Motherhood and Violence*

Gabriela Deschamps †

February 6, 2026

[Most recent version here]

Abstract

Two factors shape women's exposure to intimate partner violence (IPV): whether partners use violence and whether women remain in violent relationships. Standard IPV measures cannot separate these margins, yet they imply different policy responses. I propose a framework to disentangle them in the context of motherhood, when IPV exposure often rises. A simple model yields distinct predictions which can be tested with data recording past violence and separation histories. Using Mexican survey data with these features and exogenous fertility variation from pharmacy openings, I find that motherhood increases violence exposure entirely through reduced exit, not changes in men's behavior.

* I would like to thank Oriana Bandiera, Nava Ashraf, Rafe Meager, Robin Burgess, and Matthias Doepke for their support and guidance in this project. For very helpful comments I would like to thank Abi Adams, Dan Anderberg, Clare Balboni, Eduard Boehm, Gharad Bryan, Wendy Carlin, Alice Evans, Maitreesh Ghatak, Nilmini Herath, Paul Hünermund, Amen Jalal, Eliana La Ferrara, Gabriel Leite-Mariante, Gilat Levy, Ben Moll, Canishk Naik, Paula Onuchic, Will Parker, Michelle Rao, Imran Rasul, Núria Rodríguez Planas, Patrick Schneider, Daniel Sharp, Caterina Soto, Sarah Winton, and numerous seminar and conference participants. I thank Natalia Volkow and the team at *Laboratorio de Microdatos* of INEGI for facilitating access to confidential data, and MexFam for their assistance in organizing focus groups and interviews used for contextual interpretation. Funding provided by STICERD is gratefully acknowledged.

†University College London, email: g.deschamps@ucl.ac.uk, website: www.gabrieladeschamps.com.

I Introduction

Intimate partner violence (IPV) is a pervasive problem across levels of economic development (Sardinha et al., 2022), with more than one in four women worldwide having experienced physical or sexual IPV in their lifetime (WHO, 2021). A necessary step towards reducing IPV is understanding why it happens. When more women experience violence, does this reflect an increase in men's propensity to use violence, or a decline in women's ability to leave abusive partners? Distinguishing between these forces is essential for policy and interpretation, yet not possible with standard IPV data.

The purpose of this paper is to disentangle these mechanisms empirically. The challenge is that most commonly used IPV outcomes in surveys and administrative sources capture violence *exposure*, meaning whether women experience violence over a recent period. When separation effectively protects women from violence, exposure reflects two behavioral margins: whether violence occurs and whether women remain in relationships where violence occurs. As a result, both a woman who has a non-violent partner and a woman who has left an abusive partner look identical in the data.

I propose a framework to disentangle men's violent behavior from women's separation decisions in the context of motherhood, as most studies find that women's exposure to IPV rises after childbirth (Massenkoff and Rose, 2024; Britto et al., 2024; Bø Vatnar and Bjørkly, 2010). I develop a simple game-theoretic model that yields distinct testable predictions for each channel and test them using nationally representative Mexican survey data. The key data feature is that women report violence by both current and former partners, which lets me construct a proxy for men's violent behavior that is not mechanically affected by separation.

The results are consistent with selection: motherhood affects IPV exposure through reduced separation rather than changes in men's violent behavior. This conclusion is supported by both descriptive patterns and causal estimates using pharmacy openings as a source of exogenous variation in fertility. These findings imply that exposure-based IPV measures, while central for welfare, should not be interpreted as direct evidence of changes in perpetrators' behavior.

I begin by documenting two stylized facts from Mexico showing that separation is a key determinant of IPV exposure. First, separation is empirically relevant: about one in three women who have experienced physical abuse has left her partner, compared with fewer than one in ten among women who have not experienced physical violence. Second, separation sharply reduces subsequent exposure to that partner's violence: while roughly 40 percent of separated women report having ever suffered physical violence from their former partner, fewer than 3 percent report physical violence in the past year five years after leaving.

Motivated by these facts, I develop a simple two-period model in which men decide whether to exert violence and women decide whether to stay or leave the relationship. Men differ in their satisfaction from violence, and women differ in their costs of separation. In this framework, a woman's probability of being exposed to violence is the product of two decisions: whether her partner uses violence and whether she stays conditional on violence. Some couples exogenously become parents at the beginning of the first period, which lowers women's outside option. This shift can increase men's incentives to use violence and reduce women's ability to leave after violence, so motherhood can increase exposure through either margin.

The model highlights two channels. Under the *direct channel*, fatherhood increases men's propensity to use violence. Because violence triggers separation for some women, the increase in violence leads

some women to leave, so both exposure and separation rise among mothers. Under the *selection channel*, men's violent behavior is unchanged by fatherhood, but motherhood raises women's costs of leaving. In this case, mothers are less likely to separate conditional on violence, so exposure rises because women remain in violent relationships. Both channels predict higher IPV exposure among mothers, but they imply different patterns for men's violent behavior and women's separation decisions.

Testing the channels requires data on men's propensity to use violence and women's separation decisions, not only on women's recent exposure to violence. A common approach in the literature to isolate violent behavior is to focus on intact couples, but this can generate sample selection bias when both the treatment and violence affect separation. If violence increases separation while motherhood reduces it, a sample of partnered women would exclude an important group: non-mothers who experienced violence and left their partners. Conditioning on civil status can therefore induce a spurious correlation between motherhood and violence.

I propose that the mechanisms can be disentangled using survey data with two features. First, the survey must include women of all civil statuses, so that separation is observed rather than conditioned on. Second, it must collect violence histories for current and former partners, including measures of both recent violence and whether violence ever occurred with that partner. The recent measure captures exposure, while the ever measure provides a proxy for men's violent behavior that remains observable even after separation. In particular, a separated woman reports no recent violence but can still report whether her former partner was ever violent. ENDIREH, a nationally representative survey of household dynamics in Mexico, has precisely these features.

I start by testing the model's predictions descriptively among women who recently entered their first cohabiting relationship. Controlling for a rich set of fixed effects, I find that mothers are 30 percent more likely to experience physical violence from their first partner in the last 12 months, a measure of violence exposure. In contrast, mothers and non-mothers are equally likely to have ever experienced physical violence with that partner, suggesting that motherhood does not change men's violent behavior. At the same time, mothers are 65 percent less likely to be separated from their first partner. Together, these patterns, with no change in men's violent behavior but lower separation among mothers, are consistent with the selection channel of the model.

The next exercise moves from correlations to causality. The pharmacy sector in Mexico expanded by 21 percent between 2011 and 2016, creating a plausibly exogenous shock to fertility that I use to estimate the causal effect of motherhood on violence and separation. Pharmacies are the main source of contraceptives for non-mothers: among those who use contraception, 76 percent obtain it from a pharmacy. They offer an accessible and private alternative to public clinics, and no prescription is required for any contraceptive method. To measure changes in access, I construct a novel dataset that merges confidential block-level location data from the 2011 and 2016 waves of *ENDIREH* with the universe of businesses in Mexico, allowing me to calculate each woman's distance to the nearest pharmacy and to identify those who gained access during this period.

I estimate the effects of pharmacy openings on fertility, violence, and separation in a difference-in-differences framework. The treated group consists of women who had no pharmacy within one kilometer in 2011 but did in 2016, while the control group includes women with no pharmacy access in either year. I restrict the analysis to women who were not mothers five years before the survey to focus on the extensive margin of motherhood. The identifying assumption is that, absent pharmacy openings, treatment and

control groups would have followed parallel trends in fertility, violence, and separation. Using the 2006 wave, I confirm that there were no differential pre-trends between groups before pharmacy expansion.

Because pharmacies sell a wide range of products, it is unlikely that their openings are driven by local trends in fertility, violence, or separation. Nonetheless, pharmacy openings may still correlate with other local factors that affect these outcomes, potentially violating the parallel trends assumption. To test this, I identify eight types of businesses that grew at a similar rate between 2011 and 2016. If pharmacy effects simply reflected economic growth of the area, similar patterns would appear for other types of business openings. I find that only pharmacy openings, and not other types of businesses, significantly affect fertility, violence, or separation.

My main results are as follows. Pharmacies are an effective mechanism for reducing fertility, as women are 9.5 percentage points less likely to be mothers after a pharmacy opening ($p = 0.013$). Both the direct and selection channels predict a decline in women's exposure to violence, either because fewer men become fathers or because women can delay motherhood and leave violent partners. Consistent with these predictions, treated women are 4.2 percentage points less likely to have experienced physical violence in the last twelve months ($p = 0.02$).

To distinguish between mechanisms, I use the relationship history to identify whether the partner from before the pharmacy opening was ever violent and find that women in treated and control areas are equally likely to have experienced violence with that partner. I then measure separation as whether a woman is separated or single with a previous partner at the time of the survey and find that pharmacy access increases this probability by 9.4 percentage points ($p = 0.07$). This effect is driven by women who are single with a previous boyfriend rather than by separations from cohabiting relationships. Taken together, the evidence supports the selection channel where motherhood does not alter men's violent behavior but raises women's exposure to violence by increasing exit costs.

To use pharmacy openings as an instrument for motherhood, two additional assumptions are required beyond parallel trends. The first is monotonicity, which requires that no woman becomes more likely to be a mother when she has access to a pharmacy; this is plausible because pharmacies expand access to contraception but do not provide services that increase fertility. The second is the exclusion restriction, which requires that pharmacy openings affect violence and separation only through changes in fertility. To assess this, I use women who were already mothers prior to the pharmacy opening as a placebo group. These women had access to all other services provided by a pharmacy, but the opening occurred after they became mothers. For this group, I find no effect on fertility, violence, or separation.

I proceed to estimate the local average treatment effect (LATE), which captures the causal effect of motherhood on violence and separation for the compliers. By definition, compliers are women who wanted to avoid a pregnancy, as they are the ones whose fertility responds to increased access to contraception. Heterogeneity analysis suggests that these women are typically younger, from poorer backgrounds and in dating relationships. For this group, a one standard deviation increase in the predicted probability of motherhood raises the risk of experiencing physical violence by 10.5 percentage points and reduces the probability of separation by 18 percentage points, with no effect on men's propensity to exert violence. The causal effects are qualitatively similar to those documented in the descriptive analysis, consistent with the selection channel predicted by the model. My findings highlight the importance of reproductive rights for women. Given that motherhood imposes separation costs, delaying pregnancy allows women time to exit a violent relationship before having children.

The two-mechanism framework is not unique to motherhood. Many policies and shocks studied in the IPV literature—such as cash transfers, employment opportunities, or legal reforms—can affect women's exposure to violence through two margins: they can change men's propensity to use violence, and they can change women's likelihood of leaving violent partners. Recognizing separation as an endogenous outcome has two implications for empirical work. First, conditioning on relationship status, for example by restricting the sample to partnered women or controlling for civil status, is common in practice but can generate sample selection bias. Second, exposure-based IPV measures are central for welfare but should not be read as direct evidence of changes in men's violent behavior. My analysis provides an empirical illustration in which changes in IPV exposure are driven by changes in separation rather than changes in perpetrators' behavior.

This paper makes four contributions to the literature. First, it develops a unified theoretical and empirical framework to distinguish changes in men's violent behavior from changes in women's separation decisions. While previous work highlights the role of separation in reducing women's exposure to violence (Dugan et al., 1999; Adams et al., 2024), it does not provide a framework that allows these mechanisms to be separately identified. I address this gap by developing a noncooperative game-theoretic model in which motherhood shapes both men's incentives to exert violence and women's costs of separation, and I show that the channels can be distinguished using data that record violence with both current and former partners. The theory builds on economic models of intra-household conflict used to study dowries (Bloch and Rao, 2002; Calvi and Keskar, 2023), cash transfers (Bobonis et al., 2013; Angelucci, 2008; Haushofer et al., 2019), and employment (Anderberg et al., 2015; Sanin, 2024; Adams et al., 2024). Closely related is Anderberg et al. (2023), who also models the interaction between violence, fertility, and separation but treats violence as nonstrategic.

Second, my findings may help reconcile why IPV increases after childbirth in settings such as Brazil (Britto et al., 2024) and the United States (Massenkoff and Rose, 2024) but decreases in Sweden (Bergvall and Rodríguez-Planas, 2024), where stronger social support systems facilitate women's ability to leave abusive relationships. Third, I contribute to the literature on how separation costs shape IPV (Stevenson and Wolfers, 2007; Brassiolo, 2016; García-Ramos, 2021), showing that motherhood can increase these costs and limit women's ability to leave abusive relationships. Lastly, prior research shows that access to contraception improves women's education, labor market outcomes, and mental health (Goldin and Katz, 2002; Bailey, 2006; Miller, 2010; Ashraf et al., 2014). My findings add IPV reduction to this list, while also clarifying the mechanism: contraception protects women not by changing men's behavior, but by allowing them to delay motherhood and leave violent partners before children raise exit costs.

The remainder of the paper is organized as follows. Section II introduces the data and documents the facts that motivate the model. Section III presents the theoretical framework. Sections IV and V test the model's predictions using descriptive and causal evidence, respectively. Section VI concludes.

II Data and Motivating Facts

II.A Data

The main data source is the *Encuesta Nacional sobre la Dinámica de las Relaciones en los Hogares* (ENDIREH), a nationally representative survey from Mexico conducted every five years by the National Institute of Statistics and Geography (INEGI, 2006, 2011, 2016). ENDIREH collects detailed information

on women's experiences of emotional, economic, physical and sexual violence, as well as fertility and socioeconomic characteristics. Throughout the paper, I focus on physical violence, which is more consistently measured across waves and civil statuses. I use the 2006, 2011 and 2016 waves, which surveyed 133,398, 152,636 and 111,256 women respectively. Survey protocols follow international standards on privacy, consent and safety, and interviews are conducted by trained female enumerators with backgrounds in psychology, social work and related fields.

Three features make ENDIREH especially well suited for this study. First, the sample covers women in all civil statuses, including those currently married or cohabiting, those who are separated or divorced, and those who have never partnered. This avoids the issue of sample selection bias that arises in surveys that use civil status as inclusion criteria. Second, the questionnaire records violence histories with both current and former partners, distinguishing between violence in the past twelve months and whether violence ever occurred in a given relationship. This allows me to observe violence even after relationships end. Third, through secure access at INEGI facilities, I obtained the location of the women in the survey at the block level, which correspond to the smallest geographic unit recorded by INEGI. These confidential data allow me to measure local access to pharmacies.

