Women's Autonomy and Abortion Decision-Making*

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Abstract

We study how abortion subsidies affect abortion take-up using administrative data from Israel covering the universe of legal abortions. Leveraging a reform that expanded eligibility for government funding for abortion, we find that the subsidy significantly increased abortion, with the largest effects among young women from backgrounds with strict views on abortion. A simple model explains this pattern: subsidies reduce the need for parental financial support, shifting decision-making power toward the young woman. Our findings show that funding abortion expands young women's autonomy over abortion decisions, placing subsidies in the same policy space as parental consent laws. This mechanism is supported by both survey evidence on intergenerational mismatch in abortion attitudes and corroborative evidence from the United States.

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1 Introduction

The legal status of abortion remains contested worldwide, and even where it is legal, women often face substantial barriers to access (Singh et al., 2018). One key policy question concerns whether the financial cost of abortion meaningfully affects utilization. Standard economic reasoning suggests it should not: the monetary cost of carrying a pregnancy to term and raising a child far exceed the cost of an abortion. Yet, health policy and economic research document that even relatively small out-of-pocket costs impede access.[1] Understanding why relatively modest financial barriers produce such a large behavioral response is therefore essential for evaluating whether abortion should be publicly subsidized and how such policies ought to be designed.

Progress on this question has been limited by data constraints. Studying abortion policy is challenging because data is scarce, owing to the sensitivity of the topic. When abortion data exists, it is often aggregated and contains limited demographic detail or come from surveys where abortion is self-reported, making it difficult to uncover underlying mechanisms. We address this limitation using unique individual-level administrative data from Israel that covers the universe of births and legal abortions and links each pregnancy to rich demographic and economic information.

We use this data to study the effects of government healthcare subsidies on abortion take-up, leveraging a reform that expanded eligibility for public funding. Before the reform, women aged 19 and younger were fully covered, whereas those aged 20 and above paid the full cost out of pocket; after the reform, women older than 20 became newly eligible for full coverage. Our difference-in-differences design compares women who conceived just above and just below age 20.

Three empirical patterns emerge from our analysis. First, the subsidy increased abortion by 6.8 percentage points. Second, the effect of the subsidy is more than twice as large among young women from ethno-religious backgrounds with strict attitudes toward

[1]A large body of literature in health policy has shown how financial costs of abortion prevent or delay access (e.g., Fried, 2000; Doran and Nancarrow, 2015; Dickman et al., 2022; Upadhyay, 2022; Grossman et al., 2016). In economics the evidence show how financial costs impede access has primarily focused on the expansions and restrictions of Medicaid funding for abortion in the United States (e.g., Blank et al., 1996; Levine et al., 1996; Cook et al., 1999).

abortion—14.8 percentage points, compared with 5.6 percentage points among lenient-background women.[2] Third, the effect does not vary by household income. These patterns are inconsistent with frictionless or liquidity-constrained models of consumer behavior: a frictionless model would require implausibly high price elasticities, and a credit-constrained model predicts larger responses among poorer women. Neither framework accounts for the sharp heterogeneity by abortion attitudes.

To uncover the mechanisms behind these results, we develop a simple model in which the key friction arises from differences in abortion attitudes between young women and their parents. We define autonomy as the ability to obtain an abortion without parental approval. By relaxing daughters' dependence on parental financial support, a subsidy shifts decision-making authority toward the woman herself and thereby expands autonomy. The model predicts that increased autonomy affects behavior only when daughters and parents disagree about abortion; hence, effects are larger in communities where such intergenerational mismatches are more common. We validate this mechanism using survey data from Israel, which show that older individuals oppose abortion much more strongly than young women in strict communities, but not in lenient ones.

This framework can also be used in settings where individual-level data is unavailable, making it more broadly applicable. To illustrate this and assess the external validity of our findings, we derive location-level predictions and test them within and outside Israel. The model implies that the effects of increased autonomy should be larger in locations with stricter abortion attitudes. We confirm this using the Israeli data aggregated to the location level, and then revisit two influential U.S. studies of parental consent laws using data spanning 1960 to 2013 (Myers, 2017; Joyce et al., 2020). Although the original studies do not examine heterogeneity by religiosity, our analysis reveals strong variation by local religious composition: in more conservative communities, parental consent laws substantially reduce teen abortions and increase teen births, whereas effects are minimal in more lenient communities—consistent with the mechanism identified in our model.

[2]Israel exhibits substantial ethno-religious heterogeneity in abortion attitudes: secular Jews tend to hold more lenient views, whereas Orthodox Jews and Arabs generally hold stricter views. These differences appear in behavior as well: before the reform, 12.6% of pregnancies among lenient-background women ended in abortion, compared with 2.3% among strict-background women. See Figure 1.

Our results offer a new perspective on how to evaluate the merits of publicly funding abortion for young adults. Because the financial cost of an abortion is small relative to the lifetime cost of raising a child, the standard approach of comparing the fiscal cost of a subsidy to the welfare gains it generates for recipients is unlikely to be informative. Our analysis shows that subsidies relax parental control and enable young women to obtain an abortion without seeking financial support from their parents. Thus, the welfare question is fundamentally about autonomy: who should decide whether a pregnancy continues—the young woman or her parents? While our framework does not take a position on this normative question, it clarifies how the policy should be evaluated by placing abortion subsidies in the same conceptual space as parental consent laws, which have been extensively studied (e.g., Haas-Wilson, 1996; Kane and Staiger, 1996; Myers, 2017; Bitler and Zavodny, 2001; Joyce and Kaestner, 2001; MacBride, 2001; Levine, 2003; New, 2011; Colman and Joyce, 2009; Joyce et al., 2020; Myers and Ladd, 2020; GAO, 2025).

In the final part of the paper, we set aside the normative question of whether young women should have access to abortion and instead draw insights for policy design, taking as given that ensuring access is the policy objective. This exercise is motivated by the fact that many institutional environments provide partial but not comprehensive funding for abortion.[3] We show that a planner facing a budget constraint should prioritize locations with the lowest abortion ratios among young women. This has direct practical relevance for governments and NGOs that must allocate limited resources, especially since areas with low utilization are often first to face cuts.[4] Our findings suggest that such cuts risk reducing support precisely where it is most effective.

Our work contributes to the literature on the effects of abortion funding (Blank et al., 1996; Bitler and Zavodny, 2001; Levine et al., 1996; Cook et al., 1999; Kane and Staiger, 1996; Hoehn-Velasco et al., 2025). A recurring puzzle in this literature is why relatively small changes in out-of-pocket costs generate large behavioral responses, despite the far

[3] See Footman et al. (2023) for a review of the complex interaction between funding and legal regimes governing abortion services.

[4] For example, facing budget reductions, Planned Parenthood Michigan closed clinics in the most rural parts of the state because they served the fewest patients (Wells, 2025). At the national level, Planned Parenthood announced in July 2025 that as many as 200 clinics were at risk of closure after funding cuts under the Big Beautiful Bill Act, with roughly 60% of projected closures in rural or otherwise underserved communities (Planned Parenthood, 2025).

higher lifetime cost of raising a child. Our uniquely detailed administrative data help clarify this puzzle: economic barriers operate through social channels. In particular, subsidies allow young women to obtain abortions without involving their parents, which explains why the largest effects arise in more strict communities.

We also connect to the broader literature on barriers to abortion access, including mandatory waiting periods (Altındağ and Joyce, 2022; Joyce and Kaestner, 2001; Lindo and Pineda-Torres, 2019; Bitler and Zavodny, 2001), targeted restrictions on abortion providers (Colman et al., 2011; Fischer et al., 2018; Lindo et al., 2020), procedural barriers (Londoño-Vélez and Saravia, 2025), and clinic closures (Lindo et al., 2019; Venator and Fletcher, 2021; Myers et al., 2025). Most directly related is the literature on parental consent laws (Haas-Wilson, 1996; Kane and Staiger, 1996; Myers, 2017; Bitler and Zavodny, 2001; Joyce and Kaestner, 2001; MacBride, 2001; Levine, 2003; New, 2011; Colman and Joyce, 2009; Joyce et al., 2020; Myers and Ladd, 2020; GAO, 2025; Sanjuan, 2026). Our analysis and theoretical model shed light on a key mechanism at play in understanding who is most impacted by barriers to access—or conversely which populations may benefit the most from expanding access: those young women who cannot lean on family for support because of an intergenerational mismatch in attitudes toward abortion. Similar forces might also help explain other findings in the literature—for example, why the loss of a nearby clinic reduces abortion take-up (Lindo et al., 2019; Venator and Fletcher, 2021; Myers et al., 2025): proximity might enable young women to avoid parental involvement.

Our approach builds on the literature on economic models of abortion decision-making, which combines empirical evidence with formal frameworks that analyze abortion costs within broader settings of fertility, marriage, and child outcomes (e.g., Akerlof et al., 1996; Levine and Staiger, 2002, 2004; Ananat et al., 2009; Forsstrom, 2021). In this literature, abortion decisions are typically modeled without explicitly analyzing preference mismatch across agents. Forsstrom (2021) comes closest to our setting by incorporating parental involvement and stigma, but does not deliver sharp predictions for how changes in abortion costs differentially affect women from strict versus lenient communities.[5] Our framework

[5] In principle, existing models could be extended by allowing preferences or stigma costs to vary with abortion attitudes. As discussed in Appendix E.3, such extensions require additional functional-form assumptions and therefore do not yield disciplined comparative statics.

focuses on this intergenerational dimension and uses it to analyze both policy effects and survey evidence from Israel and the United States.

The rest of the paper is organized as follows. Section 2 describes the data and institutional context. Section 3 presents the empirical strategy and main results. Section 4 develops the theoretical model and tests its predictions. Section 5 discusses implications and extensions of our framework, and Section 6 concludes.

2 Data and Institutional Context

2.1 Data Sources

Conceptions (Abortions and Births): Our data on abortions come from administrative records of Israel's national abortion committee and include every woman who applied for approval between January 2009 and March 2016, including those who ultimately obtained abortions through private physicians. The data include each woman's demographic characteristics, gestational age at application, and the official approval outcome (see further details on the abortion committee in Section 2.2).

We link these data to the 2017 civil registry, which provides information on all recorded live births, date of birth of the woman, and her marital status at time of conception. Together, the abortion and birth registries capture the universe of recorded pregnancies in Israel during our study period—excluding only illegal abortions and early miscarriages.[6]

A key advantage of these data is that they allow us to measure abortions and births at the individual pregnancy level rather than relying on aggregate rates. When we report the share of pregnancies ending in abortion, we refer to this as the *abortion ratio*, to distinguish it from an abortion *rate* (abortions per 1,000 women of a given age). Both the abortion and birth records include gestational age, which we use to infer the date of conception and

[6] Abortions after 24 weeks of gestation are included in the abortion committee data, but these late-term cases are reviewed by a special committee and are rare (approximately 250 per year). We exclude them, along with stillbirths (defined as the death of a fetus after 20 weeks of gestation; roughly 1,400 per year), from the analysis. Early spontaneous miscarriages are also not observed; however, because these events are not deliberate and are unlikely to vary differentially across the treated and comparison age groups, their omission may add noise but not bias.

thereby construct a complete timeline for each observed pregnancy.

Ethno-Religious Classification: We combine data from the Census and the Ministry of Education to classify the ethno-religious background of women in our sample.

For Jewish women, we proxy religiosity using the type of secondary school they attended: secular (*mamlachti*), religious (*mamlachti-dati*), or Orthodox (*haredi*). Although individual religiosity may evolve later in life, school type reflects parental religiosity at the time of schooling decisions and therefore provides a relevant measure of the social environment in which the woman was raised, including the fertility and abortion norms to which she was exposed.

Ethnicity is recorded when individuals receive their national identification card and is reported in the Census. We use this information to construct an indicator for whether an individual is of Arab ethnicity.

As explained below in Section 2.2, we classify Orthodox Jews, Ultra-Orthodox Jews, and Arabs of any religion as having *strict* attitudes toward abortion. The remaining population—consisting mostly of secular Jews—are categorized as *lenient*. This classification reflects the substantially more permissive abortion attitudes among secular Jews relative to the other groups.

Socioeconomic Status (SES): To measure household economic resources, we use tax records on the young woman's earnings and her parents' earnings (father and mother), linked across generations using civil registry identifiers with a 96% match rate. Our main measure is the combined earnings of the woman's parents in the year of conception (household earnings). We classify households as low- or high-SES based on whether their earnings fall below or above the median earnings in our sample.

Table A.1 shows that this classification captures sizable differences in economic resources: high-SES households earn nearly five times more than low-SES households. SES is also correlated with other important characteristics: high-SES daughters are almost twice as likely to graduate from college, and low-SES families are disproportionately represented among religious and Arab households.

Although high-SES young women earn more than their low-SES counterparts, the difference in their own earnings is modest (15%), compared to a 465% difference in parental earnings. In both SES groups, daughters' own earnings constitute only a small share of total household income, implying that young women in our sample depend on parental financial support regardless of SES. Indeed, auxiliary evidence suggests that most women in our sample still live with their parents: Zionov (2021) reports that 84% of unmarried, childless women aged 18–24 live with their parents.

Education: We use data from the education registry spanning 2005-2016 that include information on the highest educational attainment: high school completion (*bagrut*), vocational training programs, and university degrees. We use the woman's education level at the time of conception to avoid endogeneity of subsequent education decisions due to an abortion.

2.2 Background on Abortion in Israel

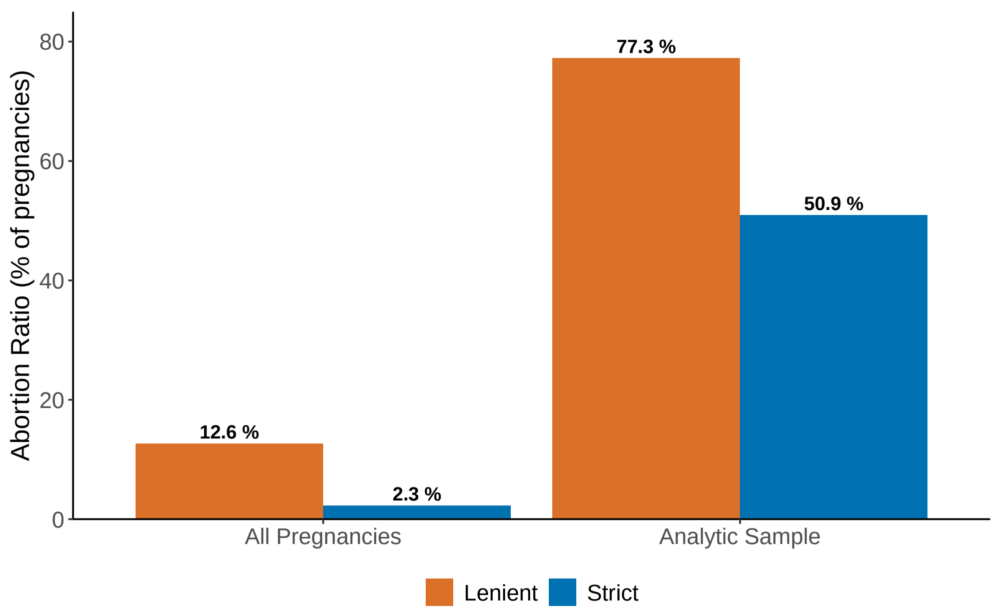
Abortion Attitudes. Abortion attitudes and behaviors in Israel vary substantially across ethno-religious groups. The population is roughly 75% Jewish, 18.6% Muslim, and 2% Christian, with the Muslim and Christian populations consisting predominantly of Arabs (Central Bureau of Statistics (Israel), 2018). Judaism generally permits abortion to protect the mother's life or health and, more broadly, when a child would be born into unstable circumstances (Amir, 2015). In practice, secular Jews—who constitute about 45% of the Jewish population—tend to hold permissive views, have high contraceptive use, and relatively low fertility, whereas Orthodox and Ultra-Orthodox Jews—roughly 30% combined—strongly oppose abortion, use contraception rarely, and have high fertility. Among Muslim- and Christian-Arab minorities, abortion is more heavily stigmatized and often considered taboo; Islam generally prohibits abortion except to save the mother's life (Shapiro, 2014; Bhalotra et al., 2021), and studies of similar populations document strong social sanctions around abortion (Foster et al., 2007).

Due to these differences in abortion attitudes, we define the secular Jewish population as having *lenient* attitudes toward abortion and combine Orthodox Jews, Ultra-Orthodox

Jews, and Arabs of any religion into a single group with *strict* attitudes toward abortion.

These differences in abortion attitudes are reflected in the abortion ratios shown in Figure 1. In the lenient population, 12.6% of pregnancies end in abortion, compared with 2.3% in the strict population. Among our analytic sample of unmarried women—described in detail below in Section 3.1—abortion ratios are substantially higher, but the large gap between lenient and strict groups persists. This variation underscores that abortion access and decision-making in Israel take place within a highly segmented social and religious landscape (see Figure A.1 for a more detailed ethno-religious breakdown).

Figure 1: Abortion Ratios in Israel by Abortion Attitudes



Notes: The figure reports abortion ratios (the percentage of pregnancies that end in abortion) for two populations: all women aged 16–45 who conceived between 2009–2013 (on the left), and the analytic sample—defined in Section 3.1—of unmarried women aged 16–23 who conceived for the first time in the same period (on the right). Within each population, observations are grouped by attitudes toward abortion: "lenient" (secular Jews) and "strict" (religious Jews, Ultra-Orthodox Jews, and Israeli Arabs).

Public debate around abortion in Israel generates relatively little political polarization or media attention compared with other high-income countries, and legal reforms have historically encountered limited opposition from either secular or religious parties. As a result, the abortion system provides relatively easy legal access, but operates within the context of substantial normative heterogeneity across groups. Further details on the Israeli context are provided in Appendix B.1.

Abortion Committee. Abortion has been legal in Israel since 1977, contingent on approval from a committee composed of two medical professionals and a social worker. All abortions—public or private—require this approval. Social workers typically assist applicants throughout the process (Oberman, 2020), and, as a result, approval rates are extremely high: in our data, 99% of applications are approved, and 97% of approved applications result in an abortion.

The committee approves an abortion if at least one of the following conditions is met: (1) the woman is under 18 or over 40; (2) the pregnancy occurred outside marriage; (3) the pregnancy resulted from rape or incest; (4) the pregnancy endangers the woman's life or health; or (5) the fetus has a congenital disorder (see approval shares in Table B.1a). Importantly, all unmarried women qualify automatically under the out-of-marriage criterion, while married women must meet another condition or report that the pregnancy is out of wedlock. More details on the committee process are provided in Appendix B.2.

Given the high approval rates and confidentiality of the process, incentives for illegal abortions are minimal. Anecdotal evidence suggests that some abortions occur outside the committee system (Oberman, 2020), but credible estimates on the quantity of these cases are unavailable.[7] Moreover, in Section 3.3 we show that there is no evidence that the 2014 reform induced women who would have otherwise obtained an illegal abortion to instead obtain a legal one.

Abortion Cost. Once approved, women pay an out-of-pocket cost for an abortion, in contrast to most healthcare services in Israel. In 2014, the public-sector cost ranged from NIS 2,100–3,500 (USD 588–980), depending on gestational stage. Private procedures (which still require the committee's approval) are faster but costlier, up to NIS 8,000 (USD 2,240). For comparison, average monthly earnings for working women were NIS 7,666 (USD 2,146),

[7]A newspaper article claimed that 15,000 illegal abortions occur annually in Israel (Newman, 2017), but we were unable to verify this figure: the organization cited in the article could not identify the underlying data, and the reporter did not respond to follow-up inquiries. A related concern is that ordering medication abortion pills online could provide a way to bypass the committee, but such practices were not widespread in Israel during the time period covered by our analysis (Oberman, 2020).

and for young women in our sample NIS 2,109 (USD 590).[8]

Policy Change: Eliminating the Cost of Abortion. Before 2014, only women aged 19 or younger or over 40 and those meeting medical or legal criteria were eligible for fully subsidized abortions (see Table B.1a). According to abortion activists,[9] women who were not eligible were often surprised to learn, upon arrival at the clinic, that they would need to pay between $588 and $980 for the procedure, since co-pays are typically rare (and small) in the Israeli healthcare system. Many women—especially religious women—struggled to secure this amount on short notice because they could not ask friends or family members for financial support for an abortion.

In January 2014, following advocacy efforts and a surplus in the national healthcare budget, the government expanded eligibility for full public funding (Amsterdamski et al., 2021). As shown in Table B.1b, the reform extended coverage from age 19 to all women up to age 32, including the committee fee (Kelner, 2013). Age was used as a proxy for income, with 32 as a budget-driven upper cutoff. After the reform, all women aged 32 or younger could obtain an abortion free of charge once approved, while the approval process itself remained unchanged. To the best of our knowledge, no other reproductive or income-related policies in Israel exhibit discontinuities at ages 19 or 32.

3 The Effect of Subsidies on Abortion Decisions

In this section, we present our main empirical analysis: estimates of the effects of the 2014 extension of abortion subsidies, with particular focus on heterogeneity by abortion attitudes. Section 3.1 defines the analytical sample. Section 3.2 presents the main results. Section 3.3 reports a series of robustness checks and shows that our findings remain stable.

[8] See Appendix B.3 for details on childrearing costs in Israel.

[9] Interview conducted by the authors with Sharon Orshalimy, Israeli reproductive-justice activist and 2013 Young Leader with Women Deliver, Tel Aviv, Israel, July 2019.

3.1 Sample

Our main analytical sample consists of unmarried women aged 16–23 at the time of conception of their first pregnancy. We restrict the sample to *unmarried* women because pregnancies conceived within marriage can be aborted only for health reasons, or if the woman is younger than 18 or older than 44—cases that were already subsidized prior to the 2014 reform. The policy therefore primarily affected unmarried women.[10] We further restrict to first pregnancies to avoid endogeneity in marriage decisions: a prior abortion may alter subsequent marriage decisions, implying that the marital status of women with earlier pregnancies could itself be influenced by the subsidy. Limiting the sample to young women (16–23) ensures comparability between the treated (20–23) and control (16–19) groups. The sample period, 2009–2016, is chosen to avoid contamination from earlier policy changes. Appendix C provides additional details on these restrictions. After applying these restrictions, the final sample includes 40,495 pregnancies. Section D shows that our results are robust to alternative sample definitions.

3.2 Main Result: Effect Heterogeneity by Abortion Attitudes

To estimate the effect of the subsidy on abortion, we employ a difference-in-differences design that exploits both the timing of the 2014 Israeli policy change and the age cutoff at 19 years (see Table B.1b). Formally, we estimate the following regression:

$$abort_{it} = \delta \left(Post_t \times T_i\right) + \gamma_{a_i} + \gamma_{y_t} + \gamma_{m_t} + \beta X_{it} + \epsilon_{it} \ , \tag{1}$$

where the dependent variable $abort_{it}$ equals one if woman i, who conceived in month–year t, obtained an abortion. The year and month corresponding to t are denoted by y_t and m_t, respectively. The treatment indicator $T_i \equiv \mathbb{1}\{20 \leq age_i\}$ identifies women whose eligibility for the subsidy was affected by the reform, based on their age at conception, and

[10] Abortion is rare for married women: only 3.9% of pregnancies among married women aged 16–45 ended in abortion, in contrast to 32.6% among unmarried women. See Table B.1b for details on legal criteria for abortion and subsidy eligibility.

$Post_t \equiv \mathbb{1}\{y_t \geq \text{Dec-2013}\}$ marks conceptions occurring after the policy took effect.[11] The coefficient δ captures the average effect of the subsidy on the probability of obtaining an abortion, conditional on pregnancy, expressed in percentage points.

We include age-at-conception fixed effects (γ_{a_i}) to control for systematic differences in abortion and fertility behavior across ages, and conception-month fixed effects (γ_{m_t}) to account for seasonality in conception. The term X_{it} is a vector of individual controls with associated coefficients β. In our preferred specification, X_{it} includes interactions between education and ethno-religious group fixed effects, which capture key determinants of abortion and fertility decisions (Kearney and Levine, 2012; Eckstein et al., 2019; Almond et al., 2019). Appendix D.1 discusses the rationale for these controls and examines alternative specifications. The error term is ϵ_{it}, and standard errors are clustered at the age-by-year level, corresponding to the level of treatment assignment (Abadie et al., 2022).

OLS estimates from Equation (1) are presented in Column (1) of Table 1. We find that the subsidy increased the abortion ratio by 6.8 percentage points, corresponding to 9.2% of the baseline value.

Why did the subsidy lead to more abortion? If this result were driven primarily by a price effect or by easing liquidity constraints, we would expect a stronger response among low-SES women, who are typically more price sensitive and financially constrained. To test this, we estimate Equation (1) separately by socioeconomic status (SES). The results, reported in Columns (2) and (3) of Table 1, show that the difference in effects between low- and high-SES women is small and statistically insignificant, suggesting that financial constraints are not the main channel.

An alternative mechanism is that the subsidy alleviates a social, rather than a purely financial, constraint. Ineligible young women—who must pay up to $980 for an abortion—may need to involve their parents to cover the cost. For women coming from families with strict attitudes toward abortion, this may preclude having the abortion altogether. The subsidy therefore allows young women to obtain an abortion without involving their par-

[11]The policy was implemented in January 2014. However, women conceiving in late 2013 could apply for the subsidy if the abortion occurred in early 2014, when the reform was already in place. Because most legal abortions in Israel occur within the first eight weeks of pregnancy, we shift the treatment date one month earlier to include conceptions in December 2013 that were effectively treated under the new policy.

Table 1: Impact of the 2014 Subsidy Expansion on Abortion Utilization

		Socioeconomic Status		Abortion Attitudes	
	Full Sample	Low SES	High SES	Lenient	Strict
Treatment Effect	0.068***	0.071***	0.064***	0.056***	0.148***
	(0.011)	(0.014)	(0.013)	(0.01)	(0.027)
	[-0.02, 0.04]	[-0.03, 0.07]	[-0.06, 0.03]	[-0.04, 0.03]	[0.07, 0.29]
P-value		0.707		0.002	
Baseline Mean	0.738	0.658	0.830	0.773	0.509
N	40,495	21,558	17,030	35,231	5,264

Notes: This table reports OLS estimates of Equation 1, which identifies the effect of the 2014 reform on abortion ratios. The sample consists of all unmarried women aged 16–23 who became pregnant for the first time between 2009–2016. Columns (2) and (3) report estimates separately for women from high- and low-SES families; Columns (4) and (5) report estimates separately for women from communities with lenient and strict attitudes toward abortion. P-values correspond to tests of statistical differences between Columns (2) and (3), and between Columns (4) and (5). All regressions include month fixed effects and the interaction of ethno-religious-group and education fixed effects. Standard errors, clustered by age-by-year at conception, are in parentheses. Upper and lower bounds that account for differential pre-trends appear in brackets (details in Appendix D). $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

ents. To explore this hypothesis, we estimate Equation (1) separately for women from strict and lenient backgrounds. The results, shown in Columns (4) and (5) of Table 1, support this interpretation. Consistent with the relevant frictions being social rather than financial, the effects differ sharply by family attitudes toward abortion. Among women from lenient backgrounds, the subsidy increased the abortion ratio by 5.6 percentage points (7.2% of the baseline). Among women from strict backgrounds, the effect was much larger—14.8 percentage points, or 29.1% of the baseline. The difference between the two groups is statistically significant ($p < 0.01$).[12]

A potential concern is that the observed heterogeneity by abortion attitudes may instead reflect differences in socioeconomic status, since women from strict backgrounds tend to have lower average income in Israel.[13] However, Table 1 shows no evidence of heterogeneity in the subsidy's effect by SES. To further address this concern, Table A.3 reports estimates

[12] Table A.2 shows that each strict subgroup (Religious Jews, Ultra-Orthodox Jews, and Arabs) exhibits substantially larger effects than the lenient group. All differences are statistically significant except for the Ultra-Orthodox, where small sample size limits precision. Effects do not differ significantly across strict subgroups, supporting our decision to pool them to increase statistical power.

[13] In our analytic sample, 18% of low-SES women come from strict communities, compared with 8% of high-SES women (Table A.3).

of Equation (1) separately by the interaction between SES and abortion attitudes. Within both the strict and lenient groups, the estimated effects are similar across low- and high-SES women and are statistically indistinguishable. By contrast, within both SES groups, the effects remain significantly larger for women from strict backgrounds than for those from lenient backgrounds. Taken together, these results indicate that the stronger response among strict-background women is not driven by income differences, but by underlying variation in abortion attitudes.

We next turn to the identifying assumptions underlying our difference-in-differences design. Equation (1) relies on the standard parallel-trends assumption that, in the absence of the policy change, treatment and control groups would have evolved similarly. We assess this assumption by estimating the following regression:

$$abort_{it} = \sum_{\substack{k=2009 \\ k \neq 2013}}^{2016} \delta_k \, \mathbb{1}\{y_t = k\} \cdot T_i + \gamma_{a_i} + \gamma_{y_t} + \gamma_{m_t} + \beta X_{it} + \epsilon_{it} \;, \tag{2}$$

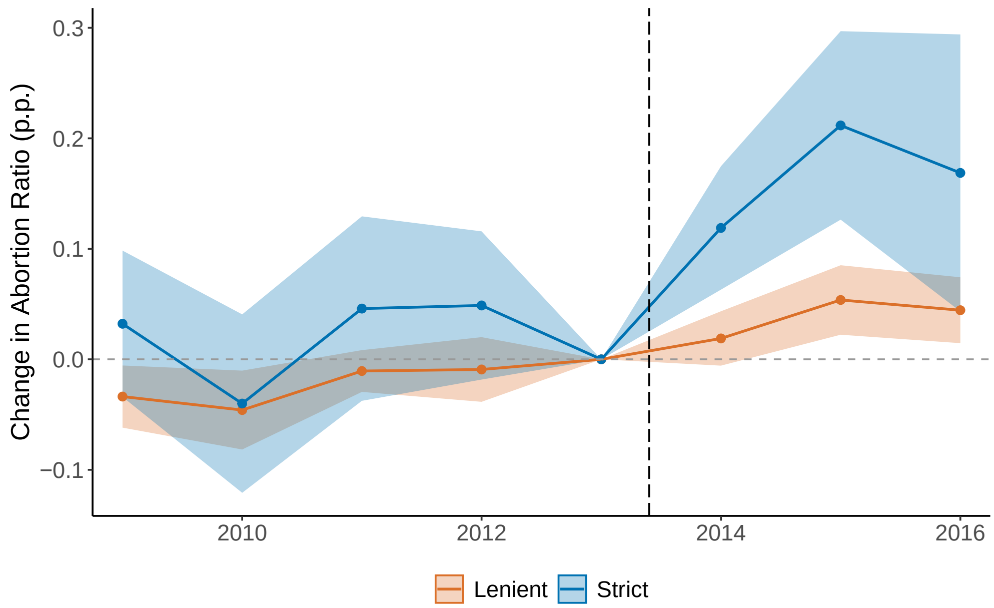
where the coefficients of interest, δ_k, trace the dynamic effects of the policy. For $k > 2013$, δ_k captures the policy's impact in year k, while for $k < 2013$ it reflects potential pre-trends. The baseline year (2013) is normalized to zero, $\delta_{2013} = 0$.

We estimate Equation (2) separately for women from lenient and strict backgrounds, with results shown in Figure 2. Estimates for the full sample are shown in Figure A.2. Among women from lenient backgrounds, we observe modest differential trends between treatment and control groups both before and after the reform, making it difficult to determine whether the policy had no effect or only a small one. In contrast, among women from strict backgrounds, there is no evidence of differential pre-trends prior to the reform, followed by a sharp and sustained increase in abortion rates for the treated group thereafter.

These results suggest that the large post-reform response among women from strict backgrounds is not driven by differences in trends, but rather reflects a causal effect of the subsidy.

Next, in Section 3.3, we examine the robustness of our findings and explore alternative model specifications to assess the stability of these results.

Figure 2: Dynamic Difference-in-Differences Estimates of the Subsidy Effect



Notes: The figure plots the dynamic difference-in-differences coefficients from Equation 2, which measure changes in abortion ratios between treated women (ages 20–23) and control women (ages 16–19) from 2009–2016. Estimates are shown separately for communities with lenient and strict attitudes toward abortion. Each point corresponds to a coefficient δ_k; 2013 is the omitted year. The vertical dashed line marks the 2014 reform. The sample includes all unmarried women aged 16–23 who conceived for the first time between 2009–2016. All regressions include month fixed effects and the interaction of ethno-religious-group and education fixed effects. Standard errors are clustered by age-by-year at conception, and shaded regions depict 95% confidence intervals.

3.3 Robustness

We now address several potential concerns with the results presented in Section 3.2 and show that our main conclusions are robust—particularly the large effect of the subsidy among women from strict backgrounds. Appendix D provides detailed descriptions of all robustness exercises, while this section summarizes the key results.

Accounting for Pre-Trends. The estimates in Table 1 rely on the standard parallel-trends assumption between treatment and control groups. To account for potential deviations from this assumption, we follow Rambachan and Roth (2023) and construct treatment effect bounds that are valid under differential trends estimated from pre-treatment data. The resulting bounds are shown in brackets in Table 1. For the lenient population, the bounds include zero, indicating that we cannot rule out the possibility of no effect or a small positive effect. In contrast, for the strict population, the bounds confirm a strong positive effect even when allowing for violations of parallel trends. The two sets of bounds do not overlap, providing additional evidence that the subsidy's impact was larger for the strict population. Further details are provided in Appendix D.2.

Effect of Subsidy on Pregnancy. By reducing the cost of abortion, the subsidy also reduced the cost of an unwanted pregnancy. This could, in principle, reduce incentives for contraceptive use and thereby increase pregnancy rates. Such an effect would bias our estimates, since our main analysis conditions on being pregnant. To test this possibility, we re-estimate Equation (1) in a broader sample that includes all unmarried women aged 16–23 between 2009 and 2016, without conditioning on pregnancy. The dependent variable is an indicator for whether a woman conceived in a given year. Results, reported in Table D.1, show no evidence that the subsidy increased conception rates: the estimated coefficients are negative and statistically insignificant. Appendix D.3 provides further details.

The null effect on pregnancy also alleviates concerns that the subsidy induced women who would otherwise obtain an illegal abortion to obtain a legal one. Legal abortions are observed in the administrative data and therefore appear as pregnancies, whereas illegal abortions are not recorded. If substantial substitution from illegal to legal abortions had occurred, we would expect to see an increase in observed pregnancies. We find no such increase.

Changes in Sample Composition. The treatment and comparison groups may have experienced differential compositional changes around the time of the reform. To test for this, we estimate a simplified difference-in-differences model analogous to our main specification (Equation 1), but excluding controls to focus on changes in observable characteristics. Results, reported in Table D.2, show that treatment and control groups are balanced across most characteristics, with the exception of education. To address this imbalance, our preferred specification includes fixed effects for the interaction between education and ethno-religious group when estimating the effects of the subsidy. Tables D.3, D.4, and D.5 further demonstrate that the results are robust to including or excluding these fixed effects, as well as to the addition of several alternative controls. Additional details are provided in Appendix D.1.

Alternative Sample Definitions. Our findings are robust to alternative sample choices, including narrower age bandwidths (Appendix D.4) and specifications that do not restrict the sample to unmarried women (Appendix D.5).

Additional Exercises. Appendix D presents additional robustness checks, including analyses that account for the large influx of migrants from the former USSR in the 1990s, who had a distinct historical relationship with abortion (Appendix D.6), as well as effects on health-related abortions (Appendix D.7).

Taking Stock. This section has presented a comprehensive set of robustness analyses of our main results. Across all exercises, a consistent pattern emerges: the subsidy had a large and statistically significant positive effect on abortion rates among women from strict backgrounds. For women from lenient backgrounds, the evidence is less consistent across specifications, and we cannot empirically distinguish between no effect and a modest positive effect. In the next section, we develop a simple theoretical framework to rationalize this observed heterogeneity in the subsidy's impact.

4 Model: Abortion Decisions with Conflicting Attitudes

This section develops a simple model to interpret the empirical findings in Section 3. We first show that neither frictionless frameworks—which treat abortion as a standard consumption good—nor models with liquidity constraints can reproduce the empirical patterns. We then introduce a model with *intergenerational constraints*, where we define *autonomy* as the ability to obtain an abortion without parental permission. In this framework, autonomy affects behavior only when there is a mismatch between daughters' and parents' abortion preferences. We interpret the abortion subsidy as increasing autonomy, since it enables young women to obtain an abortion without parental financial support. This framework accounts for the empirical findings in Section 3, including the heterogeneity of the subsidy's effects across groups with different attitudes toward abortion.

4.1 Can Standard Models Explain our Empirical Findings?

This section considers two standard models and shows why they cannot account for the empirical patterns documented in Section 3.2. Both treat the abortion decision as a standard optimization problem without intergenerational frictions, differing only in whether women face liquidity constraints. More details are provided in Appendix E.[14]

No Constraints

In a frictionless model, women compare the utility of having an abortion and giving birth, accounting for the monetary cost of abortion, C^A, and the present discounted cost of childrearing, C^B. When the procedure is subsidized, C^A is eliminated.

The key implication is that a single price sensitivity parameter governs responses to both C^A and C^B. To rationalize the observed effects of the subsidy, this parameter must be large—so large, in fact, that it would imply implausibly large changes in abortion rates in response to small percentage fluctuations in C^B. This prediction is at odds with the observed stability of abortion rates over time. For example, Cohen and Romanov (2013), using Israeli data, estimate the elasticity of fertility with respect to the cost of raising a child to be 0.54. In comparison, we show in Appendix E that a frictionless model, when calibrated to match observed policy effects, implies an elasticity of 15.8—orders of magnitude larger.

The model also fails to account for the heterogeneity in policy effects. Since poorer women place a higher marginal value on money, this model predicts larger behavioral responses among low-income groups. Yet, we find no systematic differences by income (Tables 1 and A.3).

[14]Appendix E also considers an alternative model based on stigma costs. While this framework is structurally similar to our preferred model, it cannot replicate all empirical patterns without imposing additional assumptions on the functional form linking stigma costs to parental attitudes.

Liquidity Constraints

This formulation retains the same structure as in the frictionless model, but introduces liquidity constraints. Some women may wish to have an abortion but lack the resources to pay the cost. In this case, a large observed policy effect need not imply a high price sensitivity parameter; it may instead reflect that many women are liquidity constrained.

Nevertheless, the predictions remain inconsistent with the data. This model implies larger effects for poorer women, who are more likely to be liquidity constrained, but this pattern is not present in the data (Table 1). Nor does the model explain why effects are stronger in stricter communities. Unless women in such communities are disproportionately liquidity constrained, differences by strictness should not emerge. Empirically, however, the gap persists even after conditioning on parental income (Table A.3), suggesting that liquidity constraints alone also cannot account for the observed heterogeneity.

4.2 A Model with Intergenerational Frictions

We model abortion decisions in an environment where the relevant constraint is not financial but social. In particular, pregnant young women—henceforth *daughters*—require financial support from their parents to obtain an abortion, unless it is subsidized. Subsidizing abortion thus has no effect on daughters' preferences per se, but it alters the set of feasible choices by relaxing parental control.

Let a daughter's type be denoted by h, which indexes the leniency of prevailing attitudes toward abortion in her community, with $h = 1$ indicating lenient attitudes and $h = 0$ strict attitudes.[15] Let $V_d(h)$ denote the daughter's latent desire to obtain an abortion, and let $V_p(h)$ denote the parents' latent willingness to provide support for the procedure. Both $V_d(h)$ and $V_p(h)$ are treated as random variables. In this framework, the daughter seeks an abortion if $V_d(h) \geq 0$, and parental assistance is available if $V_p(h) \geq 0$. The fact that both are functions of the same underlying type h reflects the shared socioeconomic and cultural environment that shapes the preferences of both generations. We define *autonomy* as the

[15] For expositional clarity and to better align the model with our empirical analysis, we use a binary classification of social norms. All results in this section extend naturally to a continuous measure of leniency.

ability to obtain an abortion without parental permission.

Let $W \in \{0,1\}$ denote the policy environment: when $W=0$, there is no government funding for abortion, and the procedure requires both the woman's desire and her parents' financial assistance; when $W=1$, government support is available, and having an abortion is determined only the woman's desire. Let A be an indicator for whether a woman has an abortion. Then:

$$A = \begin{cases} \mathbb{I}(V_d(h) \geq 0 \text{ and } V_p(h) \geq 0) & \text{if } W = 0, \\ \mathbb{I}(V_d(h) \geq 0) & \text{if } W = 1. \end{cases} \tag{3}$$

It follows directly from Equation (3) that an abortion subsidy increases autonomy by allowing women to obtain an abortion without parental permission. Consequently, the policy has no effect in the absence of intergenerational mismatch—that is, when parents are always willing to support an abortion desired by their daughter ($\mathbb{P}(V_p \geq 0 \mid V_d \geq 0) = 1$). If daughters and parents are aligned in their abortion preferences, parental consent is non-binding, and the subsidy does not change behavior. This highlights that a divergence in preferences between daughters and parents is a necessary condition for the policy to have any effect in our framework.

Remark 4.1. In our empirical setting, parental assistance is required to finance an abortion in the absence of a subsidy. Hence, women lack autonomy ($W = 0$) when abortion is not subsidized. The same framework, however, applies more broadly to other contexts where parental involvement is necessary for access. For example, limited autonomy may arise from parental consent laws or from barriers such as long travel distances to clinics, which may make young women dependent on their parents for logistical or financial support (Lindo et al., 2019; Venator and Fletcher, 2021).

To understand heterogeneity in the effect of the subsidy across social groups, consider how the policy effect varies with h. Define the policy effect in levels as:

$$\Delta(h) \equiv \mathbb{P}(A = 1 \mid W = 1, h) - \mathbb{P}(A = 1 \mid W = 0, h). \tag{4}$$

Let us also define the percent (relative) effect:

$$\delta(h) \equiv \frac{\Delta(h)}{\mathbb{P}(A = 1 \mid W = 0, h)}. \tag{5}$$

We begin by establishing the following result, which characterizes how the percent effect of the policy varies with the leniency of prevailing social norms:

Proposition 1. Percent Effect of Abortion Subsidy Decreases with Leniency.
Suppose the pair $(V_d(h), V_p(h))$ has a joint density that is strictly positive and continuous on $\mathbb{R}^2$, and that this density satisfies a monotone likelihood ratio property (MLRP) in h. Then the percent effect of an abortion subsidy, $\delta(h)$, is smaller in communities with more lenient abortion attitudes, i.e., when $h = 1$.
Proof: *See Appendix H.1.*

The MLRP assumption ensures that more lenient attitudes shift the joint distribution of daughter and parent preferences in a way that makes both more favorable toward abortion. This means that in more lenient communities, both the woman and her parents are more likely to support abortion.[16]

The intuition behind this result is straightforward. In our framework, the subsidy affects behavior solely by increasing daughters' autonomy (Equation 3). As a result, behavior changes only when a daughter wants an abortion ($V_d \geq 0$) but her parents are unwilling to provide support ($V_p < 0$). In more strict communities ($h = 0$), parental opposition is more common, raising the likelihood that the subsidy enables access that would otherwise be denied. Although fewer daughters in such communities wish to abort, this does not affect the conclusion of Proposition 1, which concerns the *relative* effect of the policy—that is, the proportional increase in abortions relative to the baseline rate.

Proposition 1 yields a testable prediction under mild assumptions, which we use to validate the model in Section 5.2. It also provides guidance for targeting in optimal policy design, as discussed in Section 5.3.

[16]Formally, the monotone likelihood ratio property (MLRP) holds if the ratio $\frac{f_{V_d,V_p}(v_d,v_p|h=1)}{f_{V_d,V_p}(v_d,v_p|h=0)}$ is increasing in (v_d, v_p), for all $(v_d, v_p) \in \mathbb{R}^2$. This implies that $h = 1$ make higher realizations of (V_d, V_p) more likely in the likelihood ratio sense.

While Proposition 1 is useful for its generality, it characterizes effects only in percentage terms, whereas the empirical analysis in Section 3 shows that subsidy effects are also larger in levels for women from more strict communities. This distinction is substantive: the total effect, $\Delta(h)$, and the percent effect, $\delta(h)$, are related as follows:

$$\underbrace{\Delta(h)}_{\text{total effect}} = \underbrace{\mathbb{P}(A = 1 \mid W = 0, h)}_{\text{baseline abortion rate}} \times \underbrace{\delta(h)}_{\text{percent effect}} . \tag{6}$$

Under the assumptions of Proposition 1, the baseline abortion rate is lower in more conservative communities ($h = 0$), while the percent effect is higher. Therefore, observing a larger total effect in strict communities implies that the percent effect must be sufficiently large to offset the lower baseline rate. This pattern does not follow mechanically from the model and requires additional structure. In particular, the total effect depends critically on the extent of *intergenerational mismatch* in abortion attitudes.

To formalize this idea, define $G(h)$ as an index of intergenerational mismatch in group h:

$$G(h) \equiv \mathbb{P}(V_d(h) \geq 0) \cdot \mathbb{P}(V_p(h) < 0). \tag{7}$$

The index $G(h)$ is high when many daughters in group h wish to obtain an abortion ($V_d(h) \geq 0$) and many parents in that group oppose it ($V_p(h) < 0$).

Proposition 2. Total Effect of Abortion Subsidy Decreases with Leniency.
Suppose the pair $(V_d(h), V_p(h))$ has a joint density that is strictly positive and continuous on $\mathbb{R}^2$, and that this density satisfies the monotone likelihood ratio property (MLRP) in h. Assume further that

$$G(0) > G(1).$$

Then the total effect of an abortion subsidy, $\Delta(h)$, is smaller in communities with more lenient abortion attitudes, i.e., when $h = 1$.
Proof: *See Appendix H.2.*

Proposition 2 sheds light on the mechanism underlying the empirical pattern documented in Section 1—namely, that the policy has a stronger effect in more conservative communities. Our interpretation is that intergenerational mismatch is more prevalent in these communities, as formalized by the assumption $G(0) > G(1)$. According to Proposition 2, this greater mismatch translates into a larger total effect of the policy. In Section 4.3, we empirically validate this prediction using survey data and show that intergenerational mismatch over abortion is indeed more frequent in conservative environments.

An alternative explanation is that *within-household mismatch* is more pronounced in strict communities—that is, the correlation between daughters' and parents' preferences, $\text{Corr}(V_d, V_p)$, is lower in those environments. In Section 4.3, we test this hypothesis and find that it is not supported by the data.

In sum, this section has developed a model of abortion decisions that emphasizes the role of autonomy and intergenerational mismatch. We derived two propositions concerning the effects of government support for abortion access. Propositions 1 and 2 serve distinct but complementary purposes. Proposition 1 establishes a general result under minimal assumptions. Its generality makes it particularly well-suited for empirically testing the model, as its prediction should hold across a wide range of institutional contexts. In contrast, Proposition 2 relies on stronger assumptions to account for the empirical observation that the total effect of the policy is larger in more conservative communities. This result deepens our understanding of the mechanism at work, highlighting how variation in intergenerational mismatch mediates the policy's impact.

4.3 Testing the Mechanism: Intergenerational Mismatch

In Section 3.2, we showed that the abortion subsidy had a stronger effect in more strict communities, even though baseline abortion rates were lower in those communities. Proposition 2 offers an explanation for this pattern: intergenerational mismatch over abortion attitudes is larger in strict communities than in lenient ones.

We now test this mechanism directly using public opinion data. We use data from the Israeli World Values Survey (WVS) Wave 4, collected in 2001 (Inglehart et al., 2014), and define *Young* women as those aged 18–23.[17] We contrast this group with older generations,

[17]Although our main empirical analysis includes women aged 16–23, 18 was the minimum age surveyed

defined as men and women aged 45–75. We classify respondents as *Religious* if they report that religious faith is an important quality that children should be encouraged to learn at home.[18] Attitudes toward abortion are based on responses to whether abortion is justifiable on a scale from 1 (never justifiable) to 10 (always justifiable), with *Lenient* attitudes defined as responses of 6 or higher.

To assess external validity, we replicate the analysis using U.S. public opinion data from the 2023–2024 Pew Research Center Religious Landscape Study (Pew Research Center, 2025). We define *Young* women using the same age range as in the WVS and define the older generation as men and women aged 44–73, the closest available cohort match. We classify respondents as *Religious* if they report that religion is very or somewhat important in their lives, and we classify *Lenient* abortion attitudes based on whether respondents believe abortion should be legal in most or all cases, as opposed to illegal in most or all cases.

To quantify intergenerational mismatch in abortion attitudes within each context, we estimate the following regression separately using Israeli and U.S. data:

$$\text{Lenient}_i = \beta_0 + \beta_Y \text{Young}_i + \beta_R \text{Religious}_i + \beta_{YR}(\text{Young}_i \times \text{Religious}_i) + \epsilon_i, \tag{8}$$

where β_Y measures the difference in abortion attitudes between younger and older respondents, β_R captures the difference between religious and secular individuals, and β_{YR} measures how much larger the age gap is within religious communities relative to secular ones. The term ϵ_i captures idiosyncratic variation.

OLS estimates of Equation 8 using Israeli data are reported in the first column of Table 2. The coefficient on *Young* is small and statistically insignificant, indicating little intergenerational mismatch in abortion attitudes among secular communities. Combined with Proposition 2, this finding helps explain why the subsidy had only modest effects for this group: when parents and children agree, increased autonomy does not change behavior.

In contrast, the coefficient on *Religious* is negative, large, and statistically significant, confirming that religious respondents hold stricter views on abortion. This validates our classification of these groups as "strict" in Section 3.2.

in the WVS.

[18] Respondents were asked to rank up to five qualities that were important to impart to children.

Table 2: Survey Evidence on Intergenerational Mismatch in Abortion Attitudes

	Israel (WVS)	US (Pew)
Young	-0.072	0.018
	(0.064)	(0.016)
Religious	-0.415***	-0.363***
	(0.043)	(0.006)
YoungXReligious	0.218**	0.085***
	(0.104)	(0.029)
N	517	19,527
Dep. Var. Mean	0.665	0.429

This table reports OLS estimates of Equation (8) using public opinion data from Israel (World Values Survey Wave 4, 1999–2004) and the United States (Pew Research Center Religious Landscape Study, 2023–2024). Each column presents a regression of an indicator for holding a lenient attitude toward abortion on indicators for being a young woman (ages 18–23), for belonging to a more religious or conservative group, and their interaction. The coefficient on *Young* captures intergenerational differences among secular respondents; the coefficient on *Religious* captures average differences between religious and secular individuals; and the interaction term *Young* $\times$ *Religious* measures how the intergenerational gap varies with religiosity. Standard errors are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Finally, the interaction term *Young* $\times$ *Religious* is large, positive, and statistically significant, consistent with the prediction that intergenerational mismatch is greater in religious communities. This supports the interpretation that a larger intergenerational mismatch in abortion attitudes explains the stronger response to the subsidy in communities with strict abortion attitudes.

Column (2) of Table 2 reports analogous estimates using U.S. survey data. The results reveal similar patterns, suggesting that the mechanism we identify may extend beyond the Israeli context.

An alternative explanation for the stronger response to the subsidy in strict communities is that it reflects not a larger overall intergenerational divergence, but rather a greater *within-household mismatch*—that is, a lower correlation between daughters' and parents' abortion attitudes, Corr(V_d, V_p). Appendix F evaluates this possibility by combining the estimates from Tables 1 and 2 to calibrate the model and recover the implied within-household correlation in abortion attitudes. The results show that this correlation is actually higher in strict communities, ruling out weaker within-household alignment as the source of the larger policy effects.

In sum, the results in this section show that survey-based measures of abortion attitudes are consistent with the conclusions of our theoretical framework, which highlights intergenerational mismatch as the key driver of the subsidy's effects. Moreover, these results demonstrate how observed behavioral responses to abortion subsidies can be used to infer underlying abortion attitudes among young women and their parents.

5 Implications and Extensions

We now discuss several implications and extensions of our model. First, we examine welfare considerations and clarify what our framework can say about the normative question of whether abortions should be subsidized. Second, we extend the model to incorporate location-level heterogeneity and use this extension to analyze data from the United States. Finally, we draw insights for policy design.

5.1 Welfare: Shifting Decision-Making Authority

Our findings offer a new perspective on the normative question of whether abortion should be subsidized. In standard welfare analysis, individuals who change their behavior in response to a subsidy must be better off, by revealed preference. The key issue would then be to compare the fiscal cost of the subsidy to the welfare gains it generates for recipients. In the context of abortion, however, this comparison is unlikely to be informative. The monetary cost of an abortion procedure is minimal relative to the total cost of raising a child. Consequently, the welfare gain to a pregnant woman who avoids an unwanted birth will almost always exceed the subsidy's fiscal cost, even under generous assumptions about the marginal cost of public funds. The exception would be a knife-edge scenario in which the non-monetary costs of abortion are nearly as large as the costs of childbirth, so that the net welfare gain is negligible and comparable to the procedure's price. Therefore, conventional cost-benefit reasoning is unlikely to illuminate the welfare implications of abortion subsidies.

What, then, is the relevant normative issue? Our findings suggest that the core question is decision-making authority. We show that abortion subsidies do not operate through a standard price effect; rather, they allow pregnant young women to bypass the need for parental financial support—and with it, parental consent. In this way, the subsidy effectively shifts decision-making power from parents to the pregnant woman herself. This reframes the normative stakes: the central issue is not whether the subsidy increases welfare in a revealed-preference sense, but rather who should have the authority to decide whether the abortion takes place—the young woman or her parents.

Our paper does not take a position on this normative question. Rather, our contribution is to clarify that this is the question at the heart of the welfare analysis. In this light, abortion subsidies should be understood not merely as financial transfers, but as part of a broader class of policies that influence the allocation of decision-making authority between parents and children. This framing connects our analysis to a substantial body of work across economics, philosophy, and law has examined the trade-offs between shielding young individuals from potentially harmful decisions and respecting their developing autonomy (e.g., Lundberg et al., 2009; Doepke and Zilibotti, 2017; Tunick, 2023).

More directly related to our context, this perspective aligns closely with the literature on parental consent laws for abortion. These laws, which require minors to involve a parent before accessing abortion services, have been extensively studied. Some argue that they protect minors from hasty decisions and promote family involvement (MacBride, 2001; New, 2011). In contrast, a substantial empirical literature finds that such requirements delay access to care (Colman and Joyce, 2009), increase rates of teen births and early marriage (Myers and Ladd, 2020), and reduce young women's educational attainment and labor market outcomes (GAO, 2025). GAO (2025) provides a comprehensive review of the broader impacts of parental involvement laws on reproductive and socioeconomic trajectories.

The policy tool we study is distinct—it operates through public subsidies rather than formal legal rights—and our contribution is precisely to show that it produces similar behavioral effects. As such, the normative evaluation of abortion subsidies should engage with the same questions of decision-making authority and individual autonomy that are central to the parental involvement literature. This insight extends to other domains in which public policy loosens intergenerational constraints, highlighting that the effects of economic instruments such as subsidies often operate through social rather than purely financial channels. Student loan programs, for example, enable young adults to pursue higher education even when their parents are unwilling or unable to contribute (Keane and Wolpin, 2001; Lochner and Monge-Naranjo, 2011), thereby shifting control over educational investment from the family to the student.

In sum, our findings highlight that abortion subsidies influence behavior not primarily by altering financial incentives, but by reallocating decision-making authority within the family. Recognizing this connection clarifies the relevant welfare considerations and opens the door for more precise debates about the role of the state in funding reproductive choice.

5.2 Applying the Model to Location-Level Data

In Section 3.2, we showed that abortion subsidies have larger effects among women from families with stricter abortion attitudes. That analysis relied on rich microdata with individual-level information on pregnancies, abortions, and religiosity. While such granularity strengthens the credibility of our findings, it also limits their replicability: similarly detailed data are rarely available in other contexts, complicating assessments of external validity.

To address this limitation, Appendix G.1 extends our theoretical framework to the location level, where data is more commonly available. We summarize the key theoretical insights here and then test their empirical predictions using two types of evidence: (i) our main dataset aggregated to the location level and (ii) external datasets from the United States.

The location-level model is a direct extension of the framework in Section 4, and its main implications mirror those of Propositions 1 and 2. We characterize the leniency of local abortion attitudes using two alternative measures: the baseline (no-subsidy) abortion rate and the share of lenient individuals in each location.

Under the same mild assumptions as in Proposition 1, the model predicts that the *percent effect* of the subsidy is smaller in more lenient locations. Moreover, under the additional assumption of Proposition 2—that intergenerational mismatch in abortion attitudes is greater in strict communities—the model also predicts that the *level effect* of the policy is smaller in such locations. Both predictions hold under either definition of location-level leniency.

Validating the Location-Level Model with Israeli Data

We now revisit our main results with a focus on heterogeneity across locations.[19] To do so, we re-estimate Equation (1) separately for each location and collect the corresponding location-level estimates of the subsidy effect, δ_l. We also compute the associated percent effect, $\widetilde{\delta}_l \equiv \frac{\delta_l}{\mathbb{P}(A=1|W=0,l)}$, where $\mathbb{P}(A = 1 \mid W = 0, l)$ denotes the baseline (no-subsidy) abortion rate in location l.

The results are in Figure 3, where each circle represents an estimate from a separate regression. The y-axis in Panel (a) shows estimates of the subsidy's effect in percentage terms and Panel (b) in levels. Computing the average across these location-specific estimates yields an overall effect of 8.4 percentage points. Reassuringly, this is similar to our main results reported in Column (1) of Table 1, which found a 6.8 percentage point effect for the country as a whole. Moreover, 87% of the estimated location-specific effects are positive, and 60% of these are statistically significant ($p < 0.05$). None of the effects are negative and statistically significant.

The x-axis in both panels of Figure 3 shows each location's baseline abortion rate. As predicted by the model, both the percent effects (Panel a) and the level effects (Panel b) are smaller in locations with higher baseline abortion rates. Table A.4 repeats the analysis using the alternative measure of location-level leniency—the share of lenient individuals—and finds similar results.

An alternative explanation is that locations with lower baseline abortion rates are simply lower–income areas, and that income—rather than attitudes—drives the larger subsidy effects. To assess this, Table A.5 reestimates the regressions underlying Figure 3 while controlling for average household income in each location. The coefficients on baseline abortion rate remain essentially unchanged, whereas the income coefficients are small and statistically insignificant.

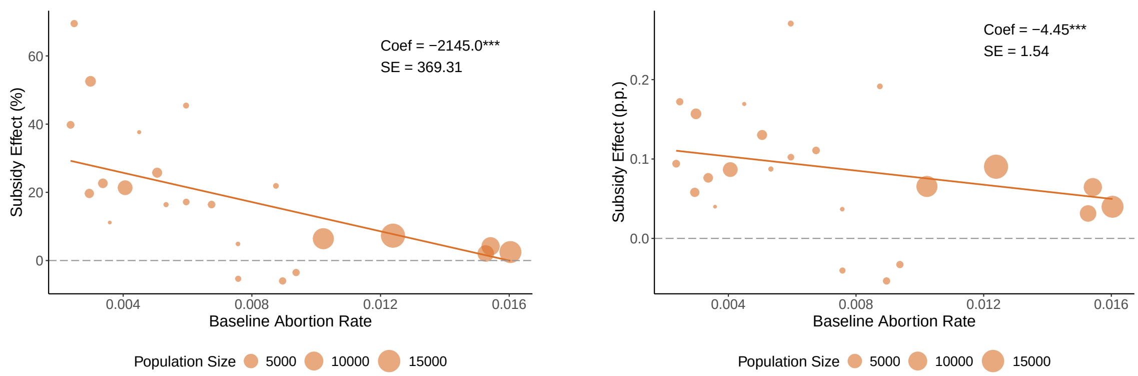
Taken together, these findings reinforce the model's central prediction: abortion subsi-

[19] We use the most granular location definition available in our data, where place of residence is defined by the combination of region, locality type, and ethno-religious group. Regions are North, South, Haifa, Tel Aviv, Center (excluding Tel Aviv), and Jerusalem. Locality type is urban, village, or kibbutz. Ethno-religious groups are either Jewish or Arab, based on the majority population in that locality. For example, "North–Urban–Arab" constitutes one location.

dies have the largest impact when abortion attitudes are more strict.

Figure 3: Subsidy Effects by Location-Level Baseline Abortion Rates

(a) Percent Effect (b) Level Effect



Notes: Each dot represents the estimated effect of the 2014 subsidy reform for a distinct location, obtained by re-estimating Equation (1) separately for each location l. Panel (a) plots the percent effect and Panel (b) plots the corresponding level effect. Baseline abortion rates are computed using pregnancies with conception dates before the 2014 subsidy expansion. All regressions include month fixed effects and the interaction of ethno-religious-group and education fixed effects; standard errors are clustered by age-by-year at conception. The sample includes all unmarried women aged 16–23 who conceived between 2009–2016. The fitted line in each panel represents the linear relationship between the estimated effect and the location characteristic, with corresponding coefficient estimates displayed in the upper right corner. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Extension to U.S. data

We now extend our analysis using data from the United States. This exercise serves two purposes. First, it shows that the mechanisms identified in our theoretical framework are not unique to the Israeli context. Second, it illustrates how these mechanisms can be studied in settings where individual-level data is unavailable.

First, we extend Myers (2017), who uses a difference-in-differences approach to estimate the effects of the introduction of the oral contraceptive pill, the legalization of abortion, and laws that grant young women confidential access to each of these (between 1960–1979) across U.S. states on teenage births. Myers finds that granting young women confidential access to abortion resulted in a 5.7 percentage point decrease in the probability of giving birth before age 19. These findings suggest that while legalizing abortion also resulted in reductions in teen births, the largest effects were due to confidential access for minors.

We rely on publicly available replication data and code and the classification of policy environments from Myers (2017) and use 1979-95 Current Population Survey (CPS) June Fertility Supplements from the original analysis along with the 1971 Survey of Churches and Church Membership in the United States (Glenmary Research Center, 1974) to classify abortion attitudes along religious lines. Specifically, we measure the strictness of abortion attitudes by the share of the population adhering to Christian denominations with the most conservative views on abortion, including the Church of Jesus Christ of Latter-Day Saints, the Church of the Nazarene, the Southern Baptist Convention, the Catholic Church, the Church of God, and the United Methodist Church. We classify areas as "strict" if this share is above the U.S. median and as "lenient" otherwise. We split the sample of states according to leniency and estimate the Myers (2017) difference-in-differences specification for giving birth before age 19 within each subgroup. These results are reported in the first two columns of Table 3. Consistent with our theory, we see the effect of removing parental consent laws is larger for states with a larger population of people with strict abortion attitudes ($p < 0.01$) and correspond to a 58.9% decrease relative to the baseline rate (compared to a 30.3% decrease in more lenient states).

Second, we extend the analysis of Joyce et al. (2020), who study the effect adding parental involvement laws for abortion access between 1985 to 2013 in the United States on state-level abortion incidence among minors. We use replication data and code provided by the authors[20] along with the 1980 Survey of Churches and Church Membership in the United States (Glenmary Research Center, 1982) to classify abortion attitudes along religious lines. We classify abortion attitudes in the same way as described above and similarly estimate the Joyce et al. (2020) specification for abortion incidence among 15 to 17 year-olds separately within the strict and lenient states (shown in the second two columns of Table 3). Here we see a small and statistically insignificant effect of introducing parental involvement laws in more lenient states, validating our theory that when parents and children have similar attitudes toward abortion, a change in parental involvement laws does not change behavior. In contrast in states with larger strict populations, we see a large,

[20]Specifically we use the authors' data on CDC Occurrence of abortion for 15-17 year-olds and replicate Model C from Table 2. See (Joyce et al., 2020) for more details on the various data sources and empirical specifications.

Table 3: U.S. Evidence on Heterogeneous Autonomy Effects by Abortion Attitudes

	Myers (2017)		Joyce et al. (2021)	
	Lenient	Strict	Lenient	Strict
Treatment Effect	-0.046***	-0.089***	-0.673	-3.526***
	(0.010)	(0.011)	(0.841)	(0.547)
Baseline Mean	0.152	0.151	14.100	10.100
P-value	0.004		0.004	
N	152,279	124,233	514	600

Notes: This table reports extensions of our analysis to two U.S. policy settings. Columns (1) and (2) replicate the Myers (2017) difference-in-differences estimates of the effects of expanding confidential access to abortion for minors on the probability of giving birth before age 19, using 1979–1995 CPS June Fertility Supplements. States are split into "strict" and "lenient" groups based on pre-policy abortion attitudes, proxied by the share of residents affiliated with Christian denominations holding conservative views on abortion (1971 Survey of Churches and Church Membership). Columns (3) and (4) replicate the Joyce et al. (2020) difference-in-differences estimates of the effects of introducing parental involvement laws on state-level abortion incidence among 15–17 year-olds, using CDC occurrence data and the authors' replication files. States are classified into strict and lenient groups using the same religious-attitudes measure (1980 Survey of Churches and Church Membership). All specifications follow the original authors' empirical models. Robust standard errors are reported in parentheses. P-values correspond to tests of statistical differences between Columns (1) and (2), and between Columns (3) and (4). $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

negative, and statistically significant effect on parental involvement laws on teen abortion incidence ($p < 0.01$ compared to lenient states), equivalent to a 34.9% decrease relative to baseline incidence.

In sum, these two extensions to U.S. policies show that increasing autonomy in abortion decisions has a larger impact in more religious communities. Combined with the results in Table 2, which document greater intergenerational mismatch in these communities, the evidence further supports the view that intergenerational mismatch plays a central role in shaping the behavioral effects of increased abortion autonomy.

5.3 Policy Design

We now turn to the implications of the model for policy design. The goal of this section is not to address the normative question of whether young women should have access to abortion. Instead, we take as given a previous policy decision to ensuring access for women above a certain age who seek it. Conditional on this objective, our aim is to characterize how access can be provided in the most cost-efficient manner. We summarize the main results here and provide full details in Appendix G.2.

Specifically, we consider how a planner might optimally allocate government resources across locations subject to a budget constraint. We focus on location-level targeting because this is how resources are often allocated in practice.

We consider two alternative cost structures. In the first, the cost of implementing the policy in a location is proportional to the number of abortions that occur after the policy is introduced. This setting captures cases where the policy takes the form of a subsidy that reimburses abortion procedures at a fixed rate per case, as in our empirical context (Section 2). In the second, costs are proportional to the size of the local population, regardless of how many abortions take place. This structure may be more relevant for direct-service interventions, such as the construction and operation of abortion clinics.

When implementation costs are proportional to the number of abortions, the policy is most efficient when targeted toward locations where the *percent increase* in abortions is greatest. As shown in Section 5.2—both theoretically and empirically—the subsidy's percent effect is larger in locations with lower baseline abortion rates. Thus, under this cost structure, it is optimal to target locations with lower baseline abortion rates.

A similar logic applies when costs are proportional to population. In this case, the most efficient allocation targets locations with the largest *change in levels* rather than in percentage terms. We find that the same policy rule holds in this case: targeting locations with lower baseline abortion rates remains optimal. However, as in Proposition 2, this conclusion depends on the assumption that intergenerational disagreement over abortion attitudes is greater in stricter communities. We have shown in Section 4.3 that this holds in both Israel and the U.S., but it may not hold universally. Hence, our cost-efficiency result applies more generally when costs depend on take-up than when they do not.

These results have direct practical relevance for the design and targeting of abortion access policies. Policymakers and NGOs must allocate limited resources and decide where to deploy abortion-related services—for example, determining where to establish clinics. Although these decisions are shaped by political, legal, logistical, and budget considerations, prioritizing areas with the greatest need is a central element of the process, as often emphasized in reports by both *International Planned Parenthood Federation (IPPF)* and *Marie*

Stopes International (MSI).[21] Identifying high-need areas, however, is often difficult in practice. Our results offer a systematic way to do so: locations with lower baseline abortion rates tend to have higher unmet demand and exhibit the largest behavioral responses to subsidies. This insight is policy-relevant, as areas with low utilization are often the first to face funding cuts when budgets tighten (Mavodza et al., 2019; Planned Parenthood, 2025). Our findings suggest that such cuts may inadvertently reduce access precisely where potential effects are greatest.

6 Conclusion

This paper examines how government subsidies shape abortion decisions using administrative data covering all legal abortions and births in Israel linked to rich demographic and socioeconomic information. We find that a subsidy—that fully covers the cost—substantially increases abortion, with much larger responses among young women from communities with strict abortion attitudes. To explain this pattern, we develop a simple model in which subsidies relax a key social friction—dependence on parental financial support—thereby increasing young women's autonomy. The model predicts that autonomy-enhancing policies matter only when daughters and parents disagree about abortion. We provide direct evidence for this mechanism using survey data. We also show that the model's predictions extend beyond Israel: in the United States, parental consent laws have much larger effects in more conservative communities.

Our analysis has important policy implications. First, we show that policies that reduce monetary barriers increase the autonomy of pregnant young women, much as parental consent laws reduce it. This reframes debates over public funding of abortion for young women. The key issue is not a standard welfare calculation—comparing consumer surplus and firm profits to the fiscal cost—but rather a question of authority: who should decide whether a pregnancy continues—the pregnant young woman or her parents? Second, for governments

[21]MSI and IPPF are two of the largest international organizations providing abortion and other reproductive health services. They operate in dozens of countries and have served millions of individuals worldwide. See IPPF (2021), "Abortion and Unsafe Abortion: A Global Health Crisis," retrieved from www.ippf.org; and MSI (2022), *Annual Report 2022: Safe Abortion Services and Advocacy*, retrieved from www.mariestopes.org.

and NGOs that must allocate limited resources for abortion services, our results suggest they should prioritize locations with the lowest abortion ratios among young women. According to our model, these areas have the highest unmet demand. This insight has practical consequences, as regions with low utilization are often the first to face budget cuts.

Several avenues for future research remain. This paper focuses on the immediate effects of abortion subsidies on abortion, but because the reform occurred in 2014, long-term consequences—including marriage, education, and labor-market trajectories—have now had time to unfold. Extending the data to more recent years would allow researchers to study the downstream effects of abortion policy. In addition, our theoretical framework abstracts from the role of male partners because comprehensive data on the men who father pregnancies that end in abortion is not available. Understanding, both empirically and theoretically, how partners or other family members influence abortion decisions would be valuable. Finally, although we provide initial evidence that the mechanisms highlighted here extend beyond Israel, further work is needed to understand how social mechanisms shape the effects of reproductive-health financial aid in other settings.

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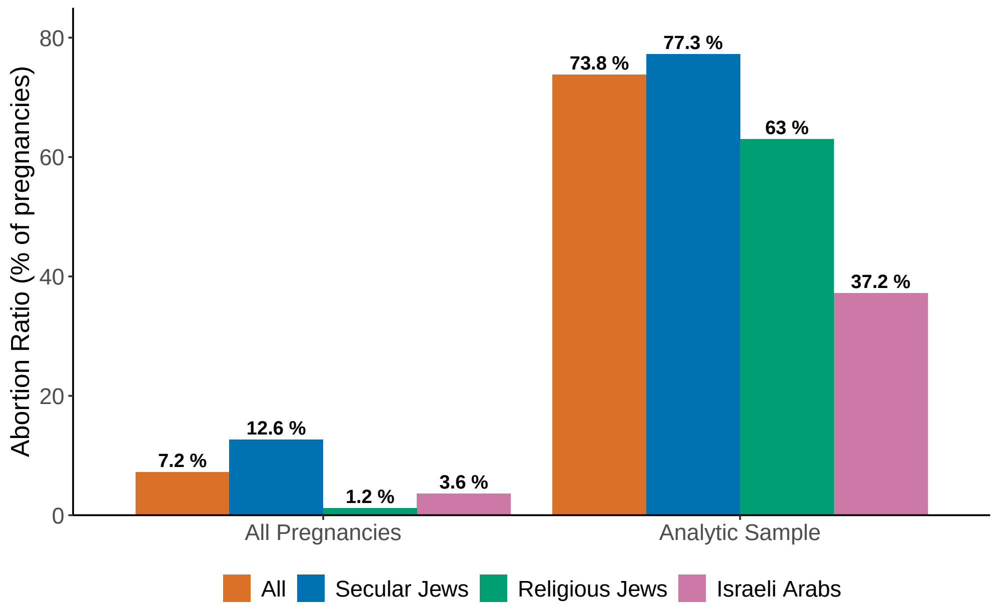
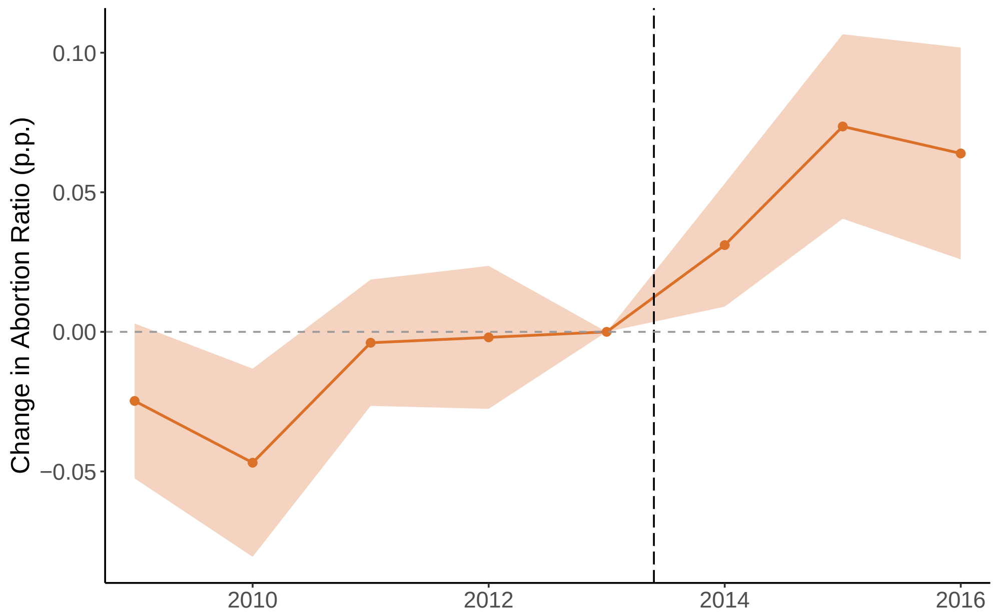
A Additional Figures and Tables

Figure A.1: Abortion Ratios in Israel by Ethno-Religious Group



Notes: The figure reports abortion ratios (the percentage of pregnancies that end in abortion) for two populations: all women aged 16–45 who conceived between 2009–2013 (left panel), and the analytic sample—defined in Section 3.1—of unmarried women aged 16–23 who conceived for the first time during the same period (right panel). Within each population, observations are grouped by ethno-religious category: secular Jews, religious Jews, Ultra-Orthodox Jews, and Israeli Arabs.

Figure A.2: Full Sample Dynamic Difference-in-Differences Estimates



Notes: The figure plots the dynamic difference-in-differences coefficients from Equation (2), which measure changes in abortion ratios between treated women (ages 20–23) and control women (ages 16–19) over 2009–2016. Estimates are pooled across all communities, without separating locations by abortion attitudes. Each point corresponds to a coefficient δ_k; 2013 is the omitted year. The vertical dashed line marks the 2014 subsidy reform. The sample includes all unmarried women aged 16–23 who conceived for the first time between 2009–2016. All regressions include month fixed effects and the interaction of ethno-religious-group and education fixed effects. Standard errors are clustered by age-by-year at conception, and shaded regions depict 95% confidence intervals.

Table A.1: Descriptive Statistics by Socioeconomic Status

Variable	Low SES	High SES
Daughter's Income (Shekels)	15,652	18,121
HH Income (Shekels)	60,224	280,643
Father's Income (Shekels)	50,283	190,467
Father's Months Worked	9	11
College (share)	0.08	0.14
Religious Jew (share)	0.08	0.06
Arab (share)	0.10	0.02

Notes: This table reports baseline characteristics for women from low- and high-SES families. Socioeconomic status is defined based on whether household earnings fall below or above the median in our sample. Reported variables include the daughter's income, combined parental income, the father's income, the father's months worked in the past year, an indicator for college graduation, and indicators for religious Jewish and Arab background. All earnings measures correspond to the year of conception.

Table A.2: Subsidy Effect on Abortion Utilization By Ethno-Religious Group

	Lenient	All Strict	All Strict Jews	Religious Jews	Ultra-Orthodox	Arabs
Treatment Effect	0.056***	0.148***	0.150***	0.131***	0.164*	0.129***
	(0.010)	(0.027)	(0.038)	(0.041)	(0.090)	(0.036)
P-value (vs. lenient)			0.018	0.078	0.232	0.052
P-value (vs. all strict)			0.961	0.729	0.863	0.671
Baseline Mean	0.773	0.509	0.630	0.705	0.453	0.372
N	35,231	5,264	2,802	1,966	836	2,462

Notes: This table reports OLS estimates of Equation 1, which identifies the effect of the 2014 subsidy reform on abortion ratios for women aged 16–23 who became pregnant for the first time between 2009–2016. Column (1) reports estimates for women from lenient backgrounds. Column (2) reports estimates for all women from strict communities (religious Jews, Ultra-Orthodox Jews, and Israeli Arabs). Columns (3)–(6) further disaggregate strict communities into: religious and Ultra-Orthodox Jews (Column 3), religious Jews only (Column 4), Ultra-Orthodox Jews only (Column 5), and Israeli Arabs (Column 6). All regressions include month fixed effects and the interaction of ethno-religious-group and education fixed effects. Standard errors, clustered by age-by-year at conception, are reported in parentheses. Reported p-values test differences in treatment effects between each subgroup and either Column (1) (lenient) or Column (2) (all strict), as indicated. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table A.3: Effect of the 2014 Reform by the Interaction of SES and Abortion Attitudes

	Lenient		Strict	
	Low SES	High SES	Low SES	High SES
Treatment Effect	0.058***	0.052***	0.141***	0.142***
	(0.014)	(0.014)	(0.033)	(0.047)
P-value (SES)	0.77		0.984	
P-value (Attitudes)			0.02	0.067
Baseline Mean	0.704	0.844	0.442	0.681
N	17,783	15,543	3,775	1,487

Notes: This table reports OLS estimates of Equation 1, which identifies the effect of the 2014 subsidy reform on abortion ratios, separately by the interaction of socioeconomic status (SES) and abortion attitudes. Columns (1) and (2) present estimates for women from lenient-attitude communities who are low-SES and high-SES, respectively; Columns (3) and (4) report the corresponding estimates for women from strict-attitude communities. Two sets of p-values are reported. *P-value (SES)* tests whether treatment effects differ between low- and high-SES women within each attitude group. *P-value (Attitudes)* tests whether treatment effects differ between strict- and lenient-background women within each SES group. All regressions include month fixed effects and the interaction of ethno-religious-group and education fixed effects. Standard errors, clustered by age-by-year at conception, are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table A.4: Subsidy Effect Heterogeneity by Location Baseline Characteristics

	Baseline Abortion Rate		Baseline Lenient Share	
	Level Effect	Percent Effect	Level Effect	Percent Effect
Coefficient	-4.45***	-2144.99***	-0.07*	-50.45***
	(1.54)	(369.31)	(0.04)	(9.06)
Baseline Mean	0.08	18.69	0.08	18.69
N	23	23	23	23

Notes: Each column reports results from a regression of the location-specific treatment effect from Equation (1) on a measure of baseline abortion attitudes. "Level Effect" refers to the estimated impact of the subsidy in percentage points, and "Percent Effect" scales this impact by the location's pre-reform abortion rate. The first two columns use the baseline abortion rate in each location as the explanatory variable; the last two columns use the share of lenient individuals in the location. Each observation corresponds to one location, and all regressions weight locations by the number of pre-reform pregnancies. Standard errors are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table A.5: Subsidy Effect Heterogeneity by Baseline Abortion Rate Controlling for Income

	Level Effect		Percent Effect	
	Income: No	Income: Yes	Income: No	Income: Yes
Baseline Abortion Rate	-4.45**	-4.83**	-2,145.0***	-1,899.6***
	(1.54)	(1.63)	(369.3)	(351.2)
Avg. HH Income		1.26e-7		-8.08e-5*
		(1.6e-7)		(3.45e-5)
P-value	0.864		0.63	
N	23	23	23	23

Notes: Each column reports estimates from a regression of the location-specific treatment effect from Equation (1) on the location's baseline abortion rate. "Level Effect" refers to the subsidy's impact in percentage points, and "Percent Effect" scales this impact by the location's pre-reform abortion rate. Columns (2) and (4) additionally control for the average household income in the location. Each observation is a distinct location, and regressions are weighted by the number of pre-reform pregnancies. Standard errors are reported in parentheses. Reported p-values test differences in the treatment effect with and without controlling for income. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

B Israeli Context

B.1 Abortion and Contraceptive Use: Norms and Prevalence

Prevalence of Abortion. Abortion is common in Israel despite the committee approval requirement. In the pre-policy period (2009-2013) Abortions accounted for about 10% of pregnancies. Although this may appear high, Israel's legal abortion ratio is relatively low compared to international benchmarks (Figure B.1). Globally, 25% of pregnancies end in abortion; in Europe the rate is 26%, and in North America 16% (Guttmacher, 2018).

Public Discourse. Public engagement with abortion policy in Israel is limited compared to the United States. A Google Trends comparison (Figure B.2) shows that search activity for the term "abortion" (*hapala*) in Hebrew is consistently lower and less reactive to events than in the U.S. The modest rise in 2014 coincides with the implementation of the cost-elimination reform but remains muted relative to U.S. peaks, such as during President Trump's election in 2016 or the Supreme Court confirmation hearings for Judge Kavanaugh. This pattern underscores the low salience of abortion in Israeli public debate.

Political Salience. Abortion is not as polarizing political issue in Israel and is often described as a "silenced phenomenon" (Amir, 2015). Liberal parties occasionally criticize the committee system but avoid pursuing reform, fearing that renewed debate could lead to more restrictive laws (Oberman, 2020; Rimaltt, 2017). Two legislative attempts to expand or abolish the committee process—in 2004 and 2006—did not pass.Religious parties have likewise failed to tighten restrictions. Bills introduced in 2008 and 2017 sought to prohibit late-term abortions or add a religious representative to the committee but did not gain sufficient support.

Cultural and Religious Attitudes. Israel's population is religiously diverse: 75% Jewish, 18.6% Muslim, 2% Christian, and 4.4% other or unaffiliated. As shown in Figure A.1, abortion ratios vary widely across groups, reflecting heterogeneity in social norms and perceived moral costs. These differences are central to understanding variation in young women's reproductive autonomy.

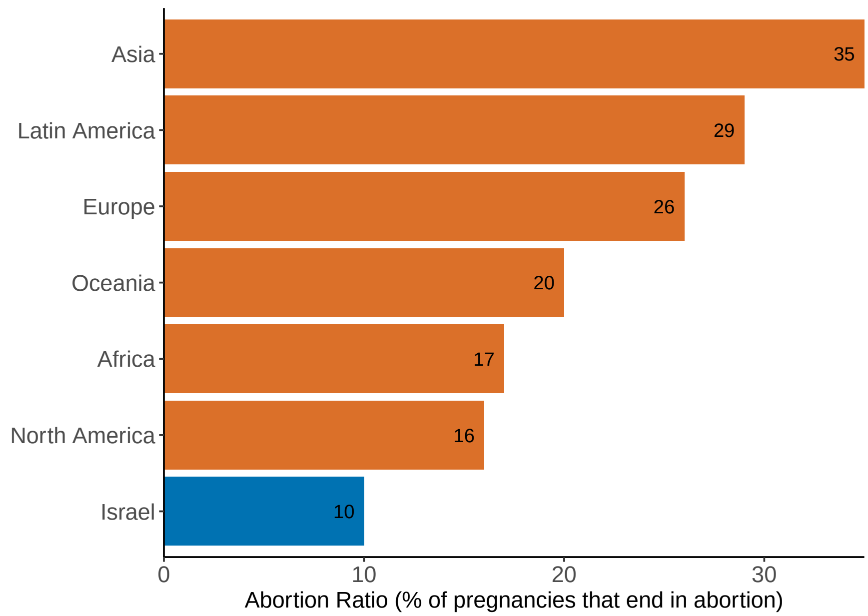
Judaism generally holds more permissive views on abortion than Christianity or Islam, prioritizing the mother's life and health (Amir, 2015). Abortions are permitted in two broad cases: (i) risk to the woman's life, or (ii) if the child would be born into an "unstable life," which may include cases such as unmarried parents, very young or old mothers, or severe congenital disorders. Despite this relative permissiveness, Israel's "demographic project" aims to preserve high Jewish fertility, creating a tension between religious interpretation and policy practice.

Religiosity within the Jewish population is strongly correlated with marital patterns, fertility, contraceptive use, and abortion attitudes. Secular Jews (about 45% of the population) generally support abortion, have high contraceptive use rates, and low fertility. In contrast, Orthodox and Ultra-Orthodox Jews (about 30% combined) strongly oppose abortion, use contraception rarely (Figure B.3), and have high fertility. Marriage and childbearing also occur earlier among religious women: about half of them are married by the age of 23, and four out of five of those who conceived were married.

Muslim and Arab Populations. The Israeli-Arab population, predominantly Muslim, views abortion as highly taboo. Among Muslims, 11% identify as secular, 57% as traditional, and 31% as religious (Central Bureau of Statistics (Israel), 2018). Islam generally prohibits abortion except to save the mother's life, which may lead some women to seek abortions outside the legal system. Reliable data on illegal or self-induced abortions in Israel are scarce, but evidence from neighboring populations offers context. A 2006 survey of Palestinian women found that 10% had self-induced an abortion, and that 25% of respondents believed self-induced abortions are necessary for unmarried women to avoid honor-based violence (Foster et al., 2007).[22] These findings highlight the strong social stigma surrounding abortion in Arab communities.

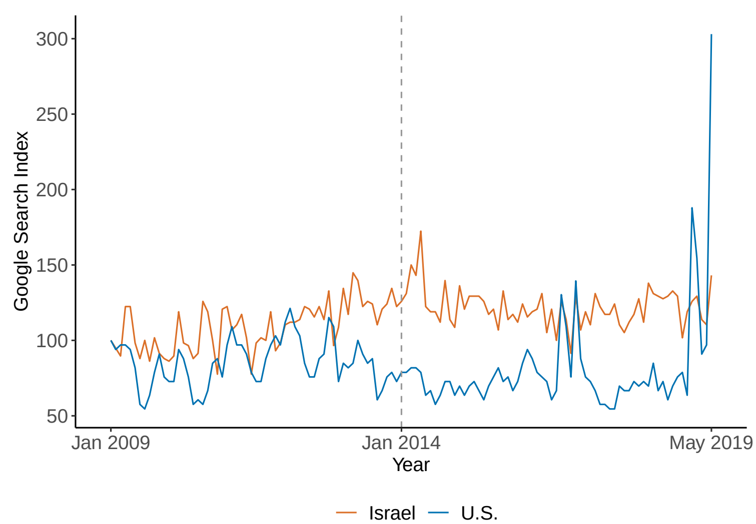
[22] "Honor killings" refer to the practice of family members harming women accused of bringing dishonor to the family through premarital or extramarital sex.

Figure B.1: Abortion Ratios Worldwide



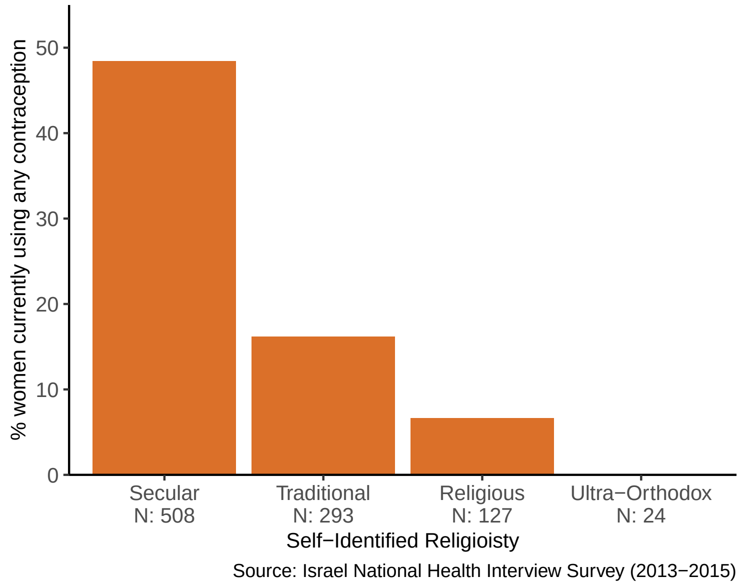
Notes: The figure reports abortion ratios (abortions per pregnancies) among women aged 15–49 by region. Global data: Guttmacher Institute (https://data.guttmacher.org/); Israeli data: Central Bureau of Statistics.

Figure B.2: Google Searches for "Abortion" (Israel vs. U.S.)



Notes: The figure compares search intensity for "abortion" in the U.S. and its Hebrew equivalent (*hapala*) in Israel, 2009–2019. Search levels are normalized to January 2009.

Figure B.3: Self-Reported Contraceptive Use by Religiosity



Notes: The figure reports self-reported contraceptive use by religiosity, using the Israel National Health Interview Survey (2013–2015) (Einav et al., 2017). The data include Jewish and Muslim women by self-identified religiosity. No Ultra-Orthodox women in the survey reported using contraception.

B.2 Abortion Committee

Israel's abortion committee system is exceptional in the global context and reflects the country's explicit demographic agenda. Officially, the committee process was introduced due to medical concerns that abortion might harm women's future fertility (Amir, 2015). This concern aligned with Israel's broader *demographic project*—an effort to reverse the decline of the global Jewish population following the Holocaust. In pursuit of this project, Israel has adopted extensive pro-natalist policies, including subsidized daycare, monthly child allowances, paid parental leave, and public coverage of infertility treatments. In contrast, contraception is not covered by national health insurance, and abortions are illegal without prior approval from an abortion committee. Consistent with this policy framework, Israel maintains the highest fertility rate among OECD countries.

When a woman seeks an abortion, her doctor refers her to one of 42 abortion committees operating across hospitals and clinics nationwide (See the full list of committees here). Each committee comprises two medical professionals and a social worker, at least one of whom must be a woman. All abortions, including those performed by private doctors, require committee authorization.

The committee grants approval if *at least one* of the following criteria is met (see rates in Table B.1a):

1. The woman is under 18 or over 40 years old;
2. The pregnancy occurred outside marriage;
3. The pregnancy resulted from rape or incest;
4. The pregnancy endangers the woman's life or health (including mental health); or
5. The fetus has a congenital disorder.

These criteria are grounded in Jewish law, which prioritizes the mother's life and health and considers the fetus part of her body until birth. Jewish law also views children born out of wedlock as socially disadvantaged, providing religious justification for abortion in such cases (Amir, 2015).

At the committee, women complete paperwork and pay a 400 NIS ($112 in 2014 rates) fee, which was also eliminated by the 2014 policy. They first meet with the social worker, who evaluates eligibility and provides counseling. In practice, social workers act as the effective gatekeepers, while committees generally serve as a formality (Oberman, 2020). Only married women aged 18–40 with healthy pregnancies are formally ineligible. In these cases, social workers often help women secure psychiatric documentation stating emotional distress or adjustment difficulties, which qualifies under the "risk to woman's health" criterion (Oberman, 2020).[23]

Although the system appears restrictive, nearly all applications are approved. In our data, 99% of applications were approved and 97% acted upon. The process is confidential, and neither partner nor parental consent is required. Late-term cases (beyond 24 weeks, about 1% of abortions) are reviewed by a special committee with stricter standards, which we exclude from analysis. High approval rates likely reflect the social workers' gatekeeping

[23] Alternatively, some women report medication use that could endanger the fetus.

role, rather than leniency in policy. Alternative explanations—such as cross-border "abortion tourism"—are implausible given Israel's geopolitical isolation and the more restrictive abortion laws in neighboring countries.

To reduce financial barriers, the government progressively expanded abortion subsidies: first in 2001 to women under 18, then in 2008 to those under 19, and in 2014 to women aged 20–32 under any approval criterion (see the 2014 change in Table B.1b).

B.3 Childrearing Cost in Israel

The cost of childrearing in Israel is substantially lower than in most other high-income countries, particularly the United States. Education and healthcare are publicly funded: healthcare coverage for children costs approximately $3 per month, and pre-kindergarten is free for children aged three and older. Even below age three, public or subsidized daycare options are widely available and typically cost less than a full-time minimum wage salary.[24]

As part of its pro-natalist demographic agenda, the government provides several direct and indirect financial supports. These include a one-time birth grant (USD 145–484 depending on birth order), a monthly child allowance through social security (USD 41–52 per child), and a tax credit for working parents (USD 118 per month per child). Together, these transfers substantially reduce the monetary cost of raising children, especially for low- and middle-income households.

Parental leave policies further mitigate the opportunity cost of childbearing. By law, mothers are entitled to 14 weeks of paid maternity leave, fully financed by the government, with the option to extend for an additional 12 weeks unpaid. During this period, employers may not terminate the parent's employment. Fathers are formally eligible to share the leave, though uptake remains rare. Some employers voluntarily grant extended unpaid leave beyond the statutory period, but they are not required to do so.

Israel's small geographic size and strong intergenerational family networks also facilitate mothers' reentry into the labor market. Many parents rely on grandparents or extended family for childcare during the child's first year, before entry into the public education sys-

[24] Israel's minimum full-time monthly wage in 2014 was NIS 4,300 (USD 1,200).

Table B.1: Eligibility Criteria for Abortions and Subsidies

(a) Abortion Eligibility Criteria and Pre-2014 Subsidies

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

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

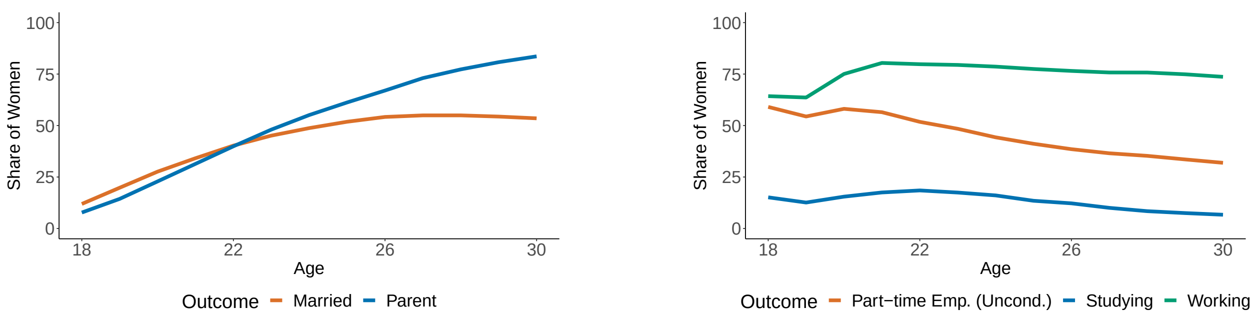
(b) 2014 Change in Abortion Subsidy (Identification Strategy)

Age	**Free?**	
	Pre-2014	**Post-2014**
Age ≤ 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 aged 19 and under were already fully subsidized by the government and therefore unaffected by the change and women age 33 and older were not included in the subsidy expansion and thus never treated. This change in funding applies to women aged 20-32 regardless of what criteria their abortion was approved under, but as can be seen in Table B.1a, of the potential criteria that apply to women aged 20-32, the out-of-marriage criterion is the only one not eligible for a subsidy prior to 2014.

tem. These patterns are particularly pronounced among religious families, where women typically marry and have children at younger ages and combine employment with childrearing (Figure B.4).

Figure B.4: Life-Cycle Decisions of Religious Women



(a) Family formation

(b) Employment and education

Notes: The figure shows the timing of four major life events for religious Jewish women in Israel—marriage, parenthood, employment, and higher education—by age group.

C Sample Restrictions

Unmarried Women. We restrict the analysis to unmarried women for both conceptual and empirical reasons. First, pregnancies outside marriage are automatically approved for legal abortion (Table B.1a), making abortions and births more comparable. Including married women would introduce selection bias because they obtain approval only under specific circumstances (e.g., health risk or infidelity) and are far less likely to terminate a pregnancy. Second, the 2014 policy change affected only pregnancies outside marriage, since funding coverage for married women did not change. Marital status is measured at the month of conception.

First Pregnancies. To avoid endogeneity in sample construction, we restrict the analysis to each woman's first observed pregnancy. This approach ensures that treatment assignment is not affected by previous fertility or abortion decisions, which could themselves depend on earlier exposure to the policy.

Age Range. Our primary sample includes unmarried women aged 16–23 at the time of conception. This range captures those most likely to be directly affected by the 2014 policy, which expanded full funding for abortions from age 19 to 32. In robustness analyses, we focus on a narrower 18–21 age band to align more closely with the lower age cutoff and maximize comparability around the policy threshold. Younger women are also the most relevant group for testing our model's predictions, as they face higher financial and social constraints on abortion access.

A potential concern is that women aged 18–19 are often serving in the military, while those aged 20–21 have typically completed service (Sade, 2023). Because military service might influence fertility decisions, we conduct robustness checks excluding women aged 20 and re-defining the treated group as 21–22; results are unchanged.

Time Period. We restrict the sample to conceptions between January 2009 and March 2016. Starting in 2009 avoids contamination from an earlier policy change that expanded abortion funding to women aged 19, since before then coverage varied by military status.

D Subsidies' Effect on Abortion Decisions: Robustness

D.1 Changes in Sample Composition

Treatment and comparison groups may have undergone differential compositional changes around the time of the reform. To assess this possibility, we estimate the following simple difference-in-differences specification:

$$outcome_{it} = \beta\,(Post_t \times T_i) + \gamma_{a_i} + \gamma_{y_t} + \epsilon_{it}\ , \tag{9}$$

where the outcome is a characteristic of woman i measured at the time of conception. Equation (9) is analogous to our main specification (Equation 1), but excludes controls to focus on compositional changes.

Results are presented in Table D.2. Panel A reports estimates for all unmarried pregnant women aged 16–23, while Panels B and C show results separately for women from strict and lenient backgrounds. The estimates indicate that treatment and control groups are balanced across most observable characteristics, with the exception of education.

To assess whether this imbalance affects our main estimates, we re-estimate Equation (1)—our primary specification for identifying the effects of the subsidy—under alternative sets of controls and examine the sensitivity of the results. Table D.3 reports results for the full analytic sample, while Tables D.4 and D.5 present results for the lenient and strict subsamples, respectively. Column (1) presents estimates without controls. Columns (2)–(4) sequentially add each component of our baseline control set, while Column (5) includes all baseline controls jointly. Columns (6)–(8) introduce additional controls, including location fixed effects, interactions between ethno-religious group and age or year, an indicator for high SES, and an indicator for employment status. Column (9) reports an additional robustness check using a triple-differences design that exploits married women—who were unaffected by the reform—as a control group.

Across all specifications, the estimated effects remain stable and statistically indistinguishable from the baseline (see p-values in Tables D.3–D.5). These findings strengthen our confidence that the results are not driven by compositional changes across cohorts. To be conservative, we adopt the specification with education-ethno-religious-group fixed effects as our preferred specification throughout the paper.

D.2 Accounting for Pre-Trends

The estimates reported in Table 1 rely on the standard parallel-trends assumption between treatment and control groups. However, Figure 2 suggests a modest differential pre-trend for the lenient population. For the strict population, although pre-trends are not apparent, the relatively large standard errors prevent us from fully ruling out their presence.

To account for possible deviations from parallel trends, we apply the methodology of Rambachan and Roth (2023), which provides bounds on treatment effects that account for differential trends estimated from pre-treatment data. Their approach incorporates statistical uncertainty in the estimation of those trends. The resulting bounds are reported in brackets in Table 1.

For the lenient population, the bounds include zero, reinforcing that we cannot statistically distinguish between no effect and a small positive effect. In contrast, for the strict population, the bounds confirm a robust positive effect even when allowing for potential pre-trends. Moreover, the two sets of bounds do not overlap, providing further evidence that the subsidy's impact was significantly larger for the strict population.

As an additional robustness check, we relax the assumption that pre- and post-treatment trends evolve at the same rate by allowing the slope of the differential trend to vary by up to 4% before and after the reform, following the range proposed by Rambachan and Roth (2023). The results, shown in Figure D.1, again confirm our main conclusion: among women from strict backgrounds, we can reject the null of no effect across all specifications within the considered range.

D.3 Effect of Subsidy on Pregnancy

By reducing the cost of abortion, the subsidy also reduced the cost of becoming pregnant. This could, in principle, lower incentives for contraceptive use and thereby increase pregnancy rates. Such an effect would bias our estimates, since our main analysis conditions on being pregnant.

To examine this possibility, we re-estimate Equation (1) using all unmarried women aged 16–23 between 2009 and 2016, without conditioning on pregnancy. We consider two outcomes: (i) an indicator for whether a woman conceived in that year, and (ii) an indicator for whether she both conceived and obtained an abortion.

Results are reported in Table D.1. The estimates for pregnancy show no evidence that the subsidy increased conception rates—the coefficients are negative and statistically insignificant. The abortion estimates, while noisier due to the inclusion of non-pregnant women (who were unaffected by the policy), still reveal a statistically significant increase in abortions among women from strict backgrounds, consistent with our main findings. For women from lenient backgrounds, the effects are statistically indistinguishable from zero.

The null effect on pregnancy also alleviates concerns that the subsidy induced women who would otherwise obtain an illegal abortion to obtain a legal one. Legal abortions are observed in the administrative data and therefore appear as pregnancies, whereas illegal abortions are not recorded. If substantial substitution from illegal to legal abortions had occurred, we would expect to see an increase in observed pregnancies. We find no such increase.

D.4 Age bandwidth

In our main specification, women in the treatment group (ages 20–23) are older than those in the control group (ages 16–19). Although we include age fixed effects, differential age-specific trends could still bias our estimates. To assess this concern, Table D.6 presents estimates of Equation (1) using alternative age bandwidths. First, we restrict the sample to women aged 18–21 (instead of 16–23), ensuring that treated and control groups are more comparable in age. While standard errors increase slightly due to the smaller sample, point estimates remain stable.

Another potential concern is that 20-year-old women secular Jewish women are likely to still be in military service, which fully covers all healthcare including abortion. As a robustness, we estimate a specification using 18–19-year-olds as the control group and 21–

22-year-olds as the treatment group, excluding 20-year-olds. The results remain virtually unchanged. Overall, the estimates do not differ significantly across specifications, as shown by the p-values in Table D.6. Importantly, across all bandwidths, the conclusion that the policy's effect is substantially larger for women from strict backgrounds remains robust.

D.5 Selection into Marriage

Our main analysis focuses on unmarried women who conceived between 2009 and 2016. If the policy influenced marriage decisions, this restriction could introduce selection bias. To assess this possibility, Table D.7 reports estimates from Equation (1) without excluding married women.

In levels, coefficients appear larger for lenient-background women when married women are included. However, this difference primarily reflects disparities in baseline abortion ratios: among all strict-background women, only 3.2% of pregnancies end in abortion, compared to 31.3% among all lenient-background women. When expressing the treatment effects as percentages of baseline means, the results are consistent across specifications. For strict-background women, the effect equals 23.3% of the baseline when including all pregnancies and 29.1% when focusing on unmarried women; for lenient-background women, the corresponding figures are 4.4% and 7.2%. Moreover, the policy effect for the strict population ($p < 0.01$) and the difference in percent effects between strict and lenient populations ($p = 0.03$) are both statistically significant.

Overall, the results are consistent with those obtained from the unmarried-only sample (Table 1), indicating that selection into marriage is unlikely to drive our findings.

D.6 Former-USSR Migration

The pre-trends observed among women from lenient backgrounds may partly reflect demographic shifts associated with the large influx of migrants from the former USSR in the early 1990s. To assess this possibility, we exclude women whose families originated from the former USSR and re-estimate Equations (2) and (1). Figure D.2 compares estimated trends for all unmarried pregnant women from lenient backgrounds with those obtained after excluding this group. Removing the former-USSR-origin population attenuates the pre-trends but does not eliminate them entirely. Table D.8 shows that the estimated effect of the subsidy remains robust. Moreover, when accounting for pre-trends using the approach of Rambachan and Roth (2023) (bounds reported in brackets in Table D.8), our conclusion remains unchanged: the subsidy had little to no effect on abortion rates among women from lenient backgrounds.

D.7 Effects by Approval Reason

In Israel, abortions are legally approved if at least one of the following conditions is met: (1) the woman is under 18 or over 40; (2) the pregnancy occurred outside marriage; (3) the pregnancy resulted from rape or incest; (4) the pregnancy endangers the woman's life or health; or (5) the fetus has a congenital disorder (see Table B.1a). Before 2014, only women aged 19 or younger and those meeting medical or legal criteria were eligible for fully subsidized abortions. Therefore, the 2014 reform essentially extended eligibility to out-of-marriage abortions for women between the ages of 20 and 32. Because health-related abortions require a more stringent approval process, the reform likely induced some substitution from health-related to out-of-marriage approvals.

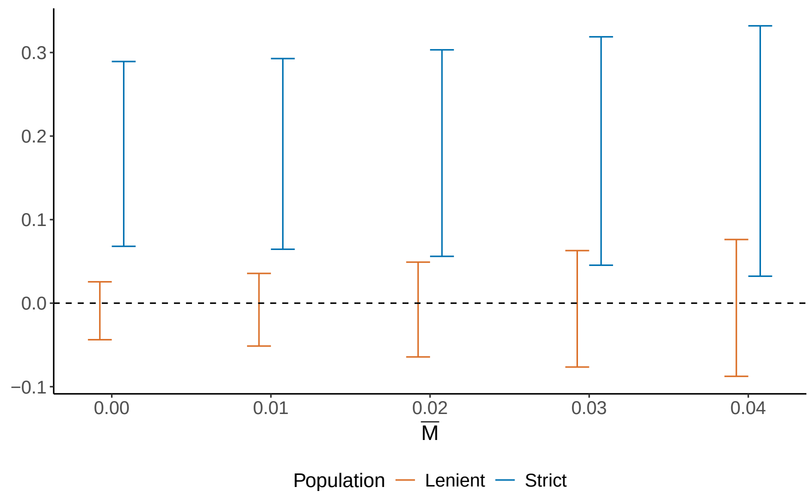
Figure D.3 confirms this pattern: a large rise in out-of-marriage approvals coincides with a decline in health-related ones. This evidence supports the interpretation that the overall increase in abortions after the reform was driven by the policy itself rather than by confounding trends.

Note that our main results do not distinguish between approval reasons. Thus, they capture the net increase in the overall abortion ratio, accounting for this reallocation across approval categories.

D.8 Taking Stock

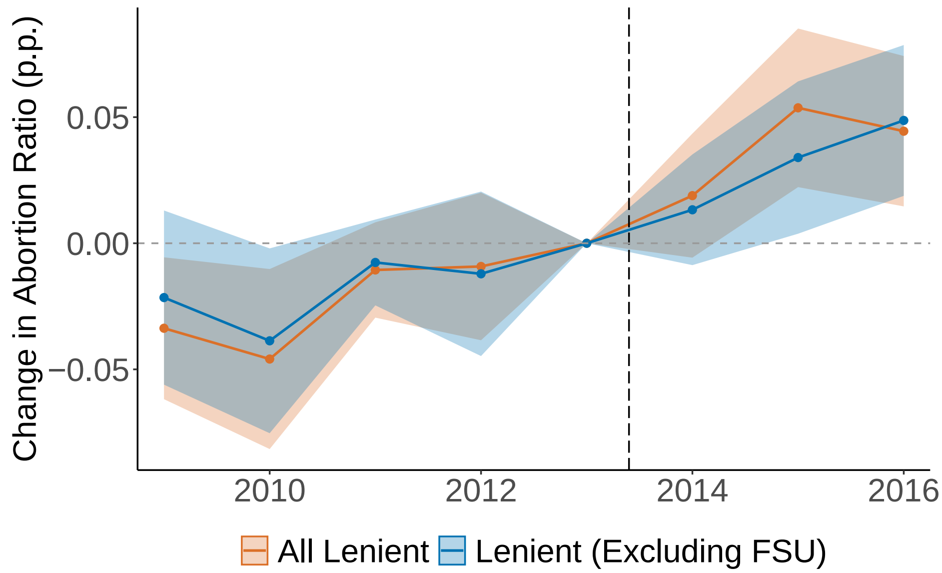
This appendix has presented a series of robustness analyses of our main findings. Taken together, the results reveal a consistent pattern: the subsidy had a large and statistically significant positive effect on abortion rates among women from strict backgrounds. For women from lenient backgrounds, the evidence is less consistent across specifications, and we cannot empirically distinguish between no effect and a small positive effect.

Figure D.1: Bounds on the Effect of the Subsidy on Abortion Utilization



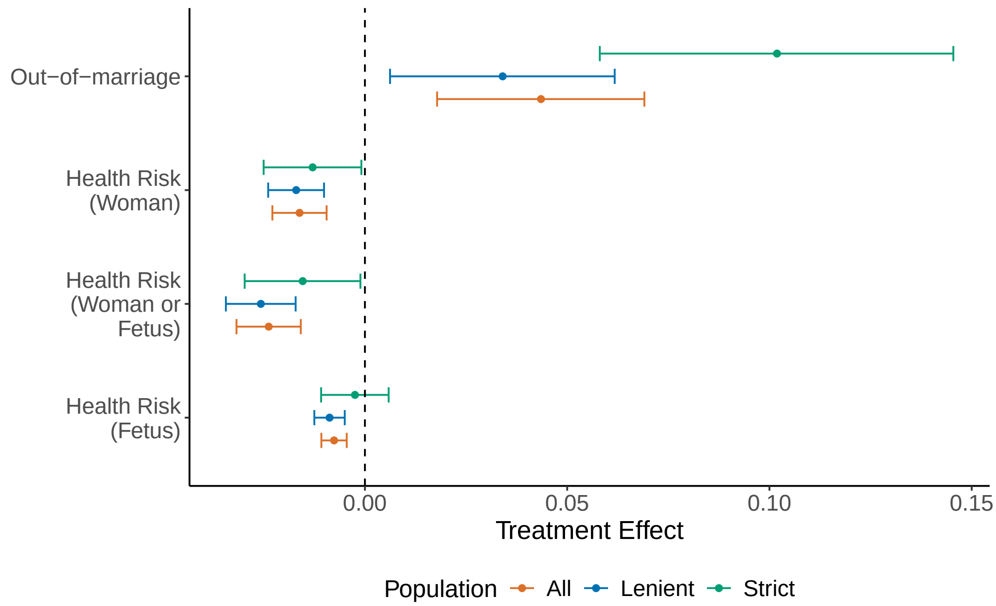
Notes: This figure displays bounds on the treatment effect of the 2014 subsidy reform constructed using the method of Rambachan and Roth (2023), which allows for potential differential pre-trends between treated and control groups. Bounds are shown separately for women from lenient and strict backgrounds. The x-axis reports the maximum allowed deviation of the post-treatment trend from the pre-treatment trend, up to 0.04 following Rambachan and Roth (2023), and the y-axis reports the corresponding treatment-effect bounds.

Figure D.2: Dynamic Difference-in-Differences Excluding Former–Soviet-Union Origin



Notes: The figure plots the dynamic difference-in-differences coefficients from Equation 2, which measure changes in abortion ratios between treated women (ages 20–23) and control women (ages 16–19) over 2009–2016. The sample consists of unmarried women aged 16–23 who conceived for the first time during this period. Estimates are shown for all women from lenient backgrounds and for the subsample excluding women from families originating in the Former Soviet Union. Each point corresponds to a coefficient δ_k; 2013 is the omitted year. The vertical dashed line marks the 2014 subsidy reform. All regressions include month fixed effects and the interaction of ethno-religious-group and education fixed effects. Standard errors are clustered by age-by-year at conception, and shaded regions depict 95% confidence intervals.

Figure D.3: Impact of the 2014 Subsidy Expansion on Abortion Approval by Reason



Notes: This figure reports OLS estimates of Equation 1 separately for each reason under which an abortion may be approved by the committee. The sample consists of unmarried women aged 16–23 who became pregnant for the first time between 2009–2016. Estimates are shown for three groups: all women, women from lenient-attitude families, and women from strict-attitude families. All regressions include month fixed effects and the interaction of ethno-religious-group and education fixed effects. Standard errors are clustered by age-by-year at conception.

Table D.1: Impact of the 2014 Subsidy Expansion on Abortion and Pregnancy

	Abortion			Pregnancy		
	Full Sample	Lenient	Strict	Full Sample	Lenient	Strict
Treatment Effect	-0.00064	-0.00890	0.00091*	-0.00335	-0.00479	-0.00459
	(0.00183)	(0.00693)	(0.00055)	(0.00329)	(0.00735)	(0.00297)
P-value (levels)		0.158			0.98	
P-value (%)		0.059			0.384	
Baseline Mean	0.02908	0.05417	0.00861	0.04673	0.07656	0.02238
N	1,268,430	570,014	698,416	1,268,430	570,014	698,416

Notes: This table reports OLS estimates of Equation 1 using all unmarried women aged 16–23 observed between 2009–2016. Outcomes are indicators for obtaining an abortion (left panel) and for becoming pregnant (right panel). Unlike the main analysis, which conditions on pregnancy, this table uses the population of all unmarried women; abortion and pregnancy outcomes therefore represent unconditional probabilities. For each outcome, Column (1) reports results for the full sample, and Columns (2) and (3) report estimates separately for women from lenient and strict backgrounds. P-values test whether the treatment effects differ between lenient and strict groups, with the first row comparing the level effects (percentage points) and the second row comparing percent effects (scaling by each group's baseline mean). All regressions include month fixed effects and the interaction of ethno-religious-group and education fixed effects. Standard errors, clustered by age-by-year at conception, are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table D.2: Testing for Composition Changes Around the 2014 Reform

Panel A: Full Sample (Unmarried, 16-23 year-olds)

	HH Income (log)	Own Income	Months Worked	College	Religious Jew	Arab
Treatment Effect	0.004	319.649	0.187	-0.052***	-0.009**	-0.003
	(0.026)	(310.634)	(0.204)	(0.010)	(0.005)	(0.006)
Baseline Mean	11.592	10798.972	11.193	0.135	0.068	0.065
N	32,619	37,148	37,148	40,495	41,371	41,371

Panel B: Strict Abortion Attitudes (Unmarried, 16-23 year-olds)

	HH Income (log)	Own Income	Months Worked	College
Treatment Effect	-0.136	154.942	-0.524	-0.066***
	(0.089)	(503.960)	(0.426)	(0.012)
Baseline Mean	11.237	10365.622	10.389	0.116
N	3,886	5,433	5,433	5,264

Panel C: Lenient Abortion Attitudes (Unmarried, 16-23 year-olds)

	HH Income (log)	Own Income	Months Worked	College
Treatment Effect	0.030	330.914	0.259	-0.052***
	(0.025)	(322.496)	(0.219)	(0.011)
Baseline Mean	11.640	10873.207	11.331	0.138
N	28,733	31,715	31,715	35,231

Notes: This table reports OLS estimates of Equation 9, which tests for changes in the composition of baseline characteristics around the 2014 subsidy reform. Outcomes are measured in the year prior to conception and include log household income, the woman's own income and months worked, indicators for college graduation, and indicators for being a religious Jew or an Israeli Arab. Panel A reports results for all unmarried women aged 16–23 who conceived for the first time between 2009–2016, while Panels B and C report results separately for women from strict and lenient backgrounds, respectively. All regressions include age and year fixed effects. Standard errors are clustered at the age–by–year at conception level and reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table D.3: Robustness of the Estimated Subsidy Effect to Sequentially Adding Controls (Full Sample)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment Effect	0.068***	0.069***	0.065***	0.071***	0.068***	0.062***	0.076***	0.076***	0.092***
	(0.014)	(0.014)	(0.012)	(0.011)	(0.011)	(0.010)	(0.011)	(0.012)	(0.031)
Baseline Mean	0.723	0.723	0.723	0.738	0.738	0.738	0.738	0.734	0.132
P-value		0.969	0.887	0.844	0.998	0.73	0.633	0.639	0.487
N	41,371	41,371	41,371	40,495	40,495	40,427	40,495	38,588	342,822
Month FE		X			X	X	X	X	X
Ethno-Relig FE			X		X	X	X	X	X
Education FE				X	X	X	X	X	X
Ethno-ReligXEducation FE					X	X	X	X	X
Location FE						X			
Ethno-ReligXAge FE							X	X	
Ethno-ReligXYear FE							X	X	
SES FE								X	
Employed FE								X	

Notes: This table reports OLS estimates of Equation 1 for all unmarried women aged 16–23 who conceived for the first time between 2009–2016. All specifications include age and year fixed effects, and columns sequentially add additional sets of controls, as indicated in the table. Column (9) reports estimates from a triple-differences specification that uses married women aged 16–23 as an unaffected comparison group. Standard errors are clustered at the age–by–year at conception level and reported in parentheses. P-values correspond to tests of equality between each column's estimate and that in Column (1). $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table D.4: Robustness of the Estimated Subsidy Effect to Sequentially Adding Controls (Lenient-Background)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment Effect	0.057***	0.057***	0.057***	0.057***	0.056***	0.052***	0.066***	0.067***	0.083***
	(0.013)	(0.013)	(0.013)	(0.010)	(0.010)	(0.010)	(0.011)	(0.012)	(0.032)
Baseline Mean	0.760	0.760	0.760	0.773	0.773	0.772	0.773	0.770	0.186
P-value		0.969	0.992	0.988	0.971	0.777	0.568	0.582	0.45
N	35,862	35,862	35,862	35,231	35,231	35,176	35,231	33,326	223,833
Month FE		X			X	X	X	X	X
Ethno-Relig FE			X		X	X	X	X	X
Education FE				X	X	X	X	X	X
Ethno-ReligXEducation FE					X	X	X	X	X
Location FE						X			
Ethno-ReligXAge FE							X	X	
Ethno-ReligXYear FE							X	X	
SES FE								X	
Employed FE								X	

Notes: This table reports OLS estimates of Equation 1 for lenient-background unmarried women aged 16–23 who conceived for the first time between 2009–2016. All specifications include age and year fixed effects, and columns sequentially add additional sets of controls, as indicated in the table. Column (9) reports estimates from a triple-differences specification that uses married women aged 16–23 as an unaffected comparison group. Standard errors are clustered at the age–by–year at conception level and reported in parentheses. P-values correspond to tests of equality between each column's estimate and that in Column (1). $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table D.5: Robustness of the Estimated Subsidy Effect to Sequentially Adding Controls (Strict-Background)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment Effect	0.134***	0.134***	0.135***	0.150***	0.148***	0.123***	0.142***	0.142***	0.191***
	(0.030)	(0.030)	(0.027)	(0.030)	(0.027)	(0.026)	(0.027)	(0.027)	(0.046)
Baseline Mean	0.487	0.487	0.487	0.509	0.509	0.509	0.509	0.509	0.030
P-value		0.995	0.975	0.69	0.721	0.787	0.832	0.836	0.293
N	5,509	5,509	5,509	5,264	5,264	5,251	5,264	5,262	118,989
Month FE		X			X	X	X	X	X
Ethno-Relig FE			X		X	X	X	X	X
Education FE				X	X	X	X	X	X
Ethno-ReligXEducation FE					X	X	X	X	X
Location FE						X			
Ethno-ReligXAge FE							X	X	
Ethno-ReligXYear FE							X	X	
SES FE								X	
Employed FE								X	

Notes: This table reports OLS estimates of Equation 1 for strict-background unmarried women aged 16–23 who conceived for the first time between 2009–2016. All specifications include age and year fixed effects, and columns sequentially add additional sets of controls, as indicated in the table. Column (9) reports estimates from a triple-differences specification that uses married women aged 16–23 as an unaffected comparison group. Standard errors are clustered at the age–by–year at conception level and reported in parentheses. P-values correspond to tests of equality between each column's estimate and that in Column (1). $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table D.6: Sensitivity of Estimated Subsidy Effects to Alternative Age Bandwidths

	All Women			Lenient			Strict		
	16-23	18-21	18-22	16-23	18-21	18-22	16-23	18-21	18-23
Treatment Effect	0.068***	0.053***	0.053***	0.056***	0.044***	0.044***	0.148***	0.110***	0.112***
	(0.011)	(0.013)	(0.012)	(0.010)	(0.012)	(0.011)	(0.027)	(0.032)	(0.031)
P-value		0.377	0.355		0.437	0.444		0.363	0.387
Baseline Mean	0.738	0.740	0.732	0.773	0.776	0.766	0.509	0.510	0.513
N	40,495	23,182	22,582	35,231	20,067	19,599	5,264	3,115	2,983

Notes: This table reports OLS estimates of Equation 1 using alternative age bandwidths for defining treated and control groups. Columns (1)–(3) report results for all unmarried women who became pregnant for the first time; Columns (4)–(6) and (7)–(9) report results separately for women from lenient and strict backgrounds, respectively. For each group, we estimate the model using three age ranges: ages 16–23 (our primary analytic bandwidth), ages 18–21, and ages 18–22 excluding age 20. P-values compare estimates from the primary bandwidth (16–23) with those obtained using the alternative bandwidths. All regressions include age-at-conception fixed effects, month fixed effects, and the interaction of ethno-religious-group and education fixed effects. Standard errors, clustered by age-by-year at conception, are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table D.7: Effect the Subsidy on Abortion Utilization (Including Married Women)

	Full Sample	Lenient	Strict
Treatment Effect	0.0057	0.0139	0.0074***
	(0.0050)	(0.0174)	(0.0022)
P-value (levels)		0.709	
P-value (%)		0.031	
Baseline Mean	0.1618	0.3126	0.0318
N	193,959	89,796	104,163

Notes: This table reports OLS estimates of Equation 1, which identifies the effect of the 2014 subsidy reform on abortion ratios for all women aged 16–23 who became pregnant for the first time between 2009–2016, including both married and unmarried women. Columns (2) and (3) report estimates separately for women from communities with lenient and strict attitudes toward abortion. The reported p-values test whether the treatment effects across attitude groups, with the first row comparing level effects (percentage points) and the second row comparing percent effects (scaling by each group's baseline abortion rate). All regressions include month fixed effects and the interaction of ethno-religious-group and education fixed effects. Standard errors, clustered by age-by-year at conception, are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

Table D.8: Effect of the Subsidy Excluding Former–Soviet-Union Origin

	All Lenient	Lenient (Excluding FSU)
Treatment Effect	0.056***	0.04***
	(0.01)	(0.01)
	[-0.04, 0.03]	[-0.04, 0.03]
P-value	0.261	
Baseline Mean	0.773	0.789
N	35,231	30,106

Notes: This table reports OLS estimates of Equation 1 for unmarried women aged 16–23 from lenient-attitude communities who became pregnant for the first time between 2009–2016. Column (1) reports estimates for the full lenient sample; Column (2) reports estimates for the same sample excluding women from families originating in the Former Soviet Union. The reported p-value tests whether the treatment effects differ between Columns (1) and (2). All regressions include month fixed effects and the interaction of ethno-religious-group and education fixed effects. Standard errors, clustered by age-by-year at conception, are reported in parentheses. Bounds that account for potential differential pre-trends, constructed using the method of Rambachan and Roth (2023), appear in brackets. $^{*}p < 0.10$, $^{**}p < 0.05$, and $^{***}p < 0.01$.

E Alternative Models

This appendix presents three alternatives to our main theoretical framework. The models in Sections E.1 and E.2 characterize the abortion decision as a standard optimization problem without intergenerational frictions, differing only in whether individuals face liquidity constraints. We show that both models generate predictions that are inconsistent with the empirical patterns documented in Section 3.2. Section E.3 introduces intergenerational frictions similar to those in our preferred framework, but through a distinct mechanism: a stigma cost associated with involving parents in the abortion decision.

E.1 No Constraints

Setup. Let i index a pregnant woman. Let C^A denote the monetary cost of obtaining an abortion and C^B the expected present discounted monetary cost of giving birth and raising a child. Let ξ_i^A and ξ_i^B capture non-monetary utilities from abortion and birth, respectively. The utility of abortion, $U_i^A(W)$, and birth, U_i^B, are

$$U_i^A(W) \; = \; \xi_i^A \; - \; (1-W)\,\alpha\, C^A, \qquad U_i^B \; = \; \xi_i^B \; - \; \alpha\, C^B,$$

where $W \in \{0,1\}$ indicates whether abortion is subsidized (with $W=1$ eliminating the out-of-pocket cost C^A) and $\alpha > 0$ is a price sensitivity parameter. A pregnant woman aborts if $U_i^A(W) \geq U_i^B$.

Intuition. The key insight is that the same price sensitivity parameter α governs both the cost of abortion, C^A, and the (much larger) expected cost of raising a child, C^B. As a consequence, a price sensitivity that is large enough to explain observed changes in abortion rates following the policy change implies that even tiny percentage fluctuations in C^B would generate enormous swings in abortion behavior.

Policy effect and identification of α. To quantify this argument in a simple way, we adopt a logit formulation of the choice problem. Assume

$$\xi_i^A = \overline{\xi}^A + \epsilon_i^A, \qquad \xi_i^B = \overline{\xi}^B + \epsilon_i^B,$$

with $\epsilon_i^A, \epsilon_i^B$ i.i.d. Type I Extreme Value.

Under this parameterization, the price sensitivity parameter, α, can be estimated as:

$$\alpha \;=\; \frac{\varphi}{C^A},$$

where φ is the change in log-odds when moving from no subsidy to subsidy:

$$\varphi \equiv \log\!\left(\frac{\mathbb{P}(A=1\,|\,W=1)}{\mathbb{P}(A=0\,|\,W=1)}\right) \;-\; \log\!\left(\frac{\mathbb{P}(A=1\,|\,W=0)}{\mathbb{P}(A=0\,|\,W=0)}\right) \tag{10}$$

Elasticity with respect to C^B. The elasticity of the abortion rate with respect to C^B is

$$\frac{\partial \ln \mathbb{P}(A=1\,|\,W)}{\partial \ln C^B} \;=\; \alpha\, C^B \left(1 - \mathbb{P}(A=1\,|\,W)\right).$$

Substituting $\alpha = \varphi/C^A$ and defining $R \equiv C^B/C^A$ as the ratio between the monetary cost of having an abortion and raising a child, we can express the elasticity as:

$$\frac{\partial \ln \mathbb{P}(A=1\,|\,W)}{\partial \ln C^B} = R \cdot \varphi \cdot \left[1 - \mathbb{P}(A=1\,|\,W)\right].$$

Numerical illustration. We calibrate the model using $\varphi = 0.39$ and $\mathbb{P}(A = 1 \mid W = 0) = 0.738$, both estimated from the data.[25] We set $R = 155$ based on estimates from Cohen and Romanov (2013).[26] Based on these values, we compute the elasticity of abortion rates with

[25]From Table 1, the baseline abortion rate is $\mathbb{P}(A = 1 \mid W = 0) = 0.738$, and the estimated policy effect is 0.068, implying $\mathbb{P}(A = 1 \mid W = 1) = 0.806$. Substituting these values into equation (10) yields $\varphi \approx 0.39$.

[26]Cohen and Romanov (2013) estimate the monthly cost of raising a child at 1,150 ILS. Using an annual discount rate of 1.75%—the Israeli policy rate in 2013—this implies a present discounted value of child-rearing costs of 264,000 ILS (in 2013 prices). With an abortion cost of 1,700 ILS, this yields $R \approx 155$.

respect to the cost of raising a child as follows:

$$\frac{\partial \ln \mathbb{P}(A=1 \mid W=0)}{\partial \ln C^B} \;=\; 100 \times 0.53 \times (1-0.2) \;=\; 15.8. \tag{11}$$

Equation (11) implies that a 10% increase in the expected cost of giving birth and raising a child would lead to more than a 150% increase in the abortion rate. If this were accurate, abortion rates would be expected to fluctuate dramatically in response to even small economic shocks—a pattern not observed in the data. Such an extreme elasticity suggests that the model's implications are not realistic under plausible variation in C^B. For comparison, Cohen and Romanov (2013), using Israeli data, estimate the elasticity of fertility with respect to the cost of raising a child to be 0.54—orders of magnitude smaller than the elasticity implied by Equation (11). Thus, the simple model presented in this section, when combined with observed policy effects, generates implausibly strong predictions about abortion behavior.

Heterogeneity. The model also fails to account for the heterogeneity in policy effects observed in the data. Because the policy operates solely through the price-sensitivity parameter α, the framework predicts larger responses among low-income women, who place greater marginal value on income. In contrast, we find no systematic differences in the policy's effects by parental income (see Table 1).

E.2 Liquidity Constraints

Setup. We next consider a setting in which some women wish to abort but cannot afford to do so. Let Y_i denote available liquidity. An abortion, indicated by $A_i(W)$, occurs if and only if it is both preferred and affordable:

$$A_i(W) \;=\; \mathbb{1}\{U_i^A(W) \geq U_i^B\} \cdot \mathbb{1}\{Y_i \geq C^A(1-W)\}.$$

With $W=1$, the subsidy eliminates the cost and liquidity never binds. With $W=0$, only women with $Y_i \geq C^A$ can afford an abortion.

Policy effect. Assuming independence between liquidity and preferences, the abortion rate is

$$a(W) = \pi^a(W) \cdot \pi^y(W),$$

where $\pi^a = \mathbb{P}(U_i^A(W) \geq U_i^B)$ is the share of women who prefer abortion and $\pi^y(W) = \mathbb{P}(Y_i \geq C^A(1-W))$ is the share who can afford it. The policy effect, measured by the change in log probabilities, decomposes as

$$\log a(1) - \log a(0) = \overbrace{[\log \pi^a(1) - \log \pi^a(0)]}^{\text{price channel}} - \overbrace{\log \pi^y(0)}^{\text{liquidity channel}} .$$

The first term reflects the price channel, while the second captures liquidity. A large observed effect may therefore arise not only from high price sensitivity but also from a substantial share of women who are liquidity constrained (small $\pi^y(0)$). In this sense, introducing liquidity constraints resolves one of the inconsistencies between the frictionless model presented in Section E.1 and the data.

Heterogeneity. The liquidity-constraint model fails to account for the full set of empirical patterns documented in Section 3.2. This framework predicts stronger effects among low-income women, who are more likely to face liquidity constraints. Yet we do not observe larger effects by income once we condition on abortion leniency. Moreover, the model offers no explanation for why the subsidy effect is larger in stricter communities: unless women in these communities are systematically more liquidity constrained, differences by social strictness should not emerge. In the data, the gap by strictness persists even after controlling for household income (see Table A.3), suggesting that liquidity constraints alone cannot explain the observed heterogeneity.

E.3 Stigma

Setup. In this section, we consider an alternative friction to the one in the baseline model presented in Section 4.2. In both frameworks, the central barrier to abortion is young women's reliance on parental financial support, so a subsidy increases autonomy by allowing them to bypass that requirement. The key difference is that, in the baseline model, requesting support is costless but parents may refuse to provide it. Here, instead, we assume that asking parents for financial support entails a non-monetary cost, capturing stigma or psychological disutility.

Let a daughter's latent desire to obtain an abortion be denoted by $V_d(h)$, where $h \in \{0, 1\}$ indexes the leniency of prevailing abortion attitudes in her community, with $h = 1$ denoting lenient environments and $h = 0$ strict ones. Let $V_p(h)$ denote the parents' latent willingness to support the procedure. As in the main model, both $V_d(h)$ and $V_p(h)$ are random variables shaped by the shared social environment, h.

A daughter obtains an abortion if her net utility $U(W, h)$ is non-negative:

$$U(W, h) \equiv V_d(h) - c(h) \cdot (1 - W) \geq 0,$$

where $W \in \{0, 1\}$ indicates whether the abortion is subsidized. When $W = 1$, the subsidy is in place and the daughter can obtain an abortion without involving her parents. When $W = 0$, there is no subsidy and she must ask her parents for assistance, incurring a stigma cost $c(h) > 0$. We model this cost as a decreasing function of parental leniency, so that more supportive parents imply a lower stigma cost. Under this formulation, the daughter aborts under the policy if $V_d(h) \geq 0$, and without the policy if $V_d(h) \geq c(h)$.

Policy effects. Define the level and percent effects of the subsidy for group h as

$$\Delta(h) \equiv \Pr\big[V_d(h) \geq 0\big] - \Pr\big[V_d(h) \geq c(h)\big],$$

and

$$\delta(h) \equiv \frac{\Pr\big[V_d(h) \geq 0\big] - \Pr\big[V_d(h) \geq c(h)\big]}{\Pr\big[V_d(h) \geq c(h)\big]}.$$

The level effect $\Delta(h)$ measures the absolute change in abortion rates when the stigma cost is removed, while the percent effect $\delta(h)$ captures the proportional change relative to the baseline rate.

The following proposition shows that the stigma model cannot account for our empirical findings without imposing additional structure on how stigma costs vary with abortion attitudes.

Proposition 3. Total Effect Need Not Decrease with Leniency. *There exist specifications of* $(V_d(h), V_p(h))$ *satisfying the assumptions of Proposition 2, and a stigma cost function* $c(h)$ *with* $c'(h) < 0$*, such that the level effect of the subsidy in the stigma model is larger for daughters from lenient communities; that is,* $\Delta(1) > \Delta(0)$.

Proof: *See Appendix H.5.*

Proposition 3 does not imply that stigma costs cannot explain our empirical results. Rather, it shows that the assumptions maintained in our preferred model, presented in Section 4.2, are not sufficient for the stigma model to generate the observed heterogeneity. Additional restrictions on the functional form of $c(h)$—beyond monotonicity with respect to leniency—would be required. For this reason, and for parsimony, we focus on the model developed in Section 4.2.

F Model Calibration

This appendix describes the calibration of the joint distribution of abortion attitudes between daughters and parents implied by the model in Section 4.1. The objective of the calibration is to recover the underlying correlation in abortion attitudes between daughters and parents.

F.1 Latent attitudes

For each group $h \in \{0, 1\}$, let $(V_d(h), V_p(h))$ denote the daughter's and parent's latent abortion attitudes. We assume

$$(V_d(h), V_p(h)) \sim \mathcal{N}\left(\begin{pmatrix}\mu_d(h) \\ \mu_p(h)\end{pmatrix}, \begin{pmatrix}1 & \rho(h) \\ \rho(h) & 1\end{pmatrix}\right).$$

An individual is classified as holding a lenient abortion attitude if the corresponding latent index is positive.

In the model, abortion occurs without the subsidy if both the daughter and the parent support abortion, and with the subsidy whenever the daughter supports abortion. Hence, for each h,

$$P(A \mid \tau = 0, h) = P(V_d(h) > 0, V_p(h) > 0), \qquad P(A \mid \tau = 1, h) = P(V_d(h) > 0).$$

F.2 Empirical moments

We use three moments for each group h.

First, from Table 1 we obtain the baseline abortion rate

$$s_0(h) = P(A \mid \tau = 0, h),$$

and the policy effect

$$\Delta(h) = P(A \mid \tau = 1, h) - P(A \mid \tau = 0, h).$$

These imply

$$P(V_d(h) > 0) = s_0(h) + \Delta(h), \qquad P(V_d(h) > 0, V_p(h) > 0) = s_0(h).$$

Second, from Table 2 (Israel), we recover the difference between daughters' and parents' abortion attitudes,

$$g(h) = P(V_d(h) > 0) - P(V_p(h) > 0),$$

where $g(0)$ is given by the coefficient on *Young*, and $g(1)$ by the sum of the coefficients on *Young* and *Young*×*Religious*. Combining this moment with $P(V_d(h) > 0)$ yields

$$P(V_p(h) > 0) = P(V_d(h) > 0) - g(h).$$

F.3 Recovery of model parameters

Given $P(V_d(h) > 0)$ and $P(V_p(h) > 0)$, the means of the latent indices are

$$\mu_d(h) = \Phi^{-1}(P(V_d(h) > 0)), \qquad \mu_p(h) = \Phi^{-1}(P(V_p(h) > 0)),$$

where $\Phi(\cdot)$ denotes the standard normal cdf.

Finally, the correlation $\rho(h)$ is chosen to satisfy

$$P(V_d(h) > 0, V_p(h) > 0) = \Phi_2(\mu_d(h), \mu_p(h); \rho(h)),$$

where $\Phi_2(\cdot, \cdot; \rho)$ is the bivariate normal cdf with correlation ρ. For each h, this equation admits a unique solution because the joint probability is strictly increasing in ρ holding $(\mu_d(h), \mu_p(h))$ fixed.

F.4 Implied correlations

Applying this procedure yields

$$\rho(0) \approx 0.46 \quad \text{(lenient group)}, \qquad \rho(1) \approx 0.97 \quad \text{(strict group)}.$$

The substantially higher correlation for the strict group reflects that, in the data, almost all cases in which parents support abortion coincide with daughters supporting abortion as well, implying very limited scope for disagreement within these households.

G Applications and Extensions: Details

G.1 Extending the Model to incorporate Geographic Variation

Consider a unit mass of locations indexed by ℓ, each with population size N_ℓ.[27] Throughout this section, "population" refers to the pregnant young women in each location—the *daughters* in the language of our model. Let the share of daughters of type h in location ℓ be denoted by $s_\ell(h)$.

As in Section 4.2, a daughter desires an abortion if $V_d(h) > 0$. Without government support ($W = 0$), she obtains an abortion only if her parents are also willing to help, i.e., if $V_p(h) > 0$. With government support ($W = 1$), parental support is no longer required.

We now present a proposition that formalizes the connection between prevailing social norms, baseline abortion rates, and the effect of the policy at the location level.

Proposition 4. Policy Effect Declines with Strictness and Baseline Abortion Rates across Locations.

Case 1 — Percent Effect. *Assume the pair $(V_d(h), V_p(h))$ satisfies the assumptions of Proposition 1. Then the percent effect of the policy in each location ℓ is strictly increasing in the share of daughters from* strict *backgrounds (equivalently, decreasing in the share from lenient backgrounds) and decreasing in the location's baseline abortion rate—i.e., the abortion rate that would prevail under $W = 0$.*

Case 2 — Total Effect. *Assume the pair $(V_d(h), V_p(h))$ satisfies the assumptions of Proposition 2. Then the total effect of the policy in each location ℓ is strictly increasing in the share of daughters from strict backgrounds and decreasing in the location's baseline abortion rate.*

Proof: *See Appendix H.3.*

Proposition 4 shows that the core insights from Section 4.2—which focused on individual-level variation—extend naturally to aggregate data. Specifically, the effect of government support on abortion rates should be larger in locations with a higher share of strict households (or, equivalently, smaller in locations with more lenient households) and should be smaller in locations where the baseline (no-policy) abortion rate is high. Intuitively, when

[27]We normalize total population to one, so that $\int_\ell N_\ell \, d\ell = 1$.

abortions are already frequent without government support, fewer daughters' decisions hinge on a subsidy, so both the relative and absolute effects of the policy diminish. In Section 5.2, we empirically validate these predictions using data from both Israel and the United States.

G.2 Policy Design

As in Appendix G.1, we consider a unit mass of locations indexed by ℓ. The government chooses a binary policy $W_\ell \in \{0, 1\}$, where $W_\ell = 1$ indicates that abortion support is offered to all daughters in location ℓ. Let C_ℓ denote the cost of offering government support in location ℓ.

We refer to the daughters who obtain an abortion only if they receive government support as *compliers*, that is, those with $V_d(h) > 0$ and $V_p(h) < 0$. Let π_ℓ^c denote the share of compliers in location ℓ, and let π_ℓ^a denote the share of daughters who desire an abortion. These are given by:

$$\pi_\ell^c = \sum_{h \in \{0,1\}} P(V_d(h) > 0, V_p(h) < 0 \mid h) \cdot s_\ell(h),$$
$$\pi_\ell^a = \sum_{h \in \{0,1\}} P(V_d(h) > 0 \mid h) \cdot s_\ell(h).$$

The policymaker's objective is to minimize the number of daughters who want to abort but are prevented from doing so due to parental opposition. Equivalently, the planner seeks to maximize the number of *compliers* reached by the policy—that is, the number of compliers in targeted locations:

$$\begin{aligned} \max_{W} \quad & \int \pi_\ell^c \cdot N_\ell \cdot W_\ell \, d\ell \\ \text{subject to} \quad & \int C_\ell \cdot W_\ell \, d\ell \leq B, \\ & W(\ell) \in \{0, 1\}, \end{aligned} \tag{12}$$

where B denotes the total budget available.

Problem (12) takes the form of a classic knapsack problem, a well-known optimization problem. The solution is intuitive. First, rank locations by their value-to-cost ratio—given in this context by $r_\ell \equiv \frac{\pi_\ell^c \cdot N_\ell}{C_\ell}$. Then, select locations in descending order of r_ℓ until the budget is exhausted. In what follows, we explore the implications of this solution under two alternative cost structures.

Suppose first that the cost of implementing the policy in a location is proportional to the total number of abortions that occur following policy implementation. This would arise, for example, if the policy consists of a subsidy that covers the cost of abortion procedures by paying a fixed amount per abortion to third-party providers, as in the empirical setting described in Section 2. In this case, the cost of treating location ℓ is given by

$$C_\ell = \pi_\ell^a \cdot N_\ell,$$

which corresponds to the number of daughters who would receive government support in location ℓ if it is treated.[28] The corresponding value-to-cost ratio becomes:

$$r_\ell = \frac{\pi_\ell^c}{\pi_\ell^a}.$$

That is, the optimal policy targets locations with the largest share of compliers among daughters who want to abort. Importantly, this ratio is equal to the *percent effect* of the policy in location ℓ, defined as the percent increase in abortion rates induced by policy implementation:

$$\frac{\pi_\ell^c}{\pi_\ell^a} = \frac{\mathbb{P}(A = 1 \mid W_l = 1, \ell) - \mathbb{P}(A = 1 \mid W_l = 0, \ell)}{\mathbb{P}(A = 1 \mid W_l = 0, \ell)}.$$

Thus, the optimal policy should prioritize locations where the policy has the largest percent effect.

Now suppose that the cost of providing support is proportional to the size of the population in a location, regardless of how many abortions occur. In this case, the cost of treating location ℓ is given by

$$C_\ell = N_\ell,$$

[28] The cost per abortion is normalized to one without loss of generality.

which corresponds to a fixed per capita cost across locations.[29] This cost structure may be more appropriate in settings where the government provides the service directly—for example, by building and operating abortion clinics. The corresponding value-to-cost ratio becomes:

$$r_\ell = \pi_\ell^c.$$

That is, the optimal policy targets the locations with the highest share of compliers among all daughters in the population. By definition, this share is equal to the *total effect* of the policy in location ℓ; that is, the increase in the abortion rate due to the policy.

We have shown that the optimal policy should target locations where it has the largest effect, which is intuitive. We now demonstrate that such locations are those with lower abortion rates in the absence of the policy.

Proposition 5. Targeting Rule under Alternative Cost Structures

Case 1 — Per-Abortion Cost. *Suppose the cost of implementing the policy in location ℓ is proportional to the number of abortions, and the assumptions of Proposition 1 hold.*

Case 2 — Per-Capita Cost. *Suppose the cost is proportional to the population of location ℓ, and the assumptions of Proposition 2 hold.*

In both cases, the optimal targeting rule is to prioritize locations in descending order of $\mathbb{P}(A = 1 \mid W_\ell = 0)$.

Proof: *See Appendix H.4.*

The intuition for Proposition 5 builds on the individual-level results in Propositions 1 and 2. When implementation costs are proportional to the number of abortions (Case 1), the policy is most efficient when it targets locations where the *percent change* in abortion use is greatest. Proposition 1 shows that this percent effect is larger for women whose families hold stricter views on abortion. Appendix H.4 then shows that this heterogeneity implies a location-level pattern: places with lower baseline abortion rates tend to exhibit larger percent effects. Thus, under per-abortion costs, the optimal targeting rule is to prioritize locations with lower abortion rates.

[29] The per capita cost is normalized to one without loss of generality.

A similar logic applies when costs are proportional to population (Case 2). In this case, the policy is most effective when it targets locations with the largest *change in levels*, rather than in percentage terms. Proposition 2 shows that this level effect is greater for women in communities with wider intergenerational gaps in abortion views—a stronger assumption than in the previous case. Appendix H.4 shows that this implies, at the location level, that the total effect also tends to be larger in places with lower baseline abortion rates. Therefore, under per-capita costs as well, the optimal rule is to prioritize locations with lower abortion rates.

In both cases, the same targeting rule emerges: prioritize locations with lower baseline abortion rates. However, this conclusion rests on different assumptions. Under per-abortion costs, it follows from relatively weak conditions on preferences. Under per-capita costs, it requires stronger assumptions about how intergenerational disagreement varies across communities.

H Proofs

H.1 Proof of Proposition 1

Define

$$g(h) := \mathbb{P}(V_p(h) \geq 0 \mid V_d(h) \geq 0).$$

Recall that the percent effect of the policy can be written as

$$\delta(h) = \frac{\mathbb{P}(A = 1 \mid W = 1, h)}{\mathbb{P}(A = 1 \mid W = 0, h)} - 1,$$

and note that

$$\frac{\mathbb{P}(A = 1 \mid W = 1, h)}{\mathbb{P}(A = 1 \mid W = 0, h)} = \frac{\mathbb{P}(V_d(h) \geq 0)}{\mathbb{P}(V_d(h) \geq 0, V_p(h) \geq 0)} = \frac{1}{g(h)}.$$

It follows that $g(1) \geq g(0)$ implies $\delta(1) \leq \delta(0)$. Our objective is therefore to show that $g(1) \geq g(0)$.

Express $g(h)$ as a conditional expectation:

$$g(h) = \mathbb{E}[\mathbf{1}(V_p(h) \geq 0) \mid V_d(h) \geq 0].$$

Let $\varphi(x) := \mathbf{1}(x \geq 0)$. Then:

$$g(h) = \mathbb{E}[\varphi(V_p(h)) \mid V_d(h) \geq 0].$$

Under the assumption that the joint distribution of $(V_d(h), V_p(h))$ satisfies the monotone likelihood ratio property (MLRP) in h, it follows that the conditional distribution of $V_p(1) \mid V_d(1) \geq 0$ first-order stochastically dominates that of $V_p(0) \mid V_d(0) \geq 0$. That is,

$$V_p(1) \mid V_d(1) \geq 0 \quad \text{FOSD} \quad V_p(0) \mid V_d(0) \geq 0.$$

Because $\varphi(x)$ is a weakly increasing function, first-order stochastic dominance implies

$$\mathbb{E}[\varphi(V_p(1)) \mid V_d(1) \geq 0] \geq \mathbb{E}[\varphi(V_p(0)) \mid V_d(0) \geq 0],$$

that is,

$$g(1) \geq g(0).$$

We conclude that $\delta(1) \leq \delta(0)$, as claimed. ■

H.2 Proof of Proposition 2

Let $A := \{(v_p, v_d) \in \mathbb{R}^2 : v_p < 0, v_d \geq 0\}$. For each $h \in \{0, 1\}$, define:

$$\begin{aligned}
J_h &:= \mathbb{P}((V_p(h), V_d(h)) \in A), \\
a_h &:= \mathbb{P}(V_p(h) < 0), \\
b_h &:= \mathbb{P}(V_d(h) \geq 0), \\
\pi_h &:= \frac{J_h}{a_h b_h}.
\end{aligned}$$

Our goal is to show that $J_0 > J_1$. Note that:

$$\frac{J_0}{J_1} = \frac{a_0 b_0}{a_1 b_1} \cdot \frac{\pi_0}{\pi_1}.$$

The assumption $G(0) > G(1)$ implies:

$$\frac{a_0 b_0}{a_1 b_1} > 1.$$

Hence, it suffices to show that $\pi_0 \geq \pi_1$, i.e., that the normalized joint probability mass in region A is decreasing in h.

We now appeal to a known implication of the multivariate monotone likelihood ratio property (MLRP), from Lehmann (1966). Specifically:

If the family of joint densities $\{f_h\}$ satisfies MLRP in (v_p, v_d), then for any measurable rectangles $A_1 \times A_2$, the ratio

$$\frac{\mathbb{P}(V_p(h) \in A_1, V_d(h) \in A_2)}{\mathbb{P}(V_p(h) \in A_1) \cdot \mathbb{P}(V_d(h) \in A_2)}$$

is decreasing in h whenever A_1 and A_2 are monotone in opposite directions.

In our case, $A_1 = (-\infty, 0)$ and $A_2 = [0, \infty)$ are monotone in opposite directions. Therefore, MLRP implies:

$$\pi_0 \geq \pi_1.$$

Combining this with $\frac{a_0 b_0}{a_1 b_1} > 1$, we conclude:

$$J_0 = a_0 b_0 \cdot \pi_0 > a_1 b_1 \cdot \pi_1 = J_1.$$

Hence,

$$\mathbb{P}(V_p(0) < 0, V_d(0) \geq 0) > \mathbb{P}(V_p(1) < 0, V_d(1) \geq 0),$$

as desired. Since $\Delta(h) = \mathbb{P}(V_p(h) < 0, V_d(h) \geq 0)$, it follows that $\Delta(0) > \Delta(1)$. ■

H.3 Proof of Proposition 4

Case 1 — Percent effect

Recall that $s_\ell(1)$ denotes the share of daughters in lenient families in location ℓ, and $s_\ell(0) = 1 - s_\ell(1)$ represents the share in strict families. The percent effect of the policy at location ℓ is given by

$$\frac{s_\ell(0)\big[\mathbb{P}(A=1|W=1,0) - \mathbb{P}(A=1|W=0,0)\big] + s_\ell(1)\big[\mathbb{P}(A=1|W=1,1) - \mathbb{P}(A=1|W=0,1)\big]}{s_\ell(0)\,\mathbb{P}(A=1|W=0,0) + s_\ell(1)\,\mathbb{P}(A=1|W=0,1)}.$$

This can be rewritten as

$$\delta_\ell = \frac{\mathbb{P}(A=1|W=1,0) + s_\ell(1)\big[\mathbb{P}(A=1|W=1,1) - \mathbb{P}(A=1|W=1,0)\big]}{\mathbb{P}(A=1|W=0,0) + s_\ell(1)\big[\mathbb{P}(A=1|W=0,1) - \mathbb{P}(A=1|W=0,0)\big]} - 1.$$

Differentiating δ_ℓ with respect to $s_\ell(1)$ yields

$$\frac{\mathbb{P}(A=1|W=1,1)\,\mathbb{P}(A=1|W=0,0)-\mathbb{P}(A=1|W=1,0)\,\mathbb{P}(A=1|W=0,1)}{\Big[\mathbb{P}(A=1|W=0,0)+s_\ell(1)\big(\mathbb{P}(A=1|W=0,1)-\mathbb{P}(A=1|W=0,0)\big)\Big]^2}.$$

Under the assumptions of Proposition 1, $\delta(0)>\delta(1)$. This inequality implies

$$\frac{\mathbb{P}(A=1|W=1,0)}{\mathbb{P}(A=1|W=0,0)}>\frac{\mathbb{P}(A=1|W=1,1)}{\mathbb{P}(A=1|W=0,1)},$$

which can be rearranged as

$$\mathbb{P}(A=1|W=1,1)\,\mathbb{P}(A=1|W=0,0)-\mathbb{P}(A=1|W=1,0)\,\mathbb{P}(A=1|W=0,1)<0.$$

Since the denominator of the derivative is strictly positive, the derivative of δ_ℓ with respect to $s_\ell(1)$ is negative. Hence, the percent effect is strictly increasing in the share of strict households (equivalently, decreasing in the share of lenient households).

Next define the baseline (no-policy) abortion rate in location ℓ as

$$B_\ell=s_\ell(0)\,\mathbb{P}(A=1|W=0,0)+s_\ell(1)\,\mathbb{P}(A=1|W=0,1).$$

Because $\mathbb{P}(A=1|W=0,1)>\mathbb{P}(A=1|W=0,0)$, B_ℓ is strictly increasing in $s_\ell(1)$. The previous paragraph showed that δ_ℓ decreases in $s_\ell(1)$. Combining these facts and applying the chain rule yields that δ_ℓ also decreases in B_ℓ—i.e., locations with higher baseline abortion rates experience smaller percent effects. ■

Case 2 — Total effect

The total effect of the policy at location ℓ is

$$\Delta_\ell=s_\ell(1)\,\Delta(1)+[1-s_\ell(1)]\,\Delta(0)=\Delta(0)+s_\ell(1)\big[\Delta(1)-\Delta(0)\big].$$

Under the assumptions of Proposition 2, $\Delta(1)<\Delta(0)$, so $\Delta(1)-\Delta(0)<0$. It follows that Δ_ℓ is strictly decreasing in $s_\ell(1)$—equivalently, increasing in the share of strict households.

As above, the baseline abortion rate B_ℓ is strictly increasing in $s_\ell(1)$. Therefore, Δ_ℓ is also decreasing in B_ℓ, implying that locations with higher no-policy abortion rates experience smaller total effects. ■

H.4 Proof of Proposition 5

Case 1 - Per-Abortion Cost

Suppose implementation costs are proportional to the number of abortions. By Proposition 3 and under the assumptions of Proposition 1, the percent effect δ_ℓ decreases in $s_\ell(1)$, the share of daughters from lenient families. Since $\mathbb{P}(A = 1|W = 0, 0) < \mathbb{P}(A = 1|W = 0, 1)$, the no-policy abortion rate, given by $s_\ell(0)\mathbb{P}(A = 1|W = 0, 0) + s_\ell(1)\mathbb{P}(A = 1|W = 0, 1)$ is increasing in $s_\ell(1)$. It follows that δ_ℓ is decreasing in the no-policy abortion rate.

Since optimal targeting prioritizes locations with the highest percent effect, it is therefore optimal to target locations in ascending order of baseline abortion rates. ■

Case 2 - Per-Capita Cost

Suppose implementation costs are proportional to population size. By Proposition 3 and under the assumptions of Proposition 2, the total effect Δ_ℓ decreases in $s_\ell(1)$. As noted above, the no-policy abortion rate is increasing in $s_\ell(1)$, and thus the total effect is decreasing in the baseline abortion rate. Therefore, it is optimal to prioritize locations with lower no-policy abortion rates, where the policy has the greatest absolute effect. ■

H.5 Proof of Proposition 3

In this appendix, we show that there exists a parameterization of the stigma model presented in Section E.3 that satisfies the assumptions of Proposition 2, yet predicts a larger subsidy effect for the lenient group.

Let the stigma cost be given by

$$c(h) = \exp\big(-\bar{c} \cdot V_p(h)\big), \quad \bar{c} = 0.1.$$

Assume that $(V_d(h), V_p(h))$ are jointly normally distributed with zero correlation. Specifically,

$$V_d(h) \sim \mathcal{N}(\mu_d(h), 1), \quad \mu_d(1) = 0.3,\ \mu_d(0) = -0.1,$$

and

$$V_p(h) \sim \mathcal{N}(\mu_p(h), 1), \quad \mu_p(1) = 0.2,\ \mu_p(0) = -0.4.$$

These values satisfy $\mu_d(1) > \mu_d(0)$ and $\mu_p(1) > \mu_p(0)$. Under joint normality—and with a common covariance matrix across h—, this implies that the pair $(V_d(h), V_p(h))$ satisfies the monotone likelihood ratio property (MLRP) in h. Moreover, computing the mismatch index yields $G(1) \approx 0.26$ and $G(0) \approx 0.30$, so that $G(0) > G(1)$. The assumptions of Proposition 2 therefore hold.

Despite this, the model predicts a larger policy effect for the lenient group. Specifically, the subsidy effect is approximately 0.37 for the lenient group and 0.33 for the strict group.

$\blacksquare$