative to a baseline of 22% in the year of potential birth). These results suggest that the removal of financial constraints allowed the constrained women to avoid unplanned parenthood *and* a subsequent unplanned marriage.[53]

[52]'Added' in the sense of an additional penalty for being *unplanned* on top of the child penalty of a birth in general.

[53]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.

6.2.2 Human Capital Investment

Given the young age of the women in our sample, they are at a critical time for human capital investment. We examine different educational outcomes that occur after an avoided unplanned birth. Our data includes a rich set of time-varying educational outcomes, including whether a woman received or retook her high-school matriculation exam ('Bagrut'), took the SAT, is enrolled in an academic institution, or took part in any non-academic training (e.g., vocational training, teacher diploma).

We first estimate Equation 4 on academic enrollment. We find that an increase in the probability of academic enrollment (Figure VIIc). Specifically, the share of women who conceived and enrolled in an academic institution increased by 4 - 11.7 percentage points due to the elimination of the abortion cost (relative to a baseline of 4.4% in the year of potential birth). Note that the (insignificant) effect prior to conception can be attributed to the structure of the education system.[54] Furthermore, note the effect is not a continuation of these trends but a sharp break in trends during the year of potential birth, followed by an increase in enrollment that grows larger over time.

Next, we estimate Equation 4 for a broad range of human capital outcomes: whether the woman took the high school matriculation exam, is enrolled in teacher training, or is enrolled in post-high school non-academic training, in a given year. We find an increase on each one of these outcomes as a result of the policy (see Figure A6). This result, coupled with the delay in parenthood and marriage life, is consistent with the logic in Goldin et al. (2006), which argues that the reversal of the gender gap in college graduation appear to be driven by increases in girls' expected economic returns to college, which in turn arose from improvements in perceived labor market opportunities and an increase in the age of first marriage.

Finally, in Appendix H we present comparable results for the entire population of young unmarried women who conceived; these results are qualitatively similar to the results for the socially and financially constrained population. Removing the abortion cost decreased parenthood and increased investment in human capital.[55]

[54]Our treated group (20-21 year-olds)are mechanically more likely to be enrolled three years prior to conception because they are at age (18) when some young people in Israel start higher education. Our control group (18-19 year-olds), who are only 15-16 years old three years prior to conception, are still in high-school. Thus, if academic enrollment for 18 year olds increases over time (because fewer are serving in the military) we should see this effect. These differences are partially residualized because of the inclusion of the age fixed effects. However, in the data our treatment group is never 15 or 16 years old. Consequently, these fixed effects are identified only from the control group. Furthermore, since fixed effects are essentially just demeaning the outcome, there is remaining variation in higher level moments (e.g., variance).

[55]However, this larger population responded on a different educational margin: they were more likely to finalize (or perhaps retake) their high-school matriculation exam, whereas we see no real change in enrollment in academic studies.

6.2.3 Labor Market Outcomes

Does this investment in human capital translate into an increase in yearly earnings? Following the same empirical strategy we find a *temporary* increase in yearly earnings unconditional on working (see Figure VIId). 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).

This result emphasizes the benefit of the event study estimation relative to the standard reduced form: running the standard (non-dynamic) reduced form shows a null (and negative) effect (Figure F2c), while the event study emphasizes the dynamics: a temporary increase in the year of potential birth and an insignificant decrease in the later years, averaging to zero over-time. Furthermore, the dynamic approach helps us understand similar patterns in the data that shed light on the underlying mechanism. For example, Appendix Figure A7d 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 unplanned birth, and, as a result, are not taking time off. The subsequent decay of the effect on months worked is consistent with the increase in academic enrollment presented in Figure VIIc: since these women are more likely to go to college, they substitute away from time spent working.

Next, we test for the effect of abortion access on employment (extensive margin). We find a null effect on employment and a shift from full-time to part-time employment (see Figure F1)) due to the increase in abortion access.[56] Specifically, the share of socially and financially constrained 18-21-year-old unmarried women who conceived and worked part-time (earned below minimum monthly earnings, conditional on working) increased by 6.5 percentage points due to the elimination of the abortion cost (mean in years following the index pregnancy is 75% - 80%). 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 the baby. Now, when abortion is provided for free, they are more likely to invest in their human capital, but because they have little free time while they complete their studies, they shift to part-time and self-employment because of the flexibility it affords.[57,58]

Finally, we ask whether this investment in human capital translates into employment in

[56]The dynamic estimation allows us to see a delayed effect on self-employment (Figure A7a) three years after the timing of potential birth. This is consistent with the need of more flexible employment while studying.

[57]Another intuitive explanation for the null effect on employment is that women who gave birth are on maternity leave and working fewer hours, but still technically employed.

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

better-paying jobs. To answer this question, we estimate Equation 4 on the sector-level wage-premium. Following Abowd et al. (1999), we estimate the sector-level wage-premiums by running a log-wage regression on individual and sector fixed effects (see further details in Appendix I.3). The results in Figure F2e suggest an increase of 0.08 log-points (or five percentage points) in the wage-premium of the sector in which these women work.[59] Although our three years of data following the 2014 policy somewhat limit our ability to examine the policy's full labor-market consequences, the temporal dynamics imply an investment in human capital and a shift towards better paying jobs over the short-to-medium term.

7 Efficacy of Alternative (Tagging) Policy Design

7.1 Government's Unobserved Type Problem

In this final section, we use insights from our empirical findings to inform a discussion of optimal policy. The goal of expanding the abortion funding policy was to prevent cost from being a barrier to abortion access for lower income women.[60,61] To achieve this goal, the government faced a common unobserved type problem: it cannot perfectly observe which women are financially-constrained and which are not. Therefore, they rely on proxies (in this case age) to "tag" potential beneficiaries.[62]

The use of imperfect proxies to address the unobserved type problem lends itself to false negatives (FN) and false positives (FP) (Kleven and Kopczuk, 2011). The FN are women who will only be able to have the abortion if funding is provided (or the compliers), but are missed by a given tagging scheme. Similarly, the FP are women who would be able to have the abortion regardless of subsidies, or the always-takers in the context of this specific abortion funding policy. Under the existing policy, which tags financially-constrained women using age as a proxy, the FN are the financially-constrained women above the age cutoff that are not covered (blue area in Figure VIII). Likewise, the FP are the women below the arbitrary age cutoff who are not financially-constrained but are nonetheless covered under the current policy (orange area in Figure VIII).[63] Here we explore whether an alternative policy design can

[59]To better understand this result, we run the estimation for three sectors in which our socially and financially constrained population commonly works: restaurant and food services; the industrial sector; and the public service sector. Figure F2f presents the results by sector. We see a shift from the restaurant and food services to the public service sector. We attribute this shift towards white-collar employment to the increase in human capital investment.

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

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

[62]Other examples from Israel include a food stamps system and an "youth at risk" program, which determine eligibility according to information the government observes such as total earnings, wealth, and household size.

[63]Based on anecdotal evidence from Prof. Riad Agberia, a member of the committee in the ministry of health

better achieve the government's stated objective of eliminating financial barriers to abortion and compare the tradeoffs under different tagging schemes.

7.2 Constructing Alternative Policies

We begin by quantifying the FP by age (i.e. the number of women of each age who would have had the abortion even if not funded). Figure VIII plots the number of abortions before the policy change by age in orange. These women decided to abort regardless of the funding scheme (always takers), which are the shaded area below the orange line.[64] Next, we identify the FN (the number of women of each age who would not have the abortion without the funding coverage). First, we calculate the number of abortions that happen after the policy change, and then take the difference before and after the policy by age, which is the shaded blue area in Figure VIII (for formal statistical estimation of these difference see Appendix I.2).

The ratio between the FN and FP (blue and orange areas) for each age group indicates how efficient the current policy is at targeting financially constrained women at that age: a higher ratio means more financially constrained women are funded per one woman that would have paid for the abortion regardless (see Table A4 for the full set of calculations). Based on this definition of efficiency, we can plot the tradeoff between FN to FP from subsidizing each age group in Figure IXa. The y-axis sums the number of financially constrained abortions that are missed by the policy (FN). The x-axis sums the number of financially unconstrained women who are covered regardless of ability to pay (FP). More intuitively, the y-axis represents the number of 'missed' abortions and the x-axis, after scaling by the cost per abortion ($600), represents the government's cost of funding 'excess' abortions.

To illustrate this graphical framework, consider a 'no funding' policy (top left), under which there are 1,100 financially constrained women/year that conceive and want to abort but cannot (or abortions we 'missed'). Under the 'no funding' scheme, there are no excess costs of abortions, since none are funded (e.g., no FP). Then, we start plotting the ratio of the number of missed abortions to cost of excess abortions funded (or the FN/FP ratio) for the age groups funded under the current policy – beginning with the highest ratio. For example, based on this criteria, the most efficient age group to fund is 26 year olds, followed by 28 year olds, and then 27 year olds. Since the groups are added based on this efficient ratio logic, the groups are not added monotonically in age (see Table A4 that shows this calculation for each point on the figure). Note that the purpose of nonmonotonically adding age groups per this efficiency

that set up the funding scheme, the committee had a residual budget at the end of 2013 and needed to decide what to do with it. The committee members searched for requests they received and saw the letter from Dr. Eyal. The age cutoff was then set based on the number of women they could fund, given the remaining budget (Amsterdamski et al., 29.04.21).

[64]Note that the orange line does not extend below 20 since women 19 and younger were always funded. Similarly, the blue line does not extend above 32 since women 33 or older were never funded.

criterion is to map out an efficiency frontier that can be used as a benchmark for comparison to alternative tagging schemes.

Following the same logic, we continue to add all ages between 18-34 and get the entire possible funding frontier.[65] Once we add all of the age groups, we can observe the point on the frontier that corresponds to universal funding in the bottom-right corner. The existing policy, which funds women up to the age of 33, dominates universal funding with lower FP and essentially no additional FN. This is quite intuitive – the existing policy does not cover women aged 33 or older since they are likely to be financially unconstrained; this is further supported by the reduction in the estimated effect as women grow older, making women over 30 essentially unaffected (see Figure I2 for a formal statistical test).

By connecting all these dots, we get the shape of the feasible efficient frontier (see orange line in Figure IXb). Choosing a policy means selecting a point on this frontier and accepting the trade-off between the number of 'missed' abortions and 'excess' abortions funded from public funds. However, the orange frontier is constrained to the use of merely women's age as a tagging device. Using more observable information about the women will allow the government more flexibility in targeting financially constrained women, resulting in frontiers closer to the bliss point (in the southwest corner).

In Section 5 we showed the importance of parental earnings and religious background in driving demand for free abortions under the 2014 policy. Our interpretation of this finding is that the government funding allowed young women privacy and autonomy in making the abortion decision when they came from backgrounds where abortion was stigmatized and were unable to afford the cost on their own. While targeting social programs to low-income and disadvantaged populations using various forms of means-testing is a cornerstone of antipoverty programs globally, using demographic information such as religion to target populations is highly unusual, raises concerns over discrimination, and is politically impractical as a policy recommendation. Additionally, by explicitly targeting a population where abortion is stigmatized could lead to unintended policy consequences of further stigmatizing those women.

Therefore, we will perform an exercise where we combine age and parental earnings as an alternative tagging scheme and discuss in Section 7.3 how it compares to the current policy, which uses only age, and the universal funding option. To do this, we split the sample by age and parental earnings (above or below median) and calculate the efficiency frontier for all possible combinations, which is traced out in blue in Figure IXb. We can see modest improvements in the efficiency frontier, getting closer to the bliss point.[66]

[65] We stop this exercise at age 34 since the 2014 policy allows us to asses FN only up until 33 (due to the policy structure), adding older women would require us to assume that there are no FN for older ages and the exercise would be redundant for those age groups.

[66] As we showed in Section 5, the policy had the largest impact among young women from religious (Jewish), low-income families. We are not advocating a tagging scheme based on religious background, as discussed above,

7.3 Discussion

We demonstrate how a simple extension of the government's current age-based tagging scheme that more directly includes financial constraints, results in modest efficiency gains relative to the current policy. Importantly, the gains are not large and age may in fact be a useful proxy for financial constraints in this case. It is worth noting, that to incorporate socioeconomic status into this exercise, we rely on parental earnings, drawing directly from our empirical findings showing the greater importance of parental resources relative to the woman's own earnings in the sample of unmarried women aged 18-21. However, as we include older women in this exercise, parental earnings likely become less relevant than their own earnings, which may partially explain why the efficiency gains of adding this information are modest. This framework is intended as an illustrative example and can easily be extended to include different types of information and compare alternative tagging schemes.

Under this framework, choosing a policy requires making an explicit decision about the number of missed abortions (FN) the policymaker is willing to accept relative to the cost of funding 'excess' abortions from covering unconstrained women (FP). If the government cares primarily about reducing FN, or in other words minimizing the number of missed abortions, the current policy achieves this well. Notably, the number of FN under the current policy is very similar to the universal funding case.

Although the FN across the universal funding policy, existing policy, and the dominating policy which tags based on age and parental income are similar, they differ in the cost of 'excess' abortions funded. However, it is important to note that this current exercise only considers the direct cost of covering the abortion cost. We do not directly assess any differences in administrative costs of different tagging schemes, but can qualitatively say that a universal policy has the lowest administrative costs of the three and a policy based on age and parental income likely has the highest. More specifically, collecting family income information on the women might be costly, while asking the women to supply documents might discourage them, particularly if that means they need to discuss it with her family (thereby undermining they very privacy the existing policy affords). Additionally, there is a pure behavioral 'hassle cost' argument, which suggests women may be discouraged from having a wanted abortion due to the additional burden of providing different types of documentation.[67] Incorporating these

but note that performing a similar exercise that incorporates information on religion yields gains in efficiency (green line in Figure A8) and using both parental earnings and the religious background leads to additional gains in efficiency (purple line in Figure A8). This is unsurprising as this exercise is explicitly derived from our empirical findings.

[67] We attempt estimate the implications of a 'hassle cost' and find that one percent of constrained women who would want to apply are discouraged by this hassle cost. To identify this 1%, we used a 2004 policy that removed an administrative hurdle for securing the government subsidy for an abortion for 18 year-olds (see Appendix I.1 for more details). We interpret these estimates as an upper bound, given that younger women are more likely to be confused by navigating the administrative process.

costs would likely alter the tradeoffs. Finally, these assessments do not account for the forfeited earnings (and thus tax revenues) associated with having an unplanned birth, which we demonstrate in Section 6.

Finally, implementing any such policies requires a few words of caution. First, although many programs are means-tested in Israel it is nonetheless likely to be politically challenging to target funding for abortions based on SES background. Furthermore, funding abortion based on personal characteristics, such as income or religion, raises concerns about eugenics and the many cases of forced sterilization and reproductive coercion that have historically been imposed on disadvantaged populations around the world.[68] However, a key distinction is that we are considering a set of policies that aim to *remove* constraints, make it easier for women to obtain a wanted abortion, and give them greater privacy in making reproductive decisions. While the mere presence of a government policy that provides access to free abortion services for disadvantaged groups could signal a goal of discouraging reproduction among those populations, in the Israeli case we have shown that these are the populations that benefit the most from the existing policy.

8 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. 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 rate.

Our analysis investigates two primary mechanisms that explain this increase. First, we ex-

[68] Often embedded in national and international population policy discourse has been the notion that poor women were unfit to reproduce and the high fertility rates among poor women within rich and poor countries threatened the global economy. Thus, eugenic philosophies were reproduced in population policies and often shrouded language of reproductive choice (Dyck and Lux, 2016; Dyck, 2013; Woiak, 2010; Sasser, 2005). There exist numerous examples of such policies, including the Canadian government program that involuntarily sterilized indigenous women in the 1970s (Dyck and Lux, 2016; Dyck, 2013), the United States' mass sterilization campaigns throughout the 20th century that targeted Native American women, incarcerated people (particularly in California), and Puerto Rico (Zubrin, 2012; Morris, 2021; Woiak, 2010), incentives provided to doctors for IUD insertions in India under Indira Gandhi (Sasser, 2005), and the FAO and WFP making humanitarian aid to Haiti and Bangladesh contingent on implementing population control measures (Sasser, 2005).

amine 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 rate 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 *unplanned* parenthood imposes an added penalty to a woman's career. When abortion is not free, young women who cannot afford an abortion enter into early unplanned parenthood and possibly unplanned marriage; we show that this is avoided when the financial constraints to obtaining a wanted abortion are removed. Consequently, we find that avoiding early unplanned 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.

Finally, following these insights, we engage in an empirically driven exploration of optimal policy. When implementing the current policy, the government faces an unobserved type problem: it does not know which women are constrained from obtaining an abortion and would *only* be able to have a wanted abortion with financial support. The Israeli government enacted a policy that tagged women as credit-constrained women using age as a proxy. We suggest alternative policies that improve tagging precision by using other observable information on the women and provide a framework to assess the trade-offs of each funding scheme.

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 U.S., some states allow Medicaid-funded abortions for low-income women under certain conditions. Studies show that interruptions in Medicaid funding cause a reduction in the abortion rate, but the mechanisms and particularly the role of *privacy* and *social stigma* have not been investigated in this setting.[69] 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.

[69]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 rate.

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Table I: 2014 Change in Abortion Subsidy (Identification Strategy)

Age	**Free?**	
	Pre-2014	**Post-2014**
Age ≤ 19	✓	✓
$19 <$ Age < 33	X	✓
Age ≥ 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 age 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.

Table II: 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

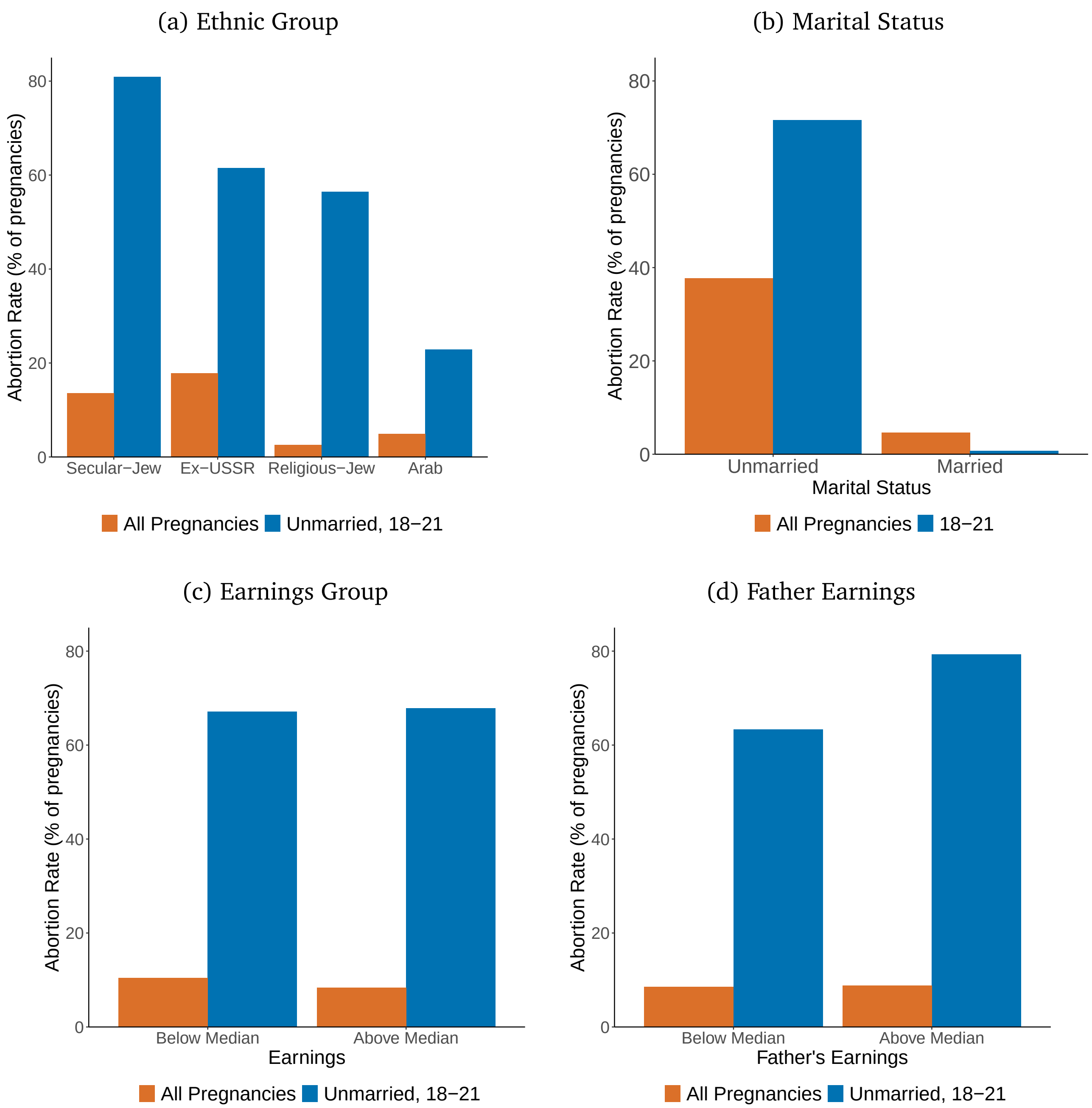
Notes: This table shows our sample construction, in terms of both total observations (pregnancies) and total women, described in Section 3. Panel A reports these sample sizes for the primary analytic sample – the conception panel. Panel B reports these sample statistics for the labor market panel. In 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 III: Effect of Removing Abortion Cost on Abortion Rate

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

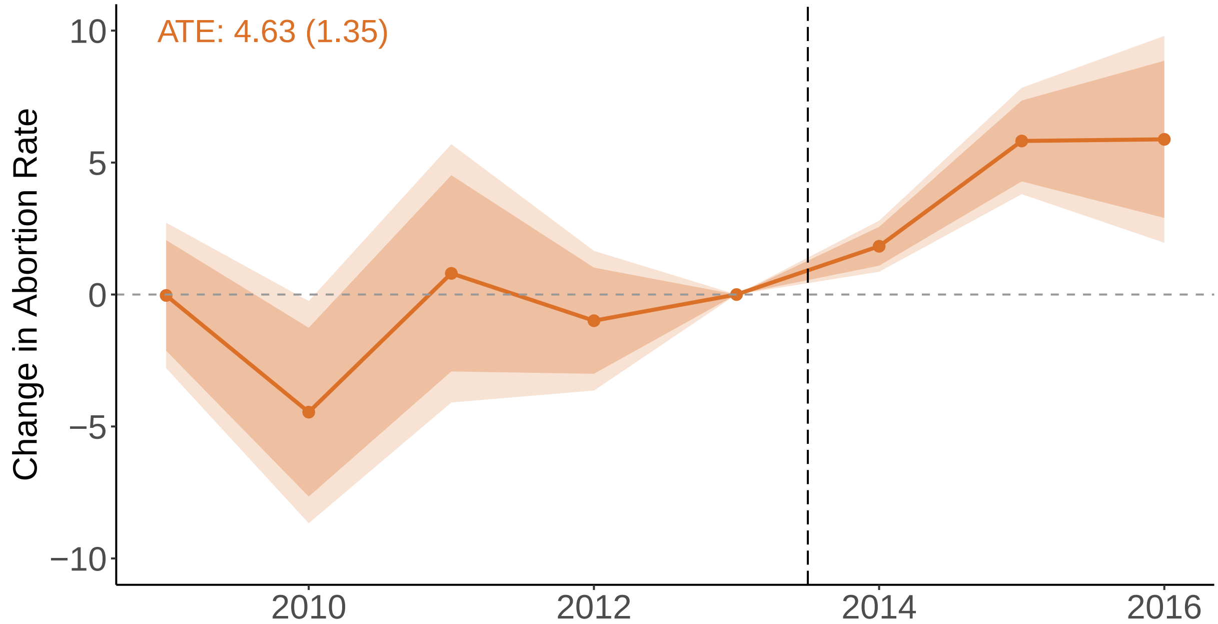
Notes: Standard errors in parentheses. This table presents the estimates on the abortion rate due to the 2014 policy that fully subsidized abortion. Our baseline specification follows Equation 1 as described in Section 4.1 – where we compare outcomes before and after the policy change for women who were affected and unaffected by the expansion of the subsidy. Women aged 19 and under were already fully subsidized by the government and therefore unaffected by the change and women aged 33 and older were not included in the subsidy expansion and thus never treated. We include a third specification using the married population as a third difference (DDD).

Figure I: Abortion Rate by Sub-Group



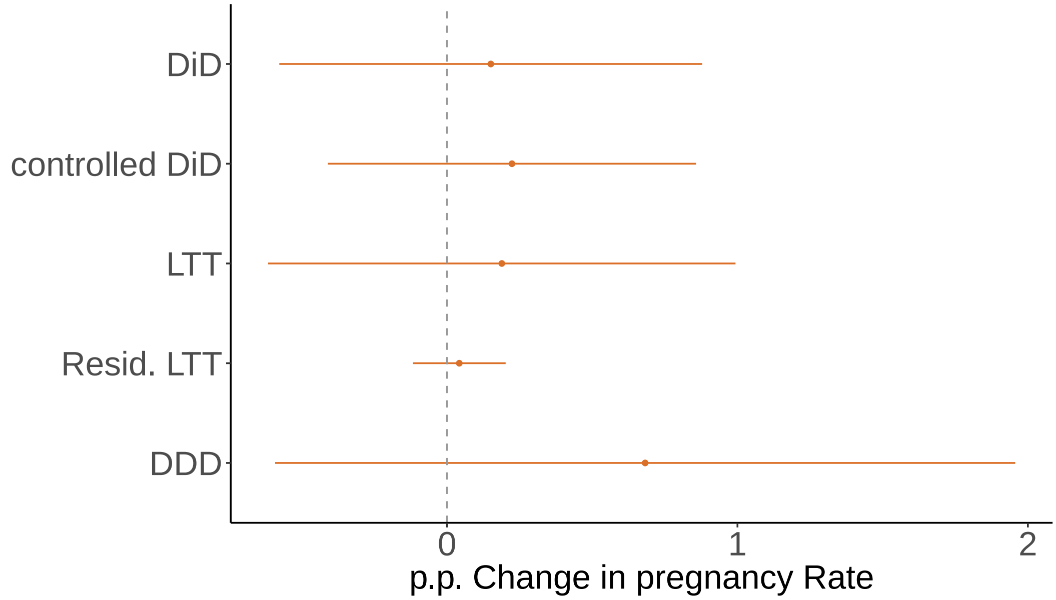
Notes: This figure presents abortions (as % of pregnancies) among important sub-groups within Israel. Each panel shows the proportion of abortions out of all pregnancies (orange) and our sub-population of unmarried 18-21 year olds (blue). Panel (a) presents the abortion rate by major ethnic groups in Israel. Panel (b) presents the abortion rate 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 rate by earnings group. Panel (d) presents the abortion rate by parental earnings group.

Figure II: Effect of Making Abortion Free



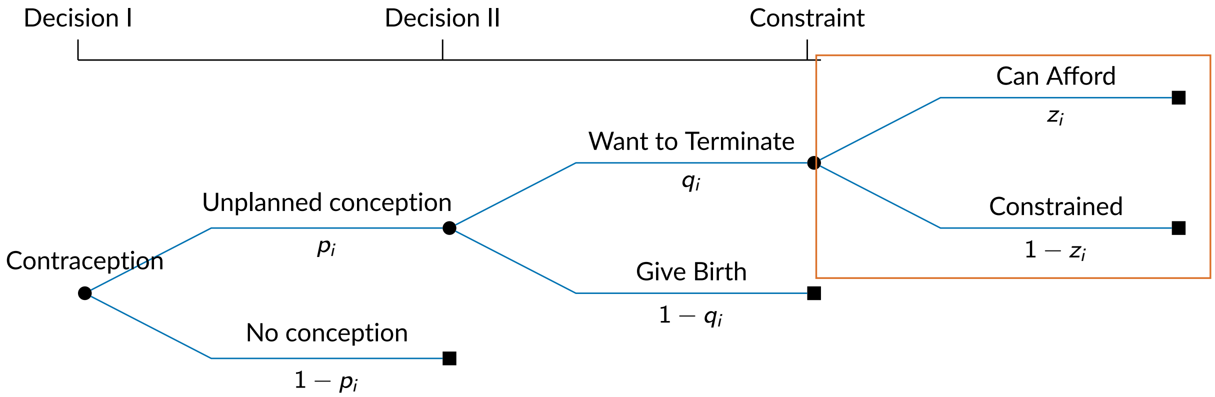
Notes: This figure presents the difference in abortion rate between treated (aged 20-21) and control (aged 18-19) women over time (2009-2016). The dashed line indicates the timing of the 2014 policy change. Each dot represents the coefficient δ_k estimated from the generalized difference-in-differences (Equation 2). Note that 2013 is dropped as the reference year. The shaded regions mark the 90% (dark orange) and 95% confidence interval (light orange) around the point estimates, respectively. In the top left we report the average-treatment-effect (ATE) estimated from Equation 1, Table III reports the corresponding estimates with robustness checks. The sample includes all unmarried women in the country aged 18-21 who conceived from 2009-2016.

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



Notes: This figure presents difference-in-difference results for the effect of the 2014 policy on conceptions. Each row presents the results from a different specification, whetre the dot represents the treatment effect ($\delta \cdot Post_t \times T_i$) and the lines mark the 95% confidence interval around the point estimate. The dashed vertical line is at 0, indicating an insignificant result (at the 5% level). The sample includes all unmarried women in the country aged 18-21 from 2009-2016. Treated women are those aged 20-21. The estimates are percentage point changes that can be interpreted as the relative change per 100 pregnancies.

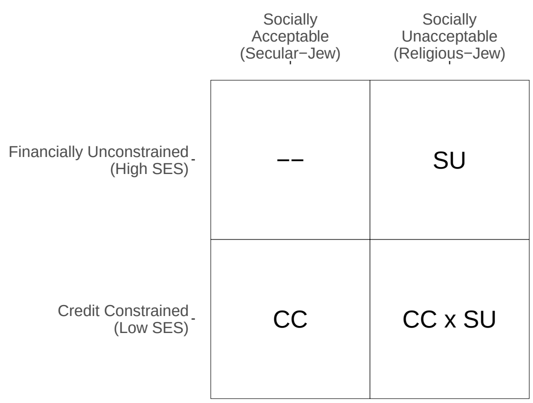
Figure IV: Decision Tree



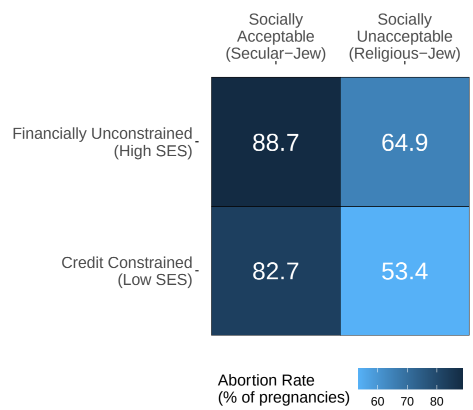
Notes: This figure illustrates our extension of the 'abortion as insurance' model: a two-step decision process as described in Section 5.1 and 5.2. In the model, a woman first makes a decision on contraception intensity p_i ($p_i \in [0, 1]$), which implies a conception will happen with probability $1 - p_i$. 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 E.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 V: Effect is Strongest Among the Socially and Financially Constrained Women

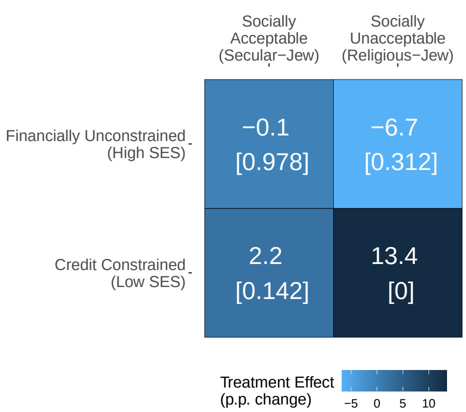
(a) Qualitative Predictions from the Model



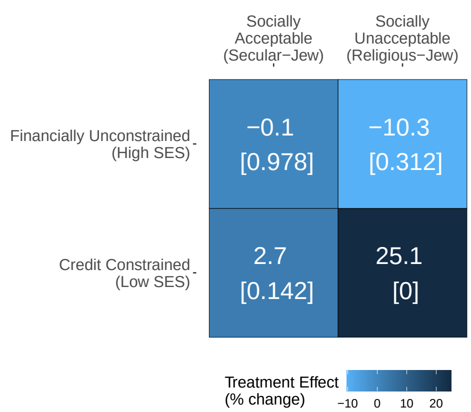
(b) Baseline Rates



(c) Policy Effect (p.p.)

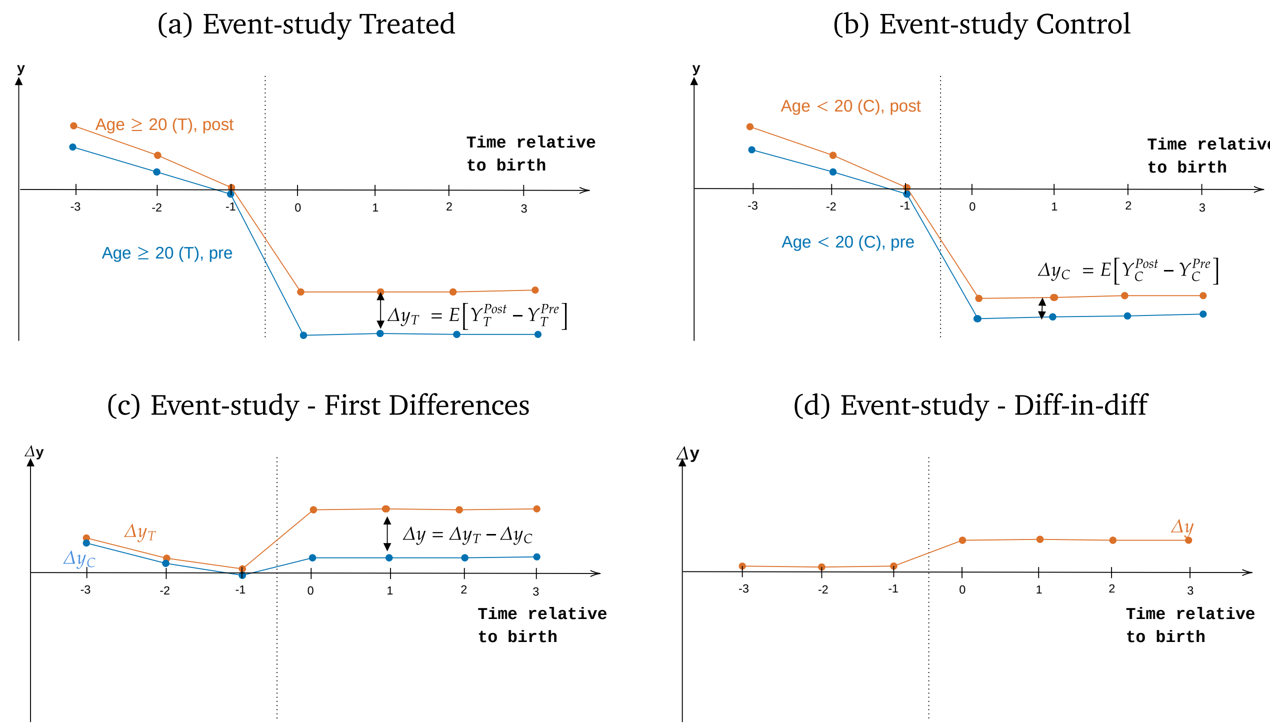


(d) Policy Effect (% Change Relative to Baseline Mean)



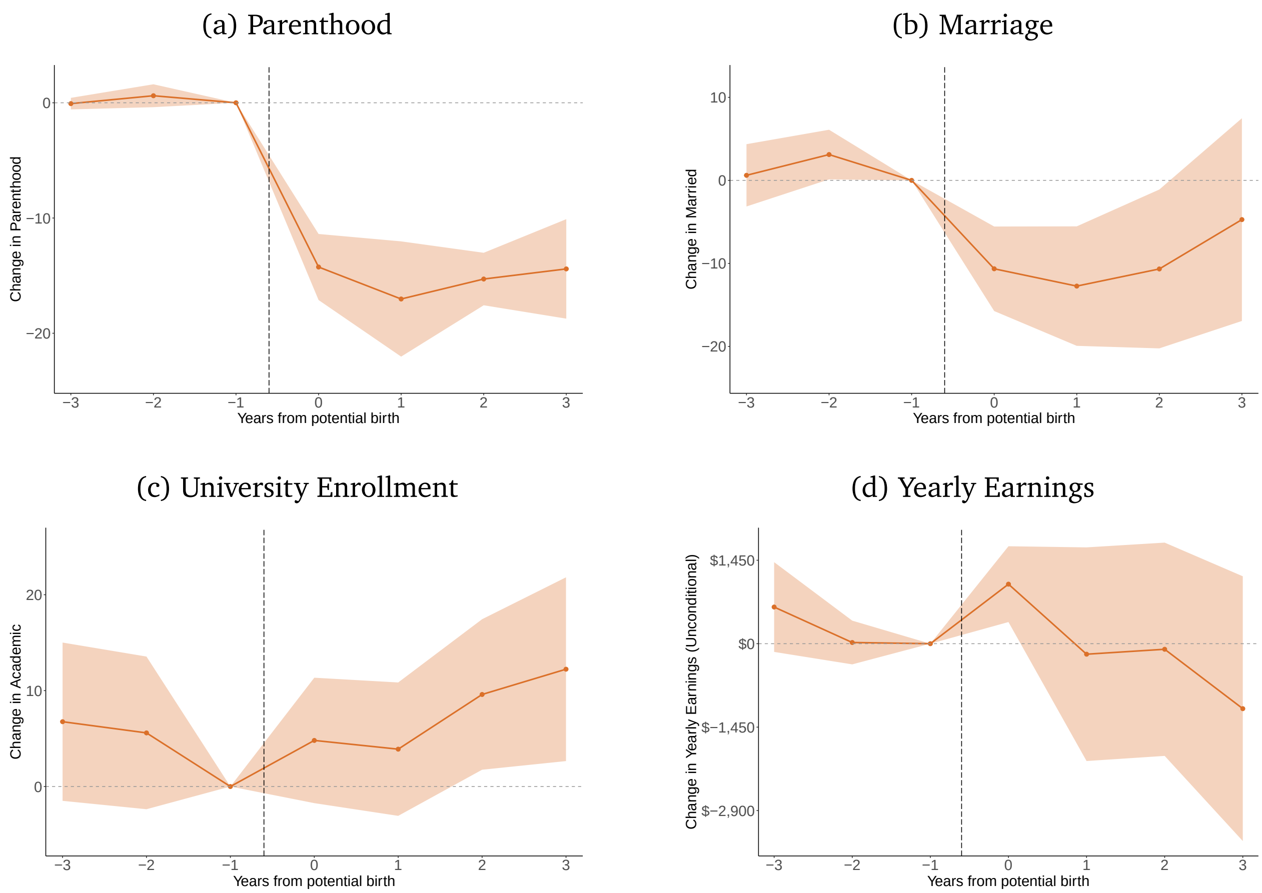
Notes: This figure presents the heterogeneous effect on abortion rate while splitting the population across two dimensions: religiosity level and SES background (based on parental earnings). Panel (a) presents the theoretical prediction based on social and financial constraints in the model; (b) presents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

Figure VI: Identification Illustration - Event-study X Difference in Differences



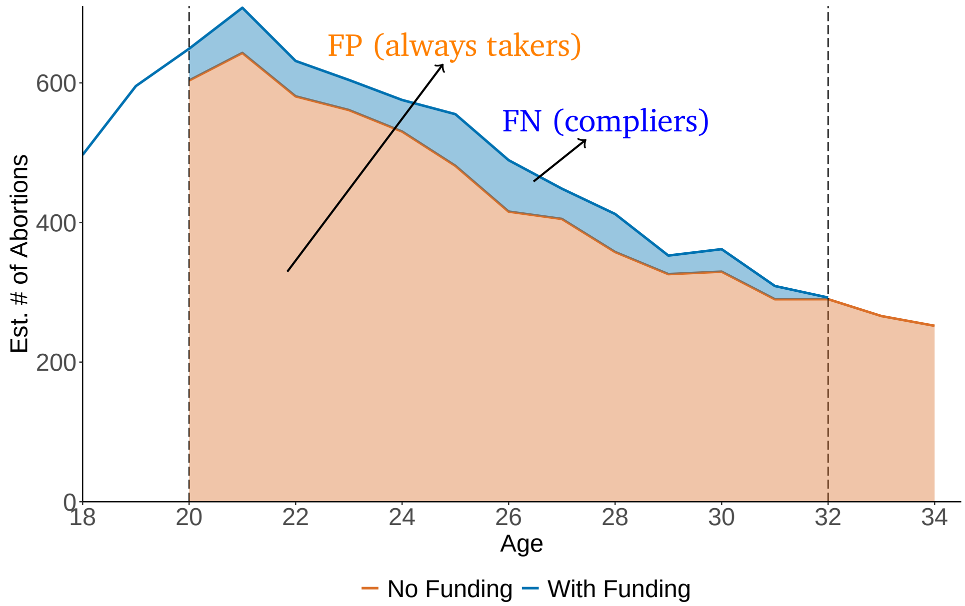
Notes: This figure illustrates the empirical strategy of Equation 4. Panels (a) and (b) show the conceptual event-study for a given outcome y (e.g., employment) relative to timing of potential birth for the population of women who conceived (pooling abortion and births), split by conceptions that happened pre-policy change (blue) and conceptions that happened post policy change (orange). Panel (a) illustrates the event-study for our treated group (age ≥ 20), while Panel (b) illustrates the corresponding event-study for our control group (age < 20). The differences between the lines in (a) and (b) are the first differences as illustrated in Panel (c). The difference between the first-differences are illustrated in Panel (d).

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



Notes: This figure presents the event study-DiD results for four outcomes: Panel (a) presents the results for the probability a woman will become a mother; Panel (b) presents the results on the probability a woman will get married; Panel (c) presents the results for the probability a woman will be enrolled in an academic, 4-year university program; and Panel (d) presents the results for the yearly earnings, unconditional on working. Each panel presents the results for the reduced form effect of the 2014 policy relative to year of potential birth as described in Equation 4. Each orange circle represents the treatment effect for the reduced form estimated, from three years prior until three years post potential birth timing, relative to one year prior to potential birth (the dropped year). The shaded regions mark 95% confidence intervals around each point estimate. The dashed vertical line is at 0, indicating an insignificant result (at the 5% level). The sample consist of unmarried women 18-21 years old who conceived between 2009-2016, and are socially and financially constrained (religious and low-SES, see Section 5.2).

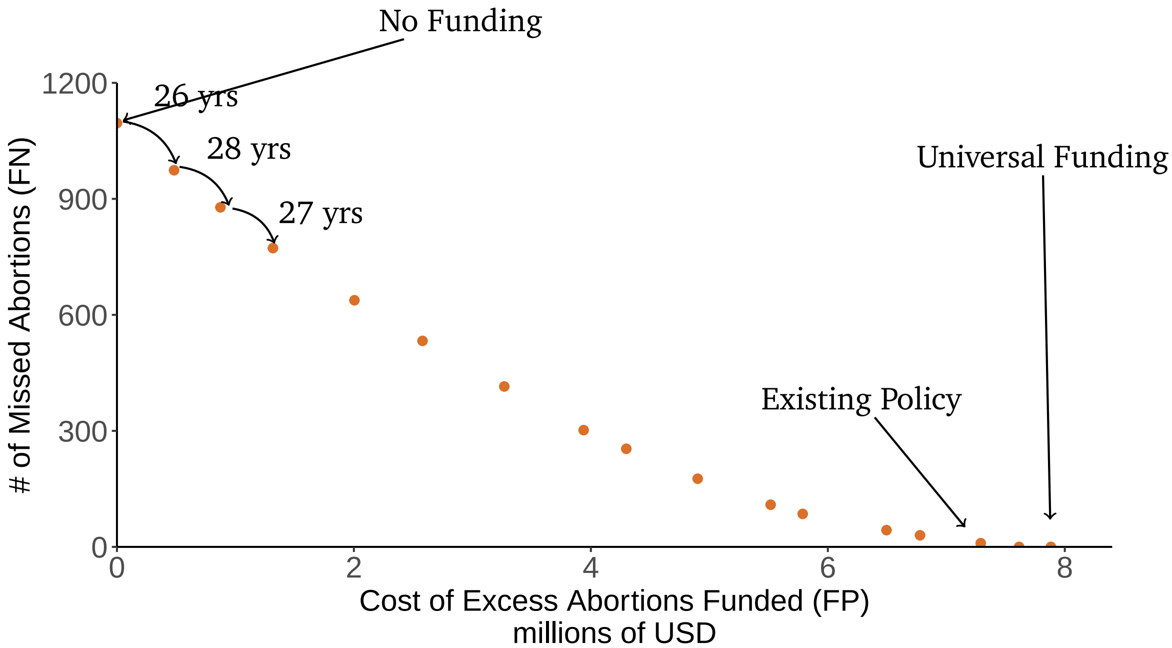
Figure VIII: Estimating Tagging Errors



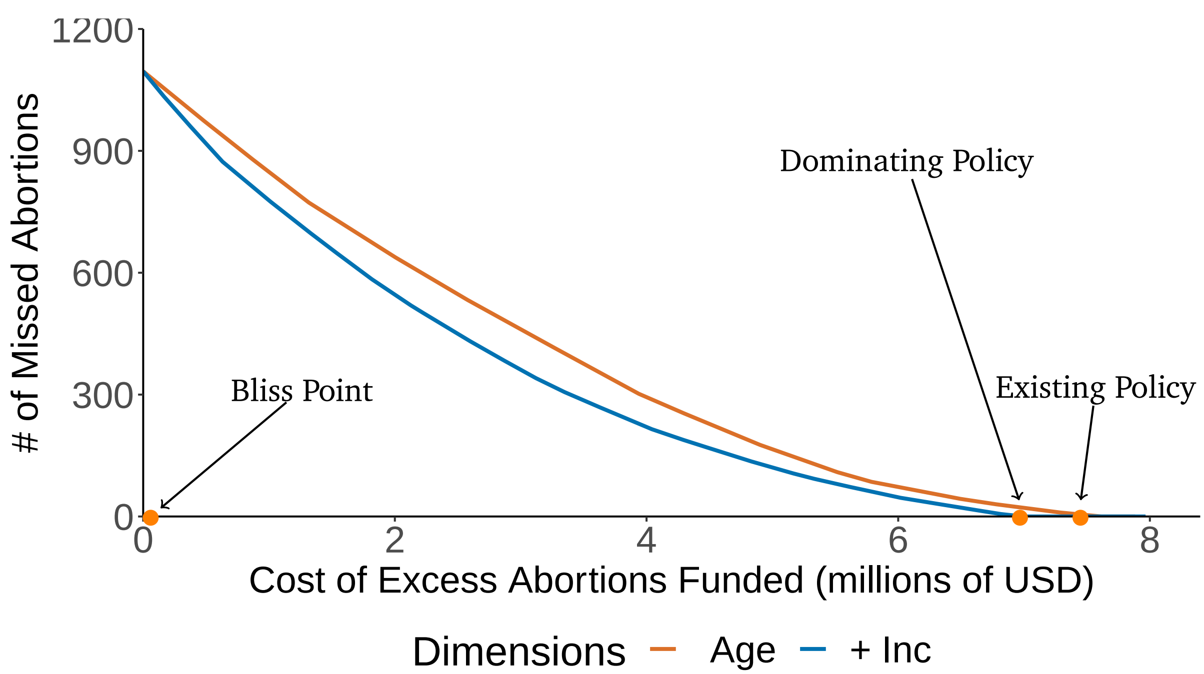
Notes: This figure plots in orange the average annual number of abortions by age before the policy change (2012-2013), which is our estimate of the number of false positives by age. The blue line plots the average annual number of abortions by age after the policy change (2014-2015). The area between the orange and blue lines is our estimate of the false negatives by age. For formal estimation of these difference see Appendix I.2.

Figure IX: Evaluating Policies

(a) # False Positives vs. # False Negatives (Existing Policy)



(b) # Abortions Missed vs. Excess Cost by Different Policies



Notes: This figure presents a framework to assess how efficient a given funding scheme is in targeting financially constrained women and to compare across different policies. Panel (a) plot the trade-off of FN to FP from funding an additional age group. The y-axis sums the number of financially constrained women that are not covered under each policy (FN). The x-axis sums the number of financially unconstrained women covered under each policy even though they can pay regardless (FP). For example, the most efficient group to fund are the 26 years old, followed by the 28 years old, and then the 27 years old. Panel (b) presents the efficient frontier by connecting all these dots (see orange line). The blue line corresponds to similar exercises where we use both the women's age coupled with her parental earnings. The y-axis represents the number of abortions we 'missed' (i.e., false negatives). We scaled the x-axis by the cost of each abortion (600$) to convey the cost of "excess" abortions (FP) funded in millions of dollars.