To measure access to pharmacies, I combine ENDIREH with two additional INEGI datasets: the *Directorio Estadístico Nacional de Unidades Económicas* (DENUE), which contains the universe of registered businesses in Mexico, including the precise location of pharmacies (INEGI, 2011, 2016), and the *Marco Geoestadístico*, which provides block-level digital maps for all thirty-two states (INEGI, 2010). In Section V.B I describe how these datasets are merged to construct measures of pharmacy access.

Finally, to document patterns of contraceptive and pharmacy use in Mexico (Section V.A), I use the *Encuesta Nacional de la Dinámica Demográfica* (ENADID), another nationally representative INEGI survey (INEGI, 2009, 2014). I use the 2009 and 2014 waves, which surveyed 100,515 and 98,711 women, respectively, and contain detailed information on contraceptive methods, sources of acquisition, and fertility intentions.

II.B Two Margins of Violence Exposure

Most empirical work measures IPV using two types of data: administrative records and household surveys.[1] Administrative sources identify IPV through police reports, hospital records, or mortality registries. Surveys usually rely on questions about whether the woman experienced violence over a recent reference period, most commonly the last 12 months. In both cases, these measures capture women's recent *exposure* to violence.

In a world where relationships are stable and women remain with the same partner, recent exposure would directly reflect men's underlying propensity to use violence. However, if women leave violent relationships and separation protects them from further violence, then exposure reflects two behavioral margins: (1) whether the partner uses violence, and (2) whether the woman remains in the relationship conditional on violence. In this case, standard IPV measures capture only the *product* of these two decisions. A woman who has a non-violent partner and a woman who left an abusive partner both report no recent violence. They are observationally equivalent.

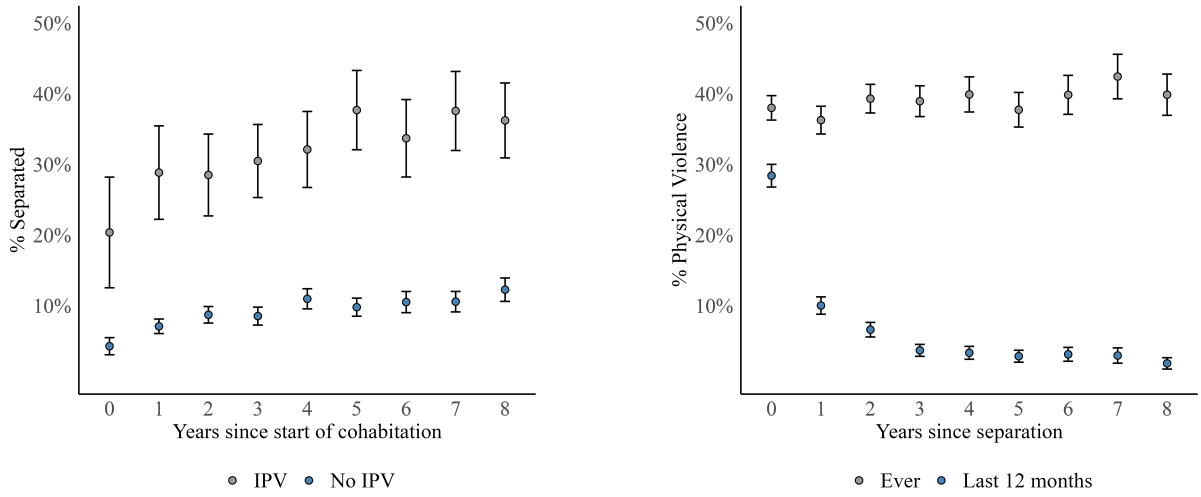
Whether this distinction matters in practice depends on the empirical importance of separation. I now show that separation is both common among women who experience violence and highly protective

[1] See Table A1 in the Appendix for a summary of the data sources and outcome measures used in recent empirical work.

against subsequent exposure.

Fact 1: Women who experience violence are more likely to separate.

I examine relationship dissolution among women who recently entered cohabiting unions. I focus on those who began their most recent cohabiting relationship within the last eight years and split them by whether they ever experienced physical violence with that partner. For each group, I compute the share of women who are separated at the time of the survey. Figure 1a plots these separation rates by years since the start of cohabitation. Across all durations, women who ever experienced physical violence are much more likely to be separated than those who did not. On average, 31.8 percent of women who experienced violence are separated compared with 9.2 percent of those who did not, meaning separation is more than three times as common among women who experienced violence. This pattern is consistent with evidence from higher-income settings (Bowlus and Seitz, 2006; Anderberg et al., 2023; Adams et al., 2024).[2]



(a) Separation by violence status

(b) Violence after separation

Notes: Panel (a) plots the probability that a woman is separated from her last cohabiting partner at the time of the survey by years since the start of cohabitation and whether she ever experienced physical IPV with that partner. Panel (b) plots the probability that a woman experiences physical IPV from her ex-partner by years since separation, distinguishing between ever experiencing physical IPV and experiencing physical IPV in the last 12 months. Both panels display 95% confidence intervals. All regressions are weighted using survey weights, and standard errors are heteroskedasticity-robust.

Figure 1: Violence and Separation Dynamics

Fact 2: Separation reduces exposure to violence.

I now examine what happens after a relationship ends. I focus on all women who separated from their last cohabiting relationship within the previous eight years and examine reports of physical violence by that same ex-partner. Figure 1b plots two measures: whether women ever experienced physical violence during the relationship, and whether they experienced physical violence from that partner in the past

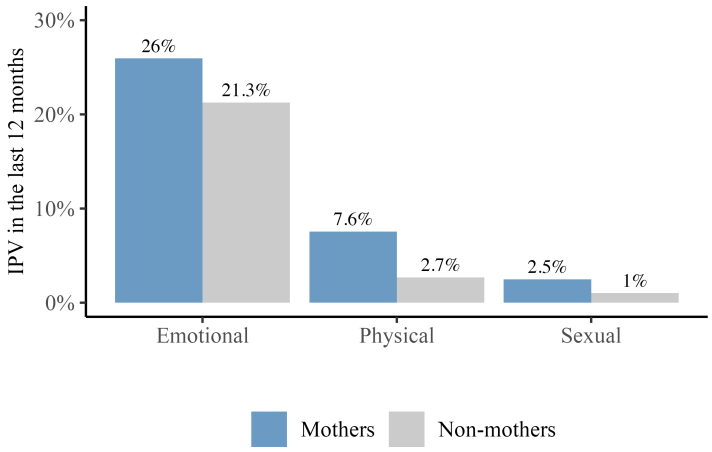
[2]For example, Bowlus and Seitz (2006) show that abused young women in Canada are 245 percent more likely to be divorced than non-abused counterparts; Anderberg et al. (2023) find that separation rates in the United Kingdom are about five times higher following periods of abuse; and Adams et al. (2024) document that improvements in women's outside options increase separation among abusive couples in Finland.

twelve months. Although roughly 40 percent report ever having been abused, recent violence declines sharply after separation. Five years after leaving, only 2.8 percent report violence by that partner in the last year (7.5 percent among those who had an abusive partner). Separation therefore sharply reduces exposure to violence from that partner. This is consistent with exposure reduction theory, proposed in criminology by Dugan et al. (1999): women reduce their exposure to violence by leaving abusive relationships.

Together, these facts show that separation is a key determinant of violence exposure. Because women who experience violence are more likely to leave, and leaving reduces their subsequent exposure, measured exposure reflects both men's violent behavior and women's separation decisions. Changes in exposure can be driven by changes in either margin.

II.C Motherhood as a case study

I propose a framework to disentangle these mechanisms in the context of motherhood. I focus on motherhood because it is a setting where both margins could plausibly operate and where the gap in violence exposure is large. In Mexico, mothers are substantially more likely to experience violence than non-mothers. Figure 2 shows the probability of experiencing violence in the last 12 months by motherhood status.[3] Twenty-six percent of mothers report emotional violence compared with 21.3 percent of non-mothers. Physical violence shows an even larger relative gap: 7.6 percent of mothers experienced physical violence in the last year compared with 2.7 percent of non-mothers, nearly a threefold difference. Sexual violence shows a similar pattern, at 2.1 percent for mothers versus 1 percent for non-mothers.



Notes: This figure shows the population-weighted share of women who experienced physical, emotional, or sexual IPV from their partner (or ex-partner) in the last 12 months, by motherhood status. The sample includes all women aged 15–45 who have ever been in a romantic relationship (boyfriend or partner). See Appendix E for variable definitions.

Figure 2: Prevalence of IPV by Motherhood Status

This pattern is not unique to Mexico. Massenkoff and Rose (2024) show that the birth of a first child increases male arrests for violent crimes by 50 to 100 percent in the United States, while Britto et al.

[3] Appendix E.A describes the definitions and survey questions used to construct the measures of emotional, physical, and sexual violence.

(2024) document a 215 percent increase in male criminal cases in Brazil. However, the relationship is not universal: Bergvall and Rodríguez-Planas (2024) find that childbirth reduces IPV-related hospitalizations in Sweden.

Why do mothers experience higher rates of violence? Is it because fatherhood increases men's violent behavior, or because motherhood makes it harder for women to leave abusive partners? Both are plausible. The birth of a child could increase men's violent behavior through several channels emphasized in the IPV literature: it increases financial and emotional stress, changes how couples allocate time and resources, and shifts household bargaining power (Bloch and Rao, 2002; Aizer, 2010; Card and Dahl, 2011).

At the same time, motherhood increases women's economic dependence on their partners through labor market penalties (Kleven et al., 2019; Kleven, 2022; Kleven et al., 2025), unequal costs of childrearing (Doepke and Kindermann, 2019), and direct childcare responsibilities. These barriers are salient to Mexican women. In focus groups, one participant explained: "If I can't make ends meet now, I know it'll be even harder if I separate. Sometimes you stay not out of love, but out of habit or necessity."[4] If motherhood makes exit more costly, then mothers may remain in violent relationships that non-mothers would leave.

III Theoretical Framework

I develop a simple two-period model in which some couples exogenously become parents, men decide whether to use violence, and women decide whether to remain in the relationship or separate. The model formalizes the two-margin framework from Section II.B and yields four testable predictions that guide the empirical analysis in Sections IV and V.

The model's structure reflects two features of the empirical setting. First, women do not initially know whether their partner will be violent. This is consistent with qualitative evidence that violence in relationships often emerges gradually rather than being apparent from the start (Daw et al., 2023; McLindon et al., 2025). Second, some couples become parents before women observe their partner's violent behavior. This reflects the Mexican context, where women become mothers soon after entering relationships: the median time from relationship start to first birth is two years, implying conception occurs roughly one year after couples begin dating (Appendix Figure A1).

III.A Environment

Private types. Before the game starts, players draw their private types. Men differ in the utility they derive from violence, and women differ in their cost of leaving a relationship. A man's private type is $\alpha \in \{\alpha^{NA}, \alpha^{A}\}$: abusive men ($\alpha^{A} > 0$) gain utility from violence, while non-abusive men ($\alpha^{NA} < 0$) incur a disutility. A man is abusive with probability q, which is common knowledge, but his type is privately known to him and not directly observed by his partner.

Each woman draws a private dissolution cost $\delta > 0$ from distribution $F(\delta)$, where lower δ corresponds to a better outside option. While some determinants of outside options, such as wages or education, are observable, others (e.g., access to informal support, hidden financial resources, psychological readiness

[4] I conducted seven focus groups and three interviews with recent mothers in Iguala, Morelia, and Huajuapan de León. Discussions followed semi-structured questions about how motherhood affected relationship dynamics.

to leave) are not. I abstract from the observable components and let δ capture the unobservable part of the outside option. This implies that men are uncertain about whether a woman will leave if violence occurs.

Timing. There are two periods, $t = 1, 2$. In $t = 1$, couples are matched and exogenous fertility is realized (p_1). Men then decide whether to exert violence ($v_1 \in \{0, 1\}$), after which women decide whether to stay ($s = 1$) or leave ($s = 0$). If a woman leaves, both partners remain single for the rest of the game. In $t = 2$, all couples who remain together and are not yet parents exogenously become parents, and partnered men again decide whether to exert violence. Women do not make any decisions in the second period.[5]

Preferences. At the end of each period, each individual receives utility $U_t^{g,c}$, where $g \in \{w, m\}$ denotes gender (woman or man) and $c \in \{r, s\}$ denotes civil status, indicating whether the individual is in a relationship (r) or single (s).

The utility of being in a relationship is a function of parenthood status and the (dis)utility of violence. Parenthood yields utility normalized to 1. Violence imposes a cost $\bar{\alpha}$ on women and yields payoff α to men:

$$U_t^{w,r} = p_t - v_t\bar{\alpha}, \qquad U_t^{m,r} = p_t + v_t\alpha.$$

For non-abusive men, $\alpha = \alpha^{NA} < 0$, so violence reduces their utility. For abusive men, $\alpha = \alpha^{A} > 0$, so violence raises their utility.

If a woman leaves the relationship, she pays a private dissolution cost δ. If she is a mother, she derives positive utility from it but also bears a cost of single motherhood $\bar{c}$.[6] Men face a common and observable dissolution cost $\bar{\delta}$ and derive utility from being a parent.[7] Utilities when single are therefore:

$$U_t^{w,s} = -\delta + p_t - p_t\bar{c}, \qquad U_t^{m,s} = -\bar{\delta} + p_t.$$

Total utility over both periods is $U^g = U_1^{g,c} + \beta U_2^{g,c}$ with discount factor β. The dissolution cost δ applies in each period of singleness (e.g., for a woman, leaving in period 1 entails a total cost of $(1+\beta)\delta$).

Assumptions. I impose the following restrictions:

1. $\bar{\alpha} > \bar{c} \geq \frac{\beta}{1+\beta}$
2. $\alpha^{NA} < -(\beta + \bar{\delta}(1+\beta))$
3. $q\bar{\alpha} < 1$
4. $F(\bar{\alpha} - \frac{\beta}{1+\beta}) < \frac{1}{1+\beta}$

Assumption 1 guarantees that violence is costly enough that some women will leave a violent relationship independently of their motherhood status, but that single motherhood remains sufficiently costly to make mothers (weakly) more likely to stay compared to non-mothers.[8] Assumption 2 rules

[5] Allowing women to leave in $t = 2$ would not change equilibrium behavior in $t = 1$ but complicates notation. An extension is available upon request.

[6] This assumption is consistent with evidence that mothers bear most childrearing costs and face lower outside options after the first birth. In Mexico, women spend more than twice as many hours on unpaid care as men (INEGI, 2019) and the child penalty is 44% (Kleven et al., 2025). Similar patterns hold in Europe, where women perform most childcare tasks (Doepke and Kindermann, 2019).

[7] This could be relaxed to allow fathers to derive utility $0 < \lambda < 1$ from having a child when single, reflecting that they may have less contact with the child after separation. Predictions would be qualitatively unchanged.

[8] If the cost of single motherhood were very low ($\bar{c} < \frac{\beta}{1+\beta}$), non-mothers would be more likely to stay than mothers after violence. While not relevant for the Mexican context, the analysis could be extended to cover such cases.

out violence by non-abusive men. Assumption 3 bounds the prevalence of abusive men so that women do not exit relationships out of fear of violence without observing some violent behavior beforehand. Assumption 4 guarantees that for some values of α^A, all abusive men reveal their type in the first period independently of their fertility outcomes.[9]

III.B Equilibrium

I study Perfect Bayesian Equilibrium (PBE). In this model, an equilibrium in pure strategies is defined as a situation where (a) men's and women's strategies maximize their expected utility, given their beliefs and the strategies of the other player, and (b) beliefs are updated according to Bayes' rule whenever possible. A formal definition of strategies and beliefs is provided in Appendix C.A.

For any set of parameters, there exists a unique equilibrium. Below, I describe the equilibrium strategies and provide intuition while the full characterization is provided in the Theoretical Appendix.

Lemma 1. *For all parameter values, non-abusive men never exert violence.*

Intuitively, non-abusive men have no incentive to be violent in either period. Because $\alpha^{NA} < 0$, violence has a direct negative effect on their utility. Moreover, Assumption 2 guarantees that the cost of violence is sufficiently high to eliminate any possible strategic benefit. The formal proof is provided in Appendix C.B.

Proposition 1. *In $t = 1$ if there is no violence all women stay. If there is violence women follow a cutoff strategy. A woman stays if and only if her dissolution cost δ is above a threshold, which depends on her motherhood status.*

The formal proof is in Appendix C.C.

Women decide whether to stay or leave in period 1 after observing the realized fertility outcome p_1 and violent behavior v_1. When $v_1 = 0$, women either infer that their partner is non-abusive (in a separating equilibrium) or retain the prior belief q that he may be abusive (in a pooling equilibrium). By Assumption 3, this belief is not high enough to induce exit, so all women stay when no violence occurs.

When $v_1 = 1$, women know their partner is abusive, since non-abusive men never exert violence. They then compare the utility of staying with that of leaving. Staying is optimal only if the dissolution cost is larger than a threshold $\delta^*_{p_1}$, which depends on her motherhood status:

$$\delta^*_1 = \bar{\alpha} - \bar{c} \quad \text{(mothers)}, \qquad \delta^*_0 = \bar{\alpha} - \tfrac{\beta}{1+\beta} \quad \text{(non-mothers)}.$$

By Assumption 1, $\delta^*_1 \leq \delta^*_0$, so mothers are (weakly) more likely to stay than non-mothers after experiencing violence. There are three groups of women. First, those with low dissolution cost ($\delta \leq \delta^*_1$) leave after violence regardless of whether they are mothers or not. Second, those with high dissolution cost ($\delta > \delta^*_0$) stay after violence regardless of motherhood status. Finally, women with intermediate dissolution cost ($\delta^*_1 < \delta \leq \delta^*_0$) stay if they are mothers but leave if they are not. The size of this intermediate group depends on $\bar{c}$. A larger cost of single motherhood lowers the threshold δ^*_1 and increases the share of women who stay once they become mothers.

[9] Intuitively, the probability that the woman leaves when childless ($F(\bar{\alpha} - \frac{\beta}{1+\beta})$) can't be too high. If it's very high, for all parameters of the model, abusive non-fathers would prefer to not reveal their type in the first period.

There is a special case where motherhood no longer affects the decision to stay. In the limit, when $\delta_1^* = \delta_0^*$, mothers and non-mothers are equally likely to stay after violence.

Proposition 2. *In $t = 2$, all abusive men exert violence. In $t = 1$, their strategies depend on two thresholds, α_1^* and α_0^*. If $\alpha^A > \alpha_0^*$, all abusive men exert violence. If $\alpha^A \in [\alpha_1^*, \alpha_0^*]$, abusive fathers exert violence while abusive non-fathers refrain. If $\alpha^A < \alpha_1^*$, all abusive men do not exert violence in the first period.*

Below I present the intuition for the proof. The full derivation is in Appendix C.D.

In period 2, all abusive men exert violence. Women cannot leave in the second period, and violence brings abusive men utility, so they are strictly better off exerting violence.

In period 1, the decision is more involved because men face dynamic incentives to delay violence. The thresholds α_1^* (fathers) and α_0^* (non-fathers) define when each group finds it optimal to reveal their type. Men exert violence whenever their satisfaction from violence exceeds the relevant threshold. Since non-fathers face both a higher risk of separation and an additional incentive to become fathers, their threshold is higher. In other words, non-fathers need higher satisfaction for violence to decide to exert violence in the first period. There are three cases to consider:

Case 1: Unconditional Violence

When $\alpha^A > \alpha_0^*$, the utility gained in period 1 from being violent is high enough to outweigh the dynamic benefits of waiting. All abusive men exert violence in period 1, regardless of fatherhood status.

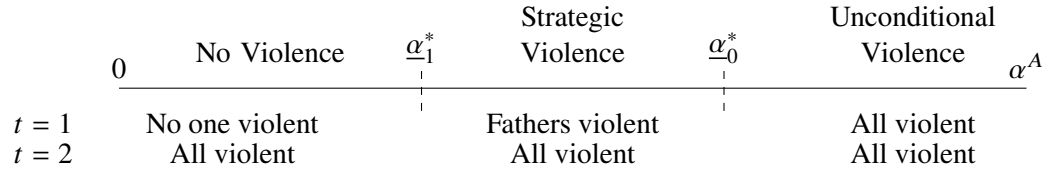
Case 2: Strategic Violence

When $\alpha^A \in [\alpha_1^*, \alpha_0^*]$, only abusive fathers exert violence in period 1, while abusive non-fathers delay. Non-fathers refrain for two reasons. First, non-mothers are (weakly) more likely to leave after violence, so non-fathers face a higher risk of separation if they reveal their type early. Second, men derive positive utility from having a child, so by waiting, they guarantee becoming fathers in the next period.

Case 3: No Violence

When $\alpha^A < \alpha_1^*$, all abusive men, regardless of fatherhood status, refrain from violence in period 1. Intuitively, the risk of separation outweighs the utility gain from violence.

The figure below summarizes the strategy of abusive men as a function of the satisfaction of violence α^A:



Cases of Interest

Combining men's and women's strategies, the values of $\bar{c}$ and α^A give rise to different equilibrium outcomes. Not all of these are informative for the purposes of this paper.[10] I therefore focus on two cases:

[10] I ignore the case α^A is low, since there is no violence in the first period. I also ignore the case α^A is high and the cost of single motherhood is low ($\bar{c} = \frac{\beta}{1+\beta}$), because motherhood is orthogonal to both violence and separation.

1. *Direct channel.* This case arises when violence is strategic ($\alpha^A \in [\alpha_1^*, \alpha_0^*]$). Only abusive fathers exert violence in the first period, while abusive non-fathers delay violence until the second period. Thus, becoming a father directly changes men's violent behavior.[11] The predictions of the model are the same qualitatively for all $\bar{c} \geq \frac{\beta}{1+\beta}$.[12]

2. *Selection channel.* This case arises when two conditions hold: violence is unconditional ($\alpha^A > \alpha_0^*$) and the cost of single motherhood is high ($\bar{c} > \frac{\beta}{1+\beta}$). On the men's side, all abusive men exert violence in the first period, independently of fatherhood status. On the women's side, mothers are strictly more likely than non-mothers to stay in a violent relationship. Together, this corresponds to a situation where motherhood does not affect violent behavior but affects separation decisions.

III.C Predictions

I derive four predictions that distinguish between the two channels. The first three concern period 1 outcomes. Let $V_1^M = P(v_1 = 1 \mid p_1)$ denote the probability that a man exerts violence, conditional on fatherhood status p_1. Let $L_1 = P(v_1 = 1 \mid p_1)F(\delta_{p_1}^*)$ denote the probability of separation.[13] Let $V_1^W = P(v_1 = 1 \mid p_1)(1 - F(\delta_{p_1}^*))$ denote the probability that a woman is exposed to violence at the end of the first period, i.e. that her partner exerts violence *and* she stays in the relationship.

Prediction 4 considers cumulative exposure to violence, conditional on fertility history. Let

$$V^M(h) = P(v_1 = 1 \text{ or } v_2 = 1 \mid h),$$

where h denotes fertility history. I compare two groups: (i) women who became mothers in period 1 (h = Mother at $t = 1$), and (ii) women who did not become mothers in period 1, stayed partnered, and became mothers in period 2 (h = Mother at $t = 2$).

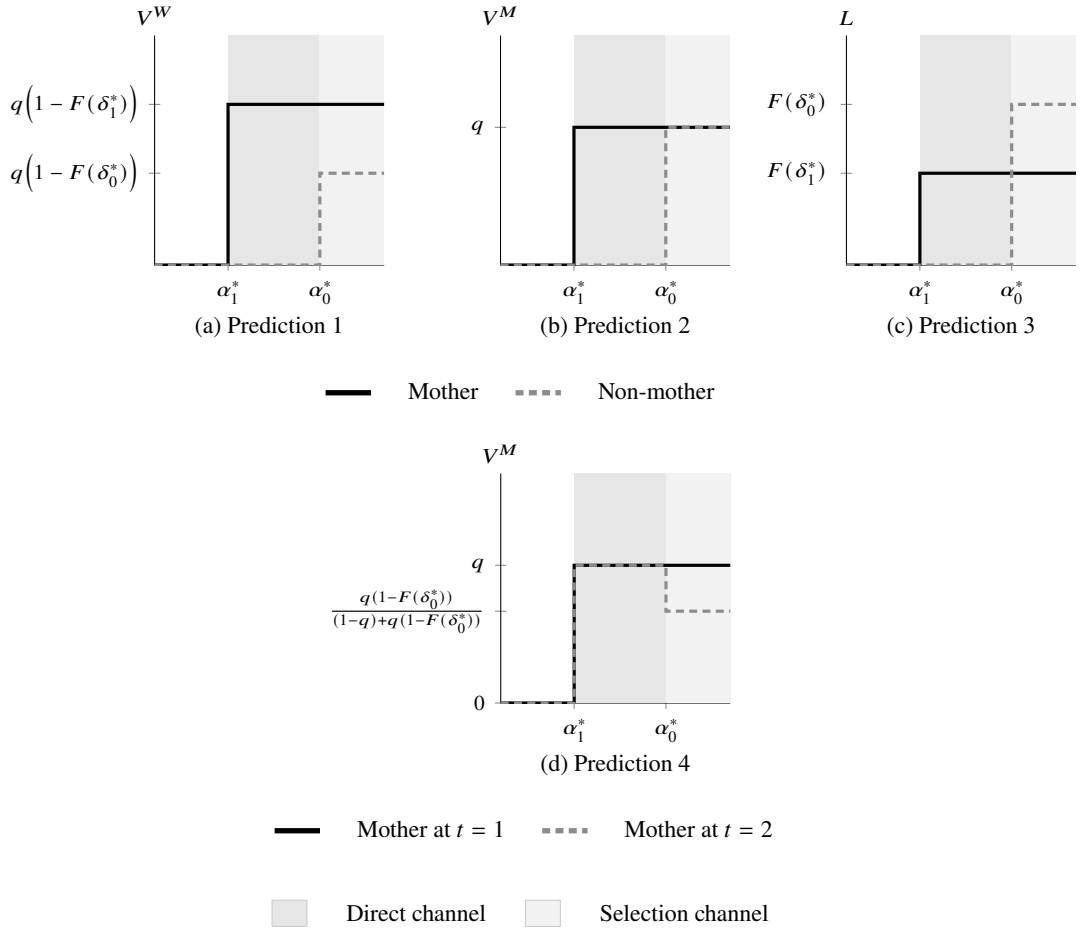
Figure 3 summarizes the four predictions. Panels (a)–(c) show the effects of motherhood on V_1^W, V_1^M, and L_1. Panel (d) compares $V^M(h)$ across the two fertility histories. Darker gray shading corresponds to the values of α^A that generate the direct channel, while lighter gray shading corresponds to the parameter values that generate the selection channel.

P1. Motherhood increases V_1^W under both channels Panel (a) shows that mothers (solid line) are more likely than non-mothers (dashed line) to experience violence under both channels, though for different reasons. Under the direct channel, non-mothers never experience violence because non-fathers do not exert violence, so mothers face higher risk because fathers are more likely to be violent. Under the selection channel, all women are equally likely to have a violent partner (q), but mothers are more likely to remain partnered, so V_1^W is strictly higher for them.

[11] The direct channel can also be interpreted through alternative microfoundations. Instead of strategic incentives to hide type, one could assume state-dependent preferences, where some men derive satisfaction from violence only once they are fathers. Here the interpretation could be that fatherhood increases stress, triggering violent behavior. The model delivers the same qualitative predictions and details are available in Appendix C.E.

[12] If the cost of single motherhood is low ($\bar{c} = \frac{\beta}{1+\beta}$), mothers and non-mothers are equally likely to stay, so motherhood affects only men's behavior. If the cost is high ($\bar{c} > \frac{\beta}{1+\beta}$), mothers are strictly more likely to stay, so motherhood affects both men's violence and women's separation.

[13] In this simple model, women only leave if there is violence, so the probability of separation is defined as the probability of violence times the probability of separation conditional on violence.



Notes: This figure summarizes the model's predictions under two channels. Panels (a)–(c) display: (a) the probability that women experience violence (V^W) at the end of period 1, (b) the probability that men exert violence (V^M) in period 1, and (c) the probability that women leave (L) at the end of period 1. Panel (d) compares the probability of having had a violent partner (V^M) between mothers who have a child in period 1 and those who delay to period 2. The horizontal axis shows men's satisfaction from violence α^A, with thresholds α_1^* (fathers) and α_0^* (non-fathers). Shaded regions indicate parameter ranges: direct channel ($\alpha_1^* \leq \alpha^A \leq \alpha_0^*$) and selection channel ($\alpha^A > \alpha_0^*$). q is the share of violent men, and δ_1^*, δ_0^* are the dissolution-cost thresholds for mothers and non-mothers. Throughout, I assume $\bar{c} > \frac{\beta}{1+\beta}$.

Figure 3: Graphical Representation of the Model's Predictions

P2. Fatherhood increases V_1^M only under the direct channel Panel (b) shows that under the direct channel, fatherhood triggers violent behavior, so mothers are more likely to have a violent partner (a share q) while non-mothers' partners do not exert violence in the first period. Under the selection channel, all abusive men exert violence regardless of fatherhood status, so $V_1^M = V_0^M = q$ is equal across groups.

P3. Motherhood affects L_1 in opposite directions across channels Panel (c) shows that separation is more likely for mothers under the direct channel but less likely under the selection channel. Under the direct channel, non-mothers do not leave because non-fathers do not exert violence. In contrast, some mothers do face violence, and those with dissolution cost below δ_1^* exit, so L_1 is strictly higher for mothers. Under the selection channel, all women are equally likely to have a violent partner, but mothers are less likely to leave because their outside option is lower, so L_1 is lower for mothers.

P4. Delaying motherhood lowers $V^M(h)$ under the selection channel only Panel (d) compares

$V^M(h) = P(v_1 = 1 \text{ or } v_2 = 1 \mid h)$ across fertility histories: women who became mothers in period 1 (solid line) versus those who delayed to period 2 (dashed line). Under the direct channel, delaying does not help because abusive non-fathers wait to reveal their type until after becoming fathers. Thus women who become mothers in period 2 are no more likely to have non-abusive partners than those who became mothers in period 1, implying $V^M(\text{Mother at } t = 1) = V^M(\text{Mother at } t = 2) = q$. Under the selection channel, abusive men reveal their type at the end of period 1, so women who delay and still become mothers in period 2 are a selected group (those with non-abusive partners or those with abusive partners and high dissolution costs). In contrast, women who become mothers in period 1 include some who would have left an abusive partner had they not had a child, but because motherhood raised their exit costs, they stayed. Consequently, $V^M(\text{Mother at } t = 2) < V^M(\text{Mother at } t = 1)$.

The table below summarizes the predictions of the model.

Table 1: Summary of Model Predictions

	Direct channel	*Selection channel*
P1: Violence exposure	↑	↑
P2: Violent partner	↑	=
P3: Separation	↑	↓
P4: Violent partner (delayed)	=	↓

Notes: P1-P3 show the effect of motherhood on each outcome: ↑ increases, ↓ decreases, = no effect. P4 compares women who became mothers early to those who delayed: ↓ indicates that women who delay are less likely to have a violent partner.

IV Testing the Model: Descriptive Evidence

IV.A Measuring the Two Margins of Violence Exposure

The model yields three objects of interest: women's exposure to violence (V^W), men's violent behavior (V^M), and separation (L). Women's exposure and separation are straightforward to measure with standard data. Men's violent behavior is not.

Women's exposure to violence (V^W) is what most IPV data sources capture. Police reports, hospital records, and survey questions about violence in the last twelve months all measure recent violence exposure. Separation (L) is also straightforward to measure, provided the sample includes women of all civil statuses.

The challenge lies in measuring men's violent behavior (V^M). As documented in Section II.B, exposure reflects both whether men use violence and whether women stay in violent relationships. Standard measures of recent violence conflate these two margins. One might think that focusing only on partnered women would solve this problem. If the issue is that exposure conflates violent behavior with separation decisions, then holding separation constant should isolate changes in violent behavior. This approach is common in IPV research: among papers using survey data, more than half either restrict to partnered women or control for civil status (Appendix Table A1).

However, this approach can create sample selection (collider) bias when both motherhood and violence affect separation. Under the selection channel, motherhood raises women's costs of leaving, making mothers less likely to separate. At the same time, violence increases separation, as documented

in Section II.B. Together, these two forces mean that non-mothers who experienced violence are more likely to have left and are therefore excluded from the partnered sample, while mothers who experienced violence are more likely to have stayed. This differential selection means that among partnered women, mothers appear more likely to have violent partners even when the true rate of violent behavior is identical across groups. Appendix D provides a detailed discussion using directed acyclic graphs.

A more informative approach requires data that capture violence with both current and former partners. For separated women, asking about violence in the last twelve months captures only current exposure, which mechanically drops to zero after leaving. Asking whether the partner was ever violent allows both partnered and separated women to report violence, providing a measure that does not mechanically decrease with separation.

Therefore, the data need two key features. First, the sample must include women of all civil statuses to measure separation and avoid sample selection bias. Second, the data must ask women about violence with current and former partners, both in the last twelve months and at any point in the relationship, to construct proxies for both violence exposure and men's violent behavior.

With this data, women's exposure to violence (V^W) can be proxied by whether a woman experienced physical violence in the last twelve months. Men's violent behavior (V^M) can be proxied by whether a partner was ever physically violent during the relationship. Separation (L) can be proxied by whether a woman is currently separated. The "ever violent" measure is not a perfect proxy for men's underlying propensity to use violence, but it remains observable even after separation and provides a first step toward distinguishing men's violent behavior from women's separation decisions.

IV.B First Partner Sample

ENDIREH has the two features needed to test the model's predictions. I begin by testing the model's predictions using a sample of women from the general population. The purpose of this section is to document the equilibrium correlations implied by the theory. Since motherhood is endogenous, these estimates should not be interpreted as causal effects. In Section V, I turn to an instrumental variables strategy that uses a fertility shock to estimate the causal impact of motherhood on violence and separation.

The analysis focuses on women who began their first cohabiting relationship within the last eight years and who had their first child after the start of cohabitation. I restrict attention to the early years of the relationship because most first births occur at this stage. About half of women become mothers within the first year and 91 percent within eight years.[14] Restricting to first unions minimizes bias from re-partnering and brings the sample closer to the setting of the model.

The survey collects detailed information on violence for a woman's current partner if she is partnered and for her last partner if she is separated. For this partner (reference partner), women are asked about different types of violence (e.g., slapping, pushing).[15] They are first asked whether the reference partner committed that type of violence at any point in the relationship and, if they answer affirmatively, whether the violence occurred in the last twelve months. In addition, the survey asks a general question about whether any other previous partner ever committed acts of physical violence.

From these questions, I construct three measures that correspond to the outcomes of the model. Women's exposure to violence, V^W, equals one if she experienced any act of physical violence by her

[14] After fifteen years about 94 percent of women have had a child, so the first eight years capture the majority of first births.
[15] See Appendix E.A for the full list of questions.

first partner in the last twelve months. Men's violent behavior, V^M, equals one if the first partner was ever physically violent. Separation, L, equals one if the woman is not cohabiting with her first partner at the time of the survey.[16]

I test predictions 1-3 of the model with the following regression:

$$Y_i = \alpha + \beta \text{Mother}_i + X_i + \epsilon_i \tag{1}$$

where the outcome variable Y_i is women's exposure to violence V^W, men's violent behavior V^M, or separation L, and $Mother_i$ is a binary variable equal to one if the woman became a mother with her first partner. I control for the following fixed effects: age, time since cohabitation, wealth quintile and school level.

To test prediction 4, I restrict the sample to women who had a baby with their first partner and estimate:

$$V_i^M = \alpha + \beta \text{Wait}_i + X_i + \epsilon_i \tag{2}$$

where $Wait_i$ equals one if the woman had a child at least four years after she began dating her first partner. I use the same controls as above and add time since first child fixed effects.

I now turn to the results of testing the four predictions of the model, summarized in Table 2.

Prediction 1. Motherhood and women's exposure to violence (V^W). Prediction 1 says that both channels imply an increase in women's exposure to violence. Consistent with this, Column (1) shows that mothers are 1.6 percentage points more likely to experience physical violence in the last twelve months than non-mothers. Relative to the 5.3 percent baseline among non-mothers, motherhood is associated with a 30 percent increase in the probability of exposure.

Prediction 2. Motherhood and men's violent behavior (V^M). Prediction 2 says that under the direct channel, mothers should be more likely to have had a violent partner, since fatherhood triggers violence. Under the selection channel, there should be no difference, since men's behavior is unchanged by fatherhood. Column (2) shows that mothers and non-mothers are equally likely to have had a violent partner. The coefficient is a precisely estimated zero, consistent with the selection channel.

Prediction 3. Motherhood and separation (L). Prediction 3 says that under the direct channel, mothers are more likely to separate, since fatherhood triggers violence which leads some women to leave. Under the selection channel, mothers are less likely to separate, since motherhood raises exit costs. Column (3) shows that mothers are 12.2 percentage points less likely to be separated from their first partner than non-mothers. Since 18.8 percent of non-mothers are separated, this corresponds to a 65 percent lower likelihood of separation among mothers.[17] This supports the selection channel.

Prediction 4. Waiting and men's violent behavior (V^M). Prediction 4 says that under the direct

[16] Separation from the first partner can be identified for all women in the sample, and the violence variables can be identified for 98.1 percent. See Appendix E.B for details.

[17] This also holds when conditioning on violence. Among women with a violent partner, mothers are 24.1 percentage points less likely to be separated than non-mothers. Nearly half of non-mothers who experience violence are separated (52.7 percent), so mothers in violent relationships are 46 percent less likely to separate than comparable non-mothers.

channel, women who wait to have a child should be equally likely to have a violent partner as those who do not. Abusive men delay violence until they become fathers, so waiting provides no additional information about a partner's type. Under the selection channel, women who delay motherhood are disproportionately those with non-violent partners, so waiting should be associated with a lower likelihood of having a violent partner. Column (4) shows that mothers who wait at least four years to have a child are 3.8 percentage points less likely to have experienced violence with their partner. On average, 14.8 percent of early mothers have a violent partner, so waiting is associated with a 26 percent lower probability of violence. This is consistent with the selection channel.

Table 2: Predictions of the Model Using the Sample of First Cohabitation

	V^W	V^M	L	V^M
	(1)	(2)	(3)	(4)
Mother	0.016***	-0.007	-0.122***	
	(0.006)	(0.008)	(0.010)	
Wait 4+ years				-0.038***
				(0.009)
Observations	32,338	32,338	32,338	24,966
Mean non-mothers	0.053	0.099	0.188	0.148
Age fixed effects	✓	✓	✓	✓
Wealth quintile fixed effects	✓	✓	✓	✓
School level fixed effects	✓	✓	✓	✓
Years since start cohab. fixed effects	✓	✓	✓	✓
Years since first child fixed effects				✓

Notes: Columns (1)–(3) report the coefficient β from Equation 1. The sample includes women who began their first cohabiting relationship less than eight years before the survey and who had their first child after the start of cohabitation. V^W equals one if the woman experienced physical IPV from her first partner in the last 12 months; V^M equals one if she ever experienced physical IPV with her first partner; L equals one if she is separated from her first partner; and *Mother* equals one if she had a child with her first partner. Column (4) reports the coefficient β from Equation 2, estimated on the sample of women who had their first child with their first partner. *Wait 4+ years* equals one if the woman waited at least four years after the start of the relationship to have her first child. All regressions include fixed effects for age, wealth quintile, schooling, and years since the start of cohabitation. Column (4) additionally includes fixed effects for years since the first child. Regressions are weighted using survey weights, and heteroskedasticity-robust standard errors are reported in parentheses. Significance levels: *** 0.01, ** 0.05, * 0.10.

These results are consistent with the selection channel. Before turning to causal evidence in Section V, I demonstrate why including women of all civil statuses is essential for this conclusion.

IV.C Sample Selection Bias in Practice

As discussed in Section IV.A, conditioning on civil status can generate spurious correlations between motherhood and violence when separation is endogenous to both. I now demonstrate this using the first partner sample. Recall from Column (2) of Table 2 that mothers and non-mothers are equally likely to have had a violent partner when the full sample is used. Table 3 shows that this result reverses when civil status is conditioned on. Column (1) adds separation as a control variable: mothers now appear 2.2 percentage points more likely to have a violent partner (p = 0.004). Column (2) restricts to partnered

women: mothers appear 1.5 percentage points more likely to have a violent partner (p = 0.039).

This reversal is mechanical: because violence increases separation while motherhood reduces it, conditioning on being partnered disproportionately excludes non-mothers who experienced violence and left, generating a spurious positive correlation between motherhood and violent behavior. By using these approaches, one would incorrectly conclude that motherhood increases men's violent behavior, consistent with the direct channel. This bias is not specific to motherhood. Any treatment that affects separation faces the same identification problem when samples are restricted to partnered women or control for civil status.

All four results line up with the selection channel. Mothers are more likely to be exposed to violence, but equally likely to have had a violent partner, and they are significantly less likely to separate. Moreover, mothers who delay childbirth are less likely to have had a violent partner. These descriptive results are consistent with a simple model where some men are abusive types and motherhood raises the cost of separation. While they cannot establish causality, they show that the model can account for the equilibrium correlations observed in the data. In the next section, I turn to an instrumental variables strategy that exploits a fertility shock to move from correlations to causal estimates.

Table 3: Sample Selection Bias from Conditioning on Civil Status

	Violent partner V^M	
	(1)	(2)
Mother	0.022***	0.015**
	(0.008)	(0.007)
Observations	32,338	28,017
Mean non-mothers	0.099	0.058
Separation fixed effects	✓	
Age fixed effects	✓	✓
Wealth quintile fixed effects	✓	✓
School level fixed effects	✓	✓
Years since start cohab. fixed effects	✓	✓

Notes: This table replicates Column (2) of Table 2 with alternative sample restrictions. Column (1) controls for separation. Column (2) restricts to partnered women. See Table 2 notes for variable definitions and controls. Heteroskedasticity-robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

V Testing the Model: Causal Evidence

To obtain causal estimates of the effect of motherhood on violence and separation, I need exogenous variation in the likelihood that a woman becomes a mother in the short term. Canonical fertility instruments capture variation at the intensive margin, for example the mix-sibling sex composition (Angrist and Evans, 1998) or having twins (Rosenzweig and Wolpin, 1980). In contrast, my focus is on the extensive margin, which requires an instrument for whether a woman becomes a mother in the first place.

Different types of policies could increase the share of couples who do not become parents. Examples

include changes in access to abortion, campaigns that provide information about the benefits of delaying pregnancy, or improvements in access to contraceptives. In this paper, I use changes in access to pharmacies as a natural experiment. The hypothesis is that pharmacies affect contraceptive use, which in turn decreases the probability of becoming a mother.

V.A Contraceptive Use and Pharmacies in Mexico

Contraceptive use in Mexico is low. Using data from the 2009 and 2014 rounds of the Encuesta Nacional de la Dinámica Demográfica (ENADID), I find that 46 percent of women aged 15 to 24 report not using contraception during their first sexual encounter. The leading reasons are unplanned sexual activity, desire to have a child, limited knowledge about contraceptives or where to obtain them and a low perceived risk of pregnancy. Very few say that their partner is opposed to contraception or that they have personal beliefs against it (see Figure A3 in the Appendix).

Among women who do use contraceptives, pharmacies are a major source of access, particularly for non-mothers. Figure A4 in the Appendix shows that 76.3 percent of non-mothers who use contraceptives obtain contraceptives from a pharmacy, compared with only 10.5 percent of mothers. This pattern reflects differences in contraceptive methods. Non-mothers primarily use short-term methods available in pharmacies, such as condoms, pills, and injectables, while mothers rely on long-term methods like sterilization and IUDs that require clinic visits (see Figure A5 in the Appendix).

In Mexico, pharmacies sell contraceptives without requiring a prescription, which lowers barriers to access. Yet one may wonder why pharmacies are preferred over public health system services, which provide some contraceptives for free. To understand this, I conducted ten qualitative interviews with pharmacists. Two main reasons emerged. The first is convenience. Pharmacies are easier to access, with shorter waiting times and more flexible hours.[18] The second is privacy. In public clinics, doctors keep records of contraceptive use and require personal information. This can discourage young or unmarried women from seeking contraceptives, particularly emergency contraception.[19]

Hence, pharmacies are the most common source of contraceptive access for non-mothers, likely because they are available without prescription and because pharmacies offer accessibility and privacy. Moreover, contraceptives represent only a small fraction of the products sold in pharmacies. Although there is no official statistic on this, I calculated the share of sexual health products, which include contraceptives, in the total number of items sold by three of the largest pharmacy chains in Mexico: Farmacias del Ahorro, Farmacias Benavides, and Farmacia San Pablo. The share of sexual health products is 3.2, 2.8, and 0.7 percent, respectively.[20] This makes it unlikely that pharmacy openings respond to local fertility or contraception demand. These patterns motivate the use of local pharmacy access as a source of exogenous variation in fertility.

[18] Large chains such as Farmacias Similares, the largest in Mexico, are open until 22:00, and some locations operate 24 hours.

[19] One pharmacist explained, "In the clinic, the doctor keeps control of the methods the woman should use. For example, young women who want the morning-after pill may be ashamed to go to the doctor. Or people who are not married prefer to come to the pharmacy. Once they get married, they go to the public center."

[20] These figures were calculated using data from the pharmacies' official websites.

V.B Calculating Access to Pharmacies

For the main analysis, I use waves 2011 and 2016 of ENDIREH. I was able to access the confidential location of the women on ENDIREH at the block level through INEGI's *Laboratorio de Microdatos*. To construct a measure of access to pharmacies, I combine two additional data sources: (i) the *Directorio Estadístico Nacional de Unidades Económicas* (DENUE), which contains the universe of businesses in Mexico, and (ii) block-level maps from INEGI's *Marco Geoestadístico*.

The DENUE provides detailed information on all active establishments in Mexico, including their name, location, economic activity, and size. The first edition was released in 2010 and has been updated annually since then.[21] Establishments are classified using the North American Industrial Classification System for Mexico (NAICS). I use two relevant categories: pharmacies without mini-supermarkets (464111) and pharmacies with mini-supermarkets (464112). The former sell only pharmaceutical products, while the latter also sell food, beverages, and household goods. I use DENUE 2011 and 2016 to construct the universe of pharmacies in those years. I apply the same procedure to identify other types of businesses in 2011 and 2016, which I later use in placebo analyses.

For spatial reference, I use the 2010 version of INEGI's *Marco Geoestadístico*, which provides shapefiles for all 32 Mexican states at the block level. This version aligns with the sampling design of ENDIREH 2011 and 2016.

Using these data, I compute several measures of geographic access. For each woman, I identify the centroid of the block where she lived at the time of the survey and measure the distance to the nearest pharmacy in 2011 and 2016. I repeat this procedure for other types of businesses (for placebo estimations). In addition, I construct a "market access" measure defined as the number of businesses within a 3 km radius of the block centroid in 2011 and 2016. Appendix E.C provides further details on the procedure and confidentiality measures.

The resulting dataset contains measures of access to pharmacies and other businesses of interest for each woman in the survey in 2011 and 2016.

V.C Causal Effect of Pharmacy Openings

Empirical strategy

I test the first three predictions of the model using a difference-in-differences framework. There are two periods (2011 and 2016) and two groups (treatment and control). The treatment group consists of women living in blocks that had no pharmacy within a one-kilometer radius in 2011 but gained access to at least one by 2016, while the control group includes women in blocks without a pharmacy within one kilometer in either year. One kilometer corresponds to a 12-15 minute walk each way (about 25-30 minutes round trip). Section V.E shows that alternative distance thresholds yield similar results.

The model focuses on the extensive margin of motherhood, that is, the decision of whether to have a first child. To align the empirical analysis with this focus, I restrict the sample to women under 30 years old who were not mothers five years before the survey.[22] The first restriction is not very limiting,

[21] The most comprehensive updates occur after each Economic Census (2009, 2014, 2019). Although large firms are updated between census years, smaller establishments such as pharmacies typically enter the registry only after each census.

[22] The survey asks women the age at which they had their first child. Using this information, I restrict the sample to women who were not mothers five years prior to the survey: for the 2016 wave, those who were not mothers in 2011; for the 2011 wave, those who were not mothers in 2006.

as Mexican women tend to become mothers early in life. The average age at first birth is 21 years (INMUJERES, 2020) and 63 percent of women are mothers at 25 years, rising to 78 percent by age 30. The sample therefore captures the vast majority of first-time mothers. In Section V.E, I show that the results are similar when using alternative age thresholds. The second restriction ensures that all women in the sample were plausibly childless before exposure to a pharmacy opening.[23]

Table A2 in the Appendix reports the summary statistics. By construction, women in the sample are young, with an average age of around 20 years. They have 9.9 years of schooling, which is slightly below the national average for women of similar ages.[24] Around 27 percent are employed at the time of the survey, and 8 percent identify as Indigenous. Access to pharmacies increased substantially for women in treated areas. In 2011 the average distance to the nearest pharmacy was about two kilometers, while in 2016 it was around 500 meters. This means that the round trip to the nearest pharmacy shortened from roughly 40 to 60 minutes to 10 to 15 minutes. In contrast, women in the control group did not experience any meaningful change in access, with an average distance of about five kilometers in both 2011 and 2016.

There are four outcome variables needed to test the model's predictions: motherhood, women's exposure to violence (V^W), men's violent behavior (V^M), and separation (L). In the first-partner analysis, all variables could be measured directly with respect to the first partner, but in this section the limited relationship histories in ENDIREH make that difficult. Ideally, each outcome would be measured with respect to the partner from before the pharmacy opening, five years prior to the survey, but information on earlier partners is often incomplete. As explained in Section IV, ENDIREH provides detailed information for the reference partner,[25] but information on earlier partners is much more limited and results in missing data for many women.

Fortunately, for motherhood, women's exposure to violence, and separation, I can construct reliable proxies without relying on full relationship histories. Motherhood equals one if the respondent is a mother at the time of the survey. Women's exposure to violence (V^W) equals one if she experienced physical violence in the last twelve months from her reference partner. Separation (L) equals one if a woman is not currently partnered but has previously been in a relationship, including those who report being separated from a cohabiting partner or single with an ex-boyfriend.[26]

By contrast, identifying men's violent behavior requires information about the partner from before the pharmacy opening, since this must capture men's propensity to exert violence independently of women's separation decisions. I therefore construct a binary variable equal to one if that pre-treatment partner was ever physically violent during the relationship. Based on the available relationship histories, I can identify the partner from five years earlier for 63 percent of the sample.[27] The remaining cases are women who started the relationship with their reference partner within the last four years, so I cannot

[23] Sampling frames differ across ENDIREH waves. I harmonize the 2011 and 2016 samples to make them comparable across periods and replicate the 2006 sampling design for the placebo analysis. Appendix E.D describes the sampling designs in detail.

[24] According to the 2020 Census, women aged 15 to 19 had on average 9.8 years of schooling, and women aged 20 to 24 had 11.5 years.

[25] The reference partner is defined as the current partner for women in a cohabiting or dating relationship and the most recent partner for separated or single women.

[26] This measure likely understates true separation rates because some women may have re-partnered by the time of the survey.

[27] This includes women who are currently in a relationship that began more than five years ago, women who are single or separated but were with their reference partner five years ago, and women who have never been in a relationship.

identify whether they were in another relationship five years ago. I impute zeros for these women as a first step for two reasons. First, relationship formation typically involves a gap between partners, so many of them were likely single five years earlier. Second, using information on the start date of each woman's first cohabiting relationship, I identify that 97.7 percent of these women were not cohabiting at that time, meaning they were either single or in a dating relationship. Among women with ex-boyfriends, only 4.7 percent report ever experiencing physical violence, compared with over 40 percent among women separated from a cohabiting partner. Thus, even if they were in a relationship, very few would have experienced violence. In Section V.E, I relax this assumption and construct lower and upper bounds based on more conservative imputations, and the main results remain similar.

Table A2 in the Appendix shows summary statistics for these variables. 37% of women were mothers at the time of the survey. Around 4% of women reported experiencing physical violence in the last 12 months, and 3% experienced physical violence from a partner five years prior to the survey. Lastly, 24% fall into the separation category.

I estimate the following specification for each outcome variable of interest:

$$Y_{ibt} = \beta_0 + \beta_1 \text{Treat}_b + \beta_2 \text{Post}_t + \beta_3 \text{Post}_t \times \text{Treat}_b + X_{ibt} + \epsilon_{ibt} \tag{3}$$

The variable Post_t is a dummy variable that takes the value of 1 if the observation is from 2016 and 0 otherwise. The variable Treat_b equals one if the block had no pharmacy within one kilometer in 2011 but had at least one by 2016. The vector X_{ibt} contains a set of control variables including age fixed effects, years of schooling, change in market access[28], type of locality (urban/rural) fixed effects, wealth quintile fixed effects, and state fixed effects. All standard errors are clustered at the AGEB level, the basic statistical unit of INEGI.[29] The coefficient of interest, β_3, captures the causal effect of gaining access to a nearby pharmacy on the outcome variable of interest.

Treatment and control areas differ in some observable characteristics, such as baseline distance to the nearest pharmacy in 2011 and the share of working women. Covariate balance, however, is not required for a difference-in-differences design. The key identifying assumption is that, in the absence of pharmacy openings, fertility, IPV, and separation would have evolved similarly across groups. To provide evidence consistent with this assumption, I conduct a falsification exercise using data from ENDIREH 2006 and 2011. Specifically, I estimate Equation 3 on this earlier period, defining $Post_t$ as an indicator for 2011. Table 4 shows no evidence of differential pre-trends between treatment and control areas in fertility, violence, or separation outcomes.

[28] Defined as the number of businesses in a 3km radius in 2016 minus the number of businesses in a 3km radius in 2011

[29] The geo-statistical framework divides the country into states, municipalities, localities, and AGEBs (basic geostatistical unit). Urban AGEB's include between one and fifty blocks and are perfectly delimited by streets, avenues, or any other trait easily identifiable in the field. Rural AGEB's are more variable in size and are delimited by natural features (e.g. river, ravine) or other features (e.g. roads, pipelines, railways)

Table 4: Pretrend Analysis

	Mother (1)	Violence Exposure (2)	Violent Partner (3)	Separation (4)
Post × Treat	-0.022	0.001	-0.001	-0.024
	(0.037)	(0.019)	(0.010)	(0.041)
Observations	9,199	9,199	9,199	9,199
Mean Dep.Var	0.297	0.030	0.017	0.229
Age fixed effects	✓	✓	✓	✓
Rural/Urban fixed effects	✓	✓	✓	✓
State fixed effects	✓	✓	✓	✓
Wealth Quintile fixed effects	✓	✓	✓	✓
School Years	✓	✓	✓	✓
Δ Mkt. Access	✓	✓	✓	✓

Notes: Each column reports the coefficient β_3 from Equation 3, estimated using data from ENDIREH 2006 and 2011. The sample includes women aged 15–30 who were not mothers five years before the survey. *Mother* equals one if the woman is a mother at the time of the survey. *Violence Exposure* equals one if the woman experienced physical intimate partner violence in the last 12 months. *Violent Partner* equals one if the woman experienced physical violence at any point in the relationship with her partner from five years before the survey. *Separation* equals one if the woman is separated or single with an ex-boyfriend at the time of the survey. Treated women are those who did not have a pharmacy within one kilometer in 2011 but had at least one in 2016; the control group includes women who did not have a pharmacy within one kilometer in either year. *Post* equals one if the survey year is 2011. All regressions control for years of schooling and change in market access, and include age, state, rural/urban, and wealth quintile fixed effects. Regressions are weighted using survey weights, and standard errors clustered at the AGEB level are reported in parentheses. Significance levels: *** 0.01, ** 0.05, * 0.10.

Results

I now turn to the effects of pharmacy openings on motherhood, violence, and separation. I begin by showing that access to a nearby pharmacy significantly reduces the likelihood of becoming a mother. I then show that the effects on women's exposure to violence, men's violent behavior, and separation closely parallel the correlations found in the first partner analysis, providing evidence consistent with the separation channel.

Table 5 summarizes the estimated coefficients β_3 from Equation 3 for all four outcomes. Each estimate comes from the full specification, which includes years of schooling, changes in market access, as well as age, wealth-quintile, state, and urban–rural fixed effects. The complete regression tables for each outcome are reported in Appendix Tables A3, A4, A5, and A6.

In the model, a fraction of couples exogenously delay parenthood. The first empirical step, therefore, is to verify that improved access to pharmacies enables women to postpone childbirth. Column (1) of Table 5 reports the estimate of β_3 from Equation 3 using as the dependent variable a binary indicator equal to one if the woman is a mother and zero otherwise. Access to a nearby pharmacy reduces the probability of being a mother by 9.5 percentage points, a 24.4 percent decline relative to the implied counterfactual.[30] This finding suggests a substantial unmet demand for contraceptive access among Mexican women and indicates that improved access to pharmacies enables greater control over fertility decisions.

[30]The counterfactual is computed for treated women in 2016 by subtracting the estimated effect of *Post* × *Treat* from their predicted probability under treatment, holding covariates and fixed effects constant.

Table 5: Effect of Pharmacy Access on Motherhood, Violence and Separation

	Mother (1)	Violence Exposure (2)	Violent partner (3)	Separation (4)
Post × Treat	-0.095**	-0.042**	0.004	0.071*
	(0.038)	(0.018)	(0.017)	(0.040)
Observations	8,635	8,635	8,635	8,635
Mean Dep.Var	0.366	0.042	0.029	0.238
Counterfactual	0.388	0.080	0.034	0.253
Age fixed effects	✓	✓	✓	✓
Rural/Urban fixed effects	✓	✓	✓	✓
State fixed effects	✓	✓	✓	✓
Wealth Quintile fixed effects	✓	✓	✓	✓
School years	✓	✓	✓	✓
Δ Mkt. Access	✓	✓	✓	✓

Notes: Each column reports the coefficient β_3 from Equation 3. The sample includes women aged 15–30 who were not mothers five years before the survey. *Mother* equals one if the woman is a mother at the time of the survey; *Violence Exposure* if she experienced physical IPV in the last 12 months; *Violent Partner* if she experienced physical IPV at any point in the relationship from five years before the survey; and *Separation* if she is separated or single with an ex-boyfriend. Treated women are those who did not have a pharmacy within one kilometer in 2011 but had at least one in 2016; the control group includes women who did not have a pharmacy within one kilometer in either year. *Post* equals one if the survey year is 2016. AAll specifications control for years of schooling, change in market access, and age, wealth-quintile, state, and urban–rural fixed effects. Market access is measured as the change in the number of businesses within three kilometers between 2011 and 2016. Counterfactual probabilities show predicted outcomes for treated women in 2016 had pharmacies not opened, obtained by subtracting the *Post* × *Treat* effect from their predicted probability under treatment, holding covariates and fixed effects constant. All regressions are weighted using survey weights, and standard errors clustered at the AGEB level are reported in parentheses. Significance levels: *** 0.01, ** 0.05, * 0.10.

Prediction 1: Motherhood and women's exposure to violence (V^W)

Given that a pharmacy opening decreases the probability of being a mother, we would expect a negative coefficient for violent exposure under both channels. The direct channel predicts that fewer men become parents and exert violence. The selection channel predicts that fewer women become mothers, so more women are able to leave, thereby reducing the risk of violence. Consistent with either channel, Column (2) of Table 5 shows that access to a nearby pharmacy reduces the probability of experiencing physical violence in the last 12 months by 4.2 percentage points, a 52 percent decline relative to the implied counterfactual.

Prediction 2: Motherhood and men's violent behavior (V^M)

The theory yields different predictions about the effect of a pharmacy opening on men's violent behavior. Under the direct channel, becoming a parent triggers violence, so we would expect a negative coefficient. Under the selection channel, the effect should be zero, as abusive men are equally likely to exert violence regardless of whether they become parents. Column (3) of Table 5 reports the estimate of β_3 using as the outcome a binary indicator equal to one if the partner from five years prior to the survey was violent at any point in the relationship. A pharmacy opening has no effect on this measure of men's violent behavior (see measurement discussion and bounds in Section V.E). This result is consistent with the selection channel, in which parenthood does not cause men to become violent.

Prediction 3: Separation Behavior (L)

The model yields opposing predictions regarding the effect of a pharmacy opening on separation. Under the direct channel, separation should fall. Having a child reveals a man's abusive type, which leads to break-up, so when fewer men become parents, fewer women discover an abusive type. Under the selection channel, women who delay motherhood learn about a partner's abusive behavior before having a child, which allows them to leave. This channel predicts a positive effect on separation. Column (4) of Table 5 reports the estimate of β_3 using a binary indicator equal to one if the woman is not currently in a relationship but has previously been in one. Access to a nearby pharmacy increases the probability of separation by 7.1 percentage points, a 28.2 percent increase relative to the implied counterfactual. This result is consistent with the selection channel.

To better understand how pharmacy openings affect relationship formation and dissolution, I next examine the full set of civil status outcomes.[31] Appendix Table A7 reports the effects on the disaggregated indicators. The increase in separation is driven by a higher probability of being single with an ex-boyfriend rather than by separations from cohabiting unions. Pharmacy openings also reduce the probability of cohabitation, which suggests that improved access to contraception lowers the likelihood that dating relationships transition into cohabiting unions.

Ruling out alternative explanations

Given that contraceptives are one among hundreds of products sold in pharmacies, it is unlikely that pharmacy openings target areas based on specific trends in fertility, violence, or separation. However, contemporaneous shocks correlated with pharmacy openings could affect outcomes. Specifically, a pharmacy opening could be correlated with regional economic growth, which might affect fertility, violence, and separation, potentially violating parallel trends.

If regional shocks were driving the results, we would expect to find similar effects from the openings of other types of businesses. To test this, I focus on businesses that expanded at a rate comparable to pharmacies, which grew by 21% between 2011 and 2016.[32] The resulting set includes bakeries, candy shops, car workshops, car parts stores, dentists, hardware stores, pawnshops, and poultry shops.

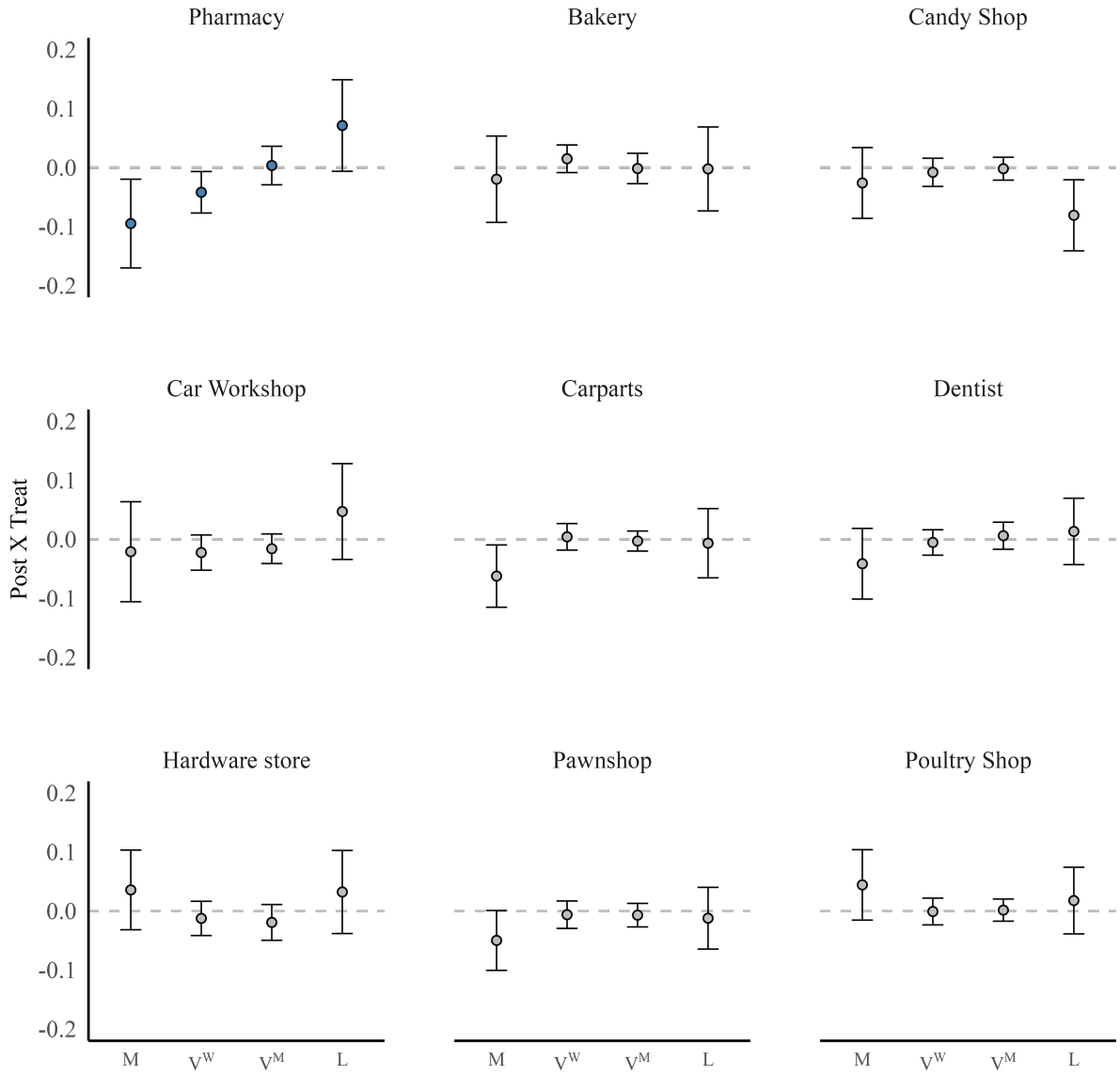
For each business type, I define treatment as living within one kilometer of that business in 2016 but not in 2011, and the control group as women who had no such business within one kilometer in either year. I restrict the sample to women who did not gain access to a pharmacy between 2011 and 2016, to isolate the effect of each business opening among women unexposed to pharmacies. The specification and controls are identical to those used in the main analysis.

Figure 4 plots the estimated coefficients on the interaction between treatment and the post-period for each business type, along with 95% confidence intervals. The top-left panel shows the results for pharmacies, which were discussed above, while the remaining panels display the corresponding estimates for other business openings. Across all outcomes, coefficients for non-pharmacy openings cluster around zero, with no consistent pattern or significant effects. Two out of thirty-two coefficients is significant at

[31] I exclude widowhood as an outcome because only five women in the main sample were widowed at the time of the survey.

[32] The selection followed a two-step procedure. First, businesses had to (1) be sufficiently prevalent in the data to ensure that women in ENDIREH could have access to them, and (2) display a growth rate similar to that of pharmacies, ensuring that the treated group would be sufficiently large. Specifically, I selected business types with at least 30,000 establishments in 2011 (compared to 46,341 pharmacies) and a growth rate of at least 15% (compared to 21% for pharmacies). In the second step, after computing access to each business, I retained only those with at least as many treated women as in the pharmacy analysis. Businesses that did not meet this second criterion include beauty salons, religious centers, restaurants, and tortillerías, which had very few treated women. See Table A8 in the Appendix for further details.

the 5% level and three at the 10%, which is expected given the large number of coefficients.[33] These results suggest that the results are particular to pharmacies and not the effects of a business opening in general.



Notes: Each panel reports difference-in-differences estimates of the effect of gaining access to a nearby business within one kilometer between 2011 and 2016. The top-left panel shows the main results for pharmacy openings, and the remaining panels report placebo estimates for other business types. For the placebo analyses, women who gained access to a pharmacy are excluded. Treated women are those who lacked a given business within one kilometer in 2011 but gained access by 2016; the control group includes women without access in both years. M equals one if the woman is a mother at the time of the survey, V^W equals one if she experienced physical IPV in the last 12 months, V^M equals one if her partner from five years before the survey ever exerted violence, and L equals one if the woman is separated or single with an ex-boyfriend. All regressions control for years of schooling and change in market access, and include age, wealth quintile, urban-rural, and state fixed effects. Regressions are weighted using survey weights, and standard errors are clustered at the AGEB level.

Figure 4: Other Business Openings

[33] Under the null hypothesis of no effect, we would expect 1.6 rejections at the 5% level (32×0.05) and 3.2 at the 10% level (32×0.10).

Additionally, this exercise helps alleviate concerns about migration. Treatment assignment is based on the woman's location at the time of the survey, as residential histories are not observed. Consequently, some women may have moved between 2011 and 2016. First, if migration is unrelated to pharmacy openings, it would introduce noise in treatment classification, biasing the estimated effects toward zero. Second, if migration were selective, such that women with lower fertility, lower exposure to violence, or higher separation rates systematically moved to growing areas, this pattern would also appear in the placebo estimations for other business openings. Lastly, pharmacy openings could bias the results if they induced residential mobility through changes in separation. However, this is unlikely to be the case, since pharmacies mainly prevent single women from entering cohabiting relationships rather than prompting women to leave them (see Appendix Table A7). As these women were not living with their partners, the observed effects are unlikely to be biased by changes in residence.

V.D Causal Effect of Motherhood on Violence and Separation

To estimate the causal effect of motherhood on the variables of interest, I use an instrumented difference-in-differences strategy. Specifically, I scale the DD effect on violence and separation by the DD effect on motherhood[34]. Estimates of Equation 3 on the binary motherhood variable provide evidence of the effect of pharmacy openings on the likelihood of becoming a mother but they also serve as the first stage in a two-stage least squares (2SLS) estimation of the impact of motherhood on violence and separation. I run the following regression:

$$Y_{ibt} = \beta_0 + \beta_1 \text{Treat}_b + \beta_2 \text{Post}_t + \beta_3 \widehat{Mother}_{ibt} + X_{ibt} + \epsilon_{ibt} \tag{4}$$

The variable $\widehat{Mother}_{ibt}$ is the predicted probability of motherhood instrumented by the interaction between treatment and post-period indicators. It therefore captures the variation in motherhood explained by exposure to a pharmacy opening. The outcome variables are: (i) whether a woman experienced physical violence in the last 12 months, (ii) whether her partner from five years before the survey was ever physically violent, and (iii) whether the woman has an ex-partner or ex-boyfriend at the time of the survey. I include the same controls as in Equation 3 and cluster standard errors at the AGEB level.

Conditional on parallel trends holding, we need two additional assumptions: monotonicity and the exclusion restriction (Hudson et al., 2017).

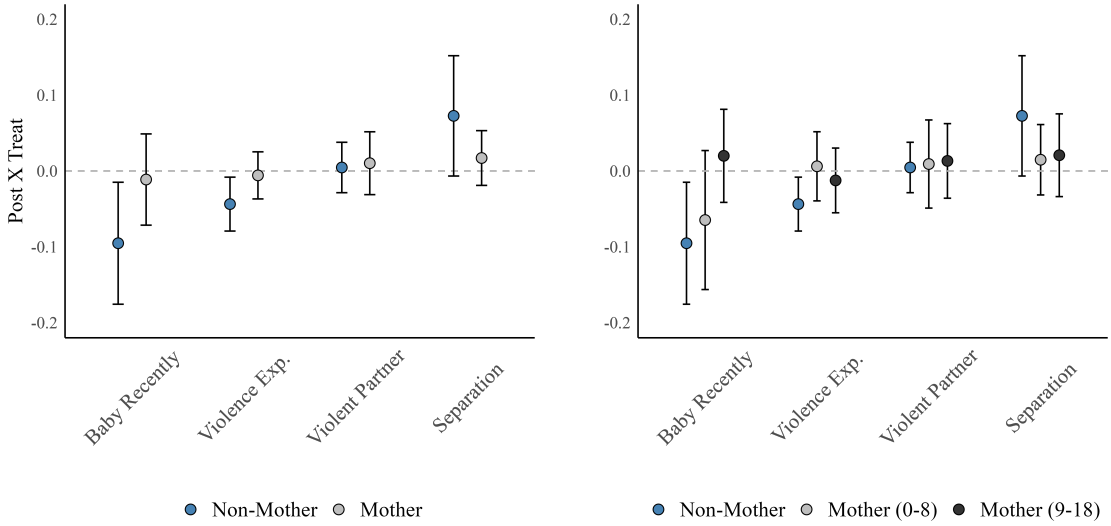
Monotonicity requires that there are no "defiers", meaning no woman is more likely to become a mother because she has access to a pharmacy. This is plausible because pharmacies do not provide services that increase fertility. Additionally I find no evidence in the heterogeneity or subgroup analyses that pharmacy openings increase the likelihood of becoming a mother for any group (see Figure 5 and Figure A6 in the Appendix).

The exclusion restriction requires that pharmacy access affects violence and separation only through its impact on motherhood. One potential concern is that pharmacies provide contraceptives as well as other medications. Access to a pharmacy might therefore improve the health of women or other household members, which could reduce stress and indirectly affect violence. To test this possibility, I focus on women who were already mothers before the pharmacy opening. Specifically, those whose first child was between zero and eighteen years old five years prior to the survey. If pharmacy access

[34] A canonical example of this strategy is Duflo (2001), where she employs a difference-in-differences to estimate the effects of school construction on education and wages.

affects violence or separation through channels other than motherhood, such as improved health, we would expect to observe similar effects among the already mothers. I estimate Equation 3 on the four main outcomes of interest.[35].

Figure 5 presents the results. Panel (a) compares the effects of pharmacy openings on the main sample (blue circles) and on already mothers (gray circles). For already mothers, all coefficients are small and statistically insignificant. Panel (b) further divides already mothers into two groups: those whose first child was between zero and eight years old (light gray circles) and those whose first child was older before the pharmacy opening (dark gray circles). Among mothers with younger first-borns, who are more likely to be affected on the intensive margin, the probability of having had a child within the last five years decreases by 6.4 percentage points after a pharmacy opening. Although this coefficient is imprecise, it is sizable and suggests that pharmacies also reduce fertility on the intensive margin. Despite this decline, there is no observed effect on violence or separation, reinforcing that only fertility on the extensive margin affects these outcomes.



Notes: Each panel plots the coefficient β_3 from Equation 3. Panel (a) reports results for women who were non-mothers five years before the survey (main analysis group) and for women who were mothers at that time. Panel (b) splits mothers by the age of their first child five years before the survey: 0–8 and 9–18 years old. The main analysis sample includes women under age 30 who were not mothers five years before the survey, while the sample of mothers has no age restriction. *Baby Recently* equals one if the woman had a baby in the last five years, *Violence Exp.* equals one if she experienced physical IPV in the last 12 months, *Violent Partner* equals one if her partner from five years before the survey ever exerted violence, and *Separation* equals one if the woman is separated or single with an ex-boyfriend. All regressions control for years of schooling and change in market access, and include age, wealth quintile, urban-rural, and state fixed effects. Regressions are weighted using survey weights, and standard errors are clustered at the AGEB level.

Figure 5: Effect of Pharmacy Access on Non-Mothers and Mothers

Who are the compliers? Before turning to the results, it is useful to clarify who the compliers are, as the coefficient β_3 in Equation 4 captures the Local Average Treatment Effect (LATE) of motherhood on violence and separation for this group. The compliers are women who become mothers when they lack access to a nearby pharmacy but delay motherhood when a pharmacy is accessible; these pregnancies are therefore unplanned. Appendix Table A7 shows that the pharmacy effects are concentrated among women in dating, non-cohabiting relationships, and Figure A6 shows that they are larger for younger and

[35]For fertility, I use a variable that equals one if the woman had a baby in the last five years.

poorer women. Taken together, these patterns suggest that the compliers are young boyfriend-girlfriend couples who wish to avoid a pregnancy.

Table 6 presents the 2SLS regression results. The first stage is moderately strong, with an F-statistic of 12.1, indicating that pharmacy openings generate meaningful variation in the likelihood of becoming a mother. Column (1) shows that becoming a mother increases the likelihood of experiencing physical violence by 44.1 percentage points (p = 0.089). Because the instrument shifts the probability of motherhood only modestly (mean fitted value 0.39, sd 0.24), I scale the estimate to provide a clearer sense of magnitude: a one-standard-deviation increase in the instrumented probability of motherhood raises the likelihood of experiencing physical violence by 10.5 percentage points. Column (2) shows no effect on men's propensity to exert violence, while Column (3) indicates that becoming a mother reduces the likelihood of separation by 75.2 percentage points (p = 0.083). Using the same scaling, a one-standard-deviation increase in the probability of motherhood lowers the likelihood of separation by 18 percentage points. These magnitudes are large but consistent with the complier group of young women for whom an unplanned birth substantially raises the cost of exit.

Table 6: Effect of Motherhood on Violence and Separation

	Violence Exposure (1)	Violent Partner (2)	Separation (3)
$\widehat{Mother}$	0.441*	-0.037	-0.752*
	(0.260)	(0.173)	(0.434)
Observations	8,635	8,635	8,635
F-test (1st stage), $\widehat{Mother}$	12.056	12.056	12.056
Age fixed effects	✓	✓	✓
Rural/Urban fixed effects	✓	✓	✓
State fixed effects	✓	✓	✓
Wealth Quintile fixed effects	✓	✓	✓
School years	✓	✓	✓
Δ Mkt. Access	✓	✓	✓

Notes: Each column reports the coefficient β_3 from Equation 4, estimated using data from ENDIREH 2011 and 2016. The sample includes women aged 15–30 who were not mothers five years before the survey. $\widehat{Mother}$ is the first-stage fitted value of the endogenous motherhood indicator, obtained by instrumenting motherhood with pharmacy openings within one kilometer between 2011 and 2016. *Violence Exposure* equals one if the woman experienced physical intimate partner violence in the last 12 months. *Violent Partner* equals one if the woman experienced physical violence at any point in her relationship with the partner from five years before the survey. *Separation* equals one if the woman is separated or single with an ex-boyfriend at the time of the survey. All regressions control for years of schooling and the change in market access, as well as age, wealth quintile, rural/urban, and state fixed effects. Regressions are weighted using survey weights, and standard errors clustered at the AGEB level are reported in parentheses. Significance levels: *** 0.01, ** 0.05, * 0.10.

Summary of Results

The table below summarizes the paper's findings. The first column lists the model's four predictions. Predictions 1-3 concern the effect of motherhood on (i) women's exposure to violence (V^W), (ii) men's

violent behavior (V^M), and (iii) separation (L). Prediction 4 concerns whether delaying motherhood allows women to learn about their partner's type; this cannot be tested in the pharmacy analysis because the timing of childbirth is endogenous. The second and third columns report the theoretical predictions under the direct and selection channels, respectively. Columns 4 and 5 present the empirical results from the first-partner analysis and the IV difference-in-differences analysis. Both exercises, one documenting equilibrium correlations for the general population and the other estimating causal effects for the sub-population of compliers, support the selection channel. The results suggest that motherhood increases women's exposure to violence by raising the cost of leaving abusive partners, rather than by altering men's underlying propensity to be violent.

Table 7: Summary of Theoretical Predictions and Empirical Results

	Direct Ch.	*Selection Ch.*	First partner	IV-DD
P1: Violence exposure	↑	↑	↑	↑
P2: Violent partner	↑	=	=	=
P3: Separation	↑	↓	↓	↓
P4: Violent partner (delayed)	=	↓	↓	

Notes: P1-P3 show the effect of motherhood on each outcome: ↑ increases, ↓ decreases, = no effect. P4 compares women who became mothers early to those who delayed: ↓ indicates that women who delay are less likely to have a violent partner. First partner refers to the descriptive analysis in Section IV. IV-DD refers to the causal estimates in Section V.

V.E Robustness

This section presents a series of robustness exercises that test the validity of the main results. I assess whether the findings are sensitive to assumptions about missing data on men's violent behavior, alternative explanations related to women's access to exit resources, and variations in how treatment and sample definitions are constructed. In all cases, the results remain consistent with the main findings.

Bounding the effect of pharmacy openings on violent behavior

As discussed earlier, information on men's violent behavior is missing for about 37 percent of women. In the main analysis, I impute zeros for these cases, which corresponds to assuming that they did not experience violence five years before the survey. To assess the robustness of this assumption, I construct conservative lower and upper bounds for the treatment effect, drawing on the partial identification literature (e.g., Manski and Pepper, 2000; Manski, 2005).

The key assumption is that the prevalence of violence among women with missing histories is not higher than among otherwise similar women for whom this information is observed. This is plausible because the missingness arises mechanically rather than from selective nonresponse,[36] and the resulting upper bound is conservative since it implicitly assumes that all women with missing histories were in a relationship five years earlier, which likely overstates their exposure to potential violence.

For each woman with missing information, I assign an individual lower bound of zero and an individual upper bound equal to the average prevalence of violence among comparable women within

[36]Missingness arises because ENDIREH records detailed information only for the reference partner, defined as the current partner for women in a relatioship and the most recent partner for separated women or single women with an ex-boyfriend. Women who began their most recent relationship within the last four years therefore have no recorded information on any partner they may have had five years before the survey.

the same treatment-year cell. I then use these individual bounds to construct treatment-level lower and upper bounds for the difference-in-differences estimator. The upper bound maximizes the estimated treatment effect by imputing missing values such that the increase in violent behavior among treated women is as large as possible and the change among control women as small as possible. The lower bound reverses this assignment, minimizing the estimated treatment effect. A full description of the procedure is provided in Appendix E.E.

The results are reported in Table A9 in the Appendix. The lower bound estimate is negative but statistically insignificant (-0.02), while the upper bound is positive and statistically significant at the 10 percent level (0.03). The upper bound represents the maximum possible treatment effect under highly conservative assumptions and should therefore be interpreted with caution. The lower bound, however, is informative. Even under the extreme scenario that minimizes the treatment effect, the confidence interval includes zero, which is consistent with no effect on men's violent behavior, precisely what the selection channel predicts. This supports the broader conclusion that I find no evidence of fatherhood increasing men's propensity to exert violence.

Changes in help-seeking behavior

An alternative explanation is that pharmacy openings provide women with exit resources. The analysis among already mothers helps rule out services that would affect mothers and non-mothers similarly. However, if pharmacies offered information on where to report violence or accessing shelters, and non-mothers disproportionately benefited from these services, pharmacy presence could increase separation, which might then reduce fertility and violence. This interpretation is unlikely. Pharmacists are not trained to provide this type of support, and interviews I conducted indicate that they do not discuss violence-related issues with customers.

However, to test this possibility quantitatively, I examine whether pharmacy openings affect women's help-seeking behavior. I re-estimate Equation 3 using three outcomes that capture where women can access support (judicial authorities, the municipality and social services). Under the mechanism I propose, the effect on help-seeking behavior is ambiguous: reduced violence exposure should lower reporting, while higher separation could increase it if women find it easier to report ex-partners. Under the alternative explanation, where pharmacies directly provide information and resources that facilitate reporting, we would expect a systematic increase in reporting. Table A10 in the Appendix shows no such pattern. Reports to judicial authorities and municipalities are negative but statistically insignificant, and reports to social services are negative and significant at the ten percent level. Overall, there is no evidence that pharmacies improved access to information or assistance for victims of violence.

Alternative definitions of access to pharmacies

The main analysis defines treatment as living within one kilometer of a pharmacy in 2016 but not in 2011. The threshold to define access to a pharmacy is an empirical question. If the threshold is too small, the treated group may include women who already had access (for example, a woman who had a pharmacy within 200 meters in 2011 and a new pharmacy opened 100 meters closer in 2016). If the threshold is too large, the treated group may include women who remain without access. To explore this, I re-estimate Equation 3 using thresholds from 300 to 1,700 meters in 100-meter increments. Figure A7 shows that fertility effects are small and statistically insignificant for thresholds below one kilometer, but become larger and significant around that point and remain stable thereafter. The figure also provides further support for the identification strategy. Effects on violence exposure closely mirror those on

fertility, while separation moves in the opposite direction.[37] This co-movement suggests that pharmacy openings affect violence and separation only when they also affect fertility, consistent with the exclusion restriction.

Alternative age thresholds

The main sample includes women younger than 30 years old. I re-estimate Equation 3 using alternative age thresholds and find that the results are robust. In particular, I consider samples of women younger than 25, younger than 35, younger than 40, and younger than 45. The estimated coefficients remain stable across age thresholds, with the effects on fertility and separation declining slightly as the sample includes older women. Figure A8 in the Appendix shows these results.

Alternative control group

In the main analysis, the control group includes women who had no pharmacy within one kilometer in 2011 and remained without one in 2016. As an alternative, I define the control group as women who already had a pharmacy within one kilometer in both years. This comparison with "always-treated" areas ensures that the main results are not driven by systematic differences between places that gained access and those that were persistently underserved. Table A11 in the Appendix reports the results. The estimated coefficients are around 25% smaller and less precise but show the same pattern: pharmacy openings reduce fertility and women's exposure to violence and increase separation.

VI Conclusion

This paper develops a framework for studying how motherhood affects intimate partner violence by separating two components of observed exposure: men's decisions to use violence and women's decisions to remain in violent relationships. Standard IPV measures combine these components, even though they imply different policy responses.

Using Mexican survey data, I document that mothers experience higher violence exposure than non-mothers but are no more likely to have violent partners. Instead, mothers are substantially less likely to leave relationships. To move from correlation to causation, I exploit pharmacy openings as a shock to contraceptive access. Pharmacies reduce fertility and violence exposure while increasing separation, with no effect on whether partners are violent. Both the descriptive and causal evidence point to selection: motherhood increases violence exposure by raising exit costs, not by changing men's behavior.

These findings have three implications. First, reproductive autonomy matters for violence. Delaying childbearing allows women to leave abusive partners before children raise exit costs. Second, policies that reduce the costs of single motherhood, such as childcare subsidies, income support, and legal assistance, may enable mothers to leave relationships that non-mothers would exit. Third, cross-country differences in the motherhood-violence relationship may reflect differences in social support rather than differences in how men respond to fatherhood. Motherhood increases violence exposure in settings like Brazil (Britto et al., 2024), the United States (Massenkoff and Rose, 2024), and Mexico, but decreases it in Sweden (Bergvall and Rodríguez-Planas, 2024), where social support systems are stronger.

The two-mechanism framework extends beyond motherhood. Employment programs, cash transfers, and legal reforms all affect women's outside options and could operate through either margin. Recogniz-

[37] The correlation between the coefficients for fertility and violence exposure is 0.85, and between fertility and separation is -0.86.

ing this distinction matters for interpreting existing evidence: changes in violence exposure commonly attributed to men's behavior may instead reflect changes in women's ability to leave. It also matters for policy design. Reducing men's violent behavior generates spillovers to future partners, as violence persists across relationships (Adams et al., 2024), while facilitating exit protects only women who leave. Disentangling these mechanisms requires data on violence with current and former partners and samples that include women of all civil statuses. With such data, the approach developed here can identify which margin a given policy affects, and whether reducing violence requires changing men's behavior or expanding women's options.

References

Adams, Abi et al. (2024). "The Dynamics of Abusive Relationships". In: *The Quarterly Journal of Economics*.

Aizer, Anna (2010). "The Gender Wage Gap and Domestic Violence". In: *American Economic Review* 100(4), pp. 1847–1859.

Aizer, Anna and Pedro Dal Bó (2009). "Love, Hate and Murder: Commitment Devices in Violent Relationships". In: *Journal of Public Economics* 93(3-4), pp. 412–428.

Alesina, Alberto, Beatrice Brioschi, and Eliana La Ferrara (2021). "Violence Against Women: A Cross-Cultural Analysis for Africa". In: *Economica* 88(349), pp. 70–104.

Anderberg, Dan, Noemi Mantovan, and Robert M. Sauer (2023). "The Dynamics of Domestic Violence: Learning about the Match". In: *The Economic Journal* 133(656), pp. 2863–2898.

Anderberg, Dan et al. (2015). "Unemployment and Domestic Violence: Theory and Evidence". In: *Economic Journal* 126(597), pp. 1947–1979.

Angelucci, Manuela (2008). "Love on the Rocks: Domestic Violence and Alcohol Abuse in Rural Mexico". In: *The B.E. Journal of Economic Analysis & Policy* 8(1), p. 43.

Angrist, Joshua D. and William N. Evans (1998). "Children and Their Parents' Labor Supply: Evidence from Exogenous Variation in Family Size". In: *American Economic Review* 88(3), pp. 450–477.

Arenas-Arroyo, Esther, Daniel Fernandez-Kranz, and Natalia Nollenberger (2021). "Intimate Partner Violence Under Forced Cohabitation and Economic Stress: Evidence from the COVID-19 Pandemic". In: *Journal of Public Economics* 194, p. 104350.

Ashraf, Nava et al. (2014). "The Psycho-Social Benefits of Access to Contraception: Experimental Evidence from Zambia". Working Paper.

Bailey, Martha J. (2006). "More Power to the Pill: The Impact of Contraceptive Freedom on Women's Life Cycle Labor Supply". In: *The Quarterly Journal of Economics* 121(1), pp. 289–320.

Bergvall, Sanna and Núria Rodríguez-Planas (2024). *Motherhood and Domestic Violence: A Longitudinal Study Using Population-Wide Administrative Data*. Tech. rep. IZA Discussion Paper No. 17129. Discussion paper. IZA Institute of Labor Economics.

Bhalotra, Sonia et al. (2024). "Dynamic Impacts of Lockdown on Domestic Violence: Evidence from Multiple Policy Shifts in Chile". In: *Review of Economics and Statistics*.

Bhalotra, Sonia et al. (2025). "Job Displacement, Unemployment Benefits and Domestic Violence". In: *Review of Economic Studies*.

Bloch, Francis and Vijayendra Rao (2002). "Terror as a Bargaining Instrument: A Case Study of Dowry Violence in Rural India". In: *American Economic Review* 92(4), pp. 1029–1043.

Bobonis, Gustavo J., Melissa González-Brenes, and Roberto Castro (2013). "Public Transfers and Domestic Violence: The Roles of Private Information and Spousal Control". In: *American Economic Journal: Economic Policy* 5(1), pp. 179–205.

Bowlus, Audra J. and Shannon Seitz (2006). "Domestic Violence, Employment, and Divorce". In: *International Economic Review* 47(4), pp. 1113–1149.

Brassiolo, Pablo (2016). "Domestic Violence and Divorce Law: When Divorce Threats Become Credible". In: *Journal of Labor Economics* 34(2), pp. 443–477.

Britto, Diogo et al. (2024). *Small Children, Big Problems: Childbirth and Crime*. Tech. rep. IZA Discussion Paper No. 16910. Discussion paper. IZA Institute of Labor Economics.

Bullinger, Lindsey Rose, Jillian B. Carr, and Analisa Packham (2021). "COVID-19 and Crime: Effects of Stay-at-Home Orders on Domestic Violence". In: *American Journal of Health Economics* 7(3), pp. 249–280.

Bø Vatnar, Solveig Karin and Stål Bjørkly (2010). "Does It Make Any Difference if She Is a Mother? An Interactional Perspective on Intimate Partner Violence with a Focus on Motherhood and Pregnancy". In: *Journal of Interpersonal Violence* 25(1), pp. 94–110.

Calvi, Rossella and Ajinkya Keskar (2023). "'Til Dowry Do Us Part: Bargaining and Violence in Indian Families". In: *Review of Economics and Statistics*.

Card, David and Gordon B. Dahl (2011). "Family Violence and Football: The Effect of Unexpected Emotional Cues on Violent Behavior". In: *The Quarterly Journal of Economics* 126(1), pp. 103–143.

Chernozhukov, Victor et al. (2024). *Applied Causal Inference Powered by ML and AI*. CausalML-book.org.

Chin, Yoo-Mi (2012). "Male Backlash, Bargaining, or Exposure Reduction?: Women's Working Status and Physical Spousal Violence in India". In: *Journal of Population Economics* 25(1), pp. 175–200.

Cinelli, Carlos, Andrew Forney, and Judea Pearl (2024). "A Crash Course in Good and Bad Controls". In: *Sociological Methods & Research* 53(2), pp. 481–504.

Cunningham, Scott (2021). *Causal Inference: The Mixtape*. Yale University Press.

Dave, Dhaval M. et al. (2025). "Fighting Abuse with Prescription Tracking: Mandatory Drug Monitoring and Intimate Partner Violence". In: *Journal of Population Economics* 38(3), p. 57.

Daw, J., G. Halliwell, S. Hay, et al. (2023). ""You Don't Notice It, It's Like Boiling Water": Identifying Psychological Abuse Within Intimate Partner Relationships and How It Develops Across a Domestic Homicide Timeline". In: *Current Psychology* 42, pp. 20000–20014.

Doepke, Matthias and Fabian Kindermann (2019). "Bargaining over Babies: Theory, Evidence, and Policy Implications". In: *American Economic Review* 109(9), pp. 3264–3306.

Duflo, Esther (2001). "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment". In: *American Economic Review* 91(4), pp. 795–813.

Dugan, Laura, Daniel S. Nagin, and Richard Rosenfeld (1999). "Explaining the Decline in Intimate Partner Homicide: The Effects of Changing Domesticity, Women's Status, and Domestic Violence Resources". In: *Homicide Studies* 3(3), pp. 187–214.

Erten, Bilge and Pinar Keskin (2018). "For Better or for Worse?: Education and the Prevalence of Domestic Violence in Turkey". In: *American Economic Journal: Applied Economics* 10(1), pp. 64–105.

Erten, Bilge and Pinar Keskin (2021). "Female Employment and Intimate Partner Violence: Evidence from Syrian Refugee Inflows to Turkey". In: *Journal of Development Economics* 150, p. 102607.

Erten, Bilge and Pinar Keskin (2024). "Trade-Offs? The Impact of WTO Accession on Intimate Partner Violence in Cambodia". In: *Review of Economics and Statistics* 106(2), pp. 322–333.

Erten, Bilge and Tomer Stern (2024). "Women's Participation in the Arab Spring Protests and the Prevalence of Domestic Violence: Evidence from Egypt". In: *Feminist Economics* 30(4), pp. 42–74.

García-Ramos, Aixa (2021). "Divorce Laws and Intimate Partner Violence: Evidence from Mexico". In: *Journal of Development Economics* 150, p. 102623.

Goldin, Claudia and Lawrence F. Katz (2002). "The Power of the Pill: Oral Contraceptives and Women's Career and Marriage Decisions". In: *Journal of Political Economy* 110(4), pp. 730–770.

Guarnieri, Eleonora and Helmut Rainer (2021). "Colonialism and Female Empowerment: A Two-Sided Legacy". In: *Journal of Development Economics* 151, p. 102662.

Haushofer, Johannes et al. (2019). *Income Changes and Intimate Partner Violence: Evidence from Unconditional Cash Transfers in Kenya*. Tech. rep. 25627. National Bureau of Economic Research.

Heath, Rachel (2014). "Women's Access to Labor Market Opportunities, Control of Household Resources, and Domestic Violence: Evidence from Bangladesh". In: *World Development* 57, pp. 32–46.

Heath, Rachel, Melissa Hidrobo, and Shalini Roy (2020). "Cash Transfers, Polygamy, and Intimate Partner Violence: Experimental Evidence from Mali". In: *Journal of Development Economics* 143, p. 102410.

Hidrobo, Melissa and Lia C. H. Fernald (2013). "Cash Transfers and Domestic Violence". In: *Journal of Health Economics* 32(1), pp. 304–319.

Hidrobo, Melissa, Amber Peterman, and Lori Heise (2016). "The Effect of Cash, Vouchers, and Food Transfers on Intimate Partner Violence: Evidence from a Randomized Experiment in Northern Ecuador". In: *American Economic Journal: Applied Economics* 8(3), pp. 284–303.

Hsu, L. C. and A. Henke (2021). "COVID-19, Staying at Home, and Domestic Violence". In: *Review of Economics of the Household* 19, pp. 145–155.

Hudson, Sally, Peter Hull, and Jack Liebersohn (2017). *Interpreting Instrumented Difference-in-Differences*.

Hünermund, Paul and Elias Bareinboim (2023). "Causal Inference and Data Fusion in Econometrics". In: *Econometrics Journal* 26(2), pp. 176–212.

INEGI (2019). *Encuesta Nacional sobre el Uso del Tiempo (ENUT) 2019: Presentación de resultados, Segunda edición*. Mexico: Instituto Nacional de Estadística y Geografía (INEGI).

Instituto Nacional de Estadística y Geografía (2006, 2011, 2016). *Encuesta Nacional sobre la Dinámica de las Relaciones en los Hogares (ENDIREH)*. Aguascalientes, Mexico. URL: https://www.inegi.org.mx/programas/endireh/.

Instituto Nacional de Estadística y Geografía (2009, 2014). *Encuesta Nacional de la Dinámica Demográfica (ENADID)*. Aguascalientes, Mexico. URL: https://www.inegi.org.mx/programas/enadid/.

Instituto Nacional de Estadística y Geografía (2010). *Marco Geoestadístico*. Aguascalientes, Mexico. URL: https://www.inegi.org.mx/temas/mg/.

Instituto Nacional de Estadística y Geografía (2011, 2016). *Directorio Estadístico Nacional de Unidades Económicas (DENUE)*. Aguascalientes, Mexico. URL: https://www.inegi.org.mx/app/descarga/default.html.

Instituto Nacional de las Mujeres (INMUJERES) (2020). *Una Mirada a la Maternidad en México*. Tech. rep. Mexico City. INMUJERES.

Ivandić, Ria et al. (2024). "Football, Alcohol, and Domestic Abuse". In: *Journal of Public Economics* 230, p. 105031.

Iyengar, Radha (2009). "Does the Certainty of Arrest Reduce Domestic Violence? Evidence from Mandatory and Recommended Arrest Laws". In: *Journal of Public Economics* 93(1–2), pp. 85–98.

Kleven, Henrik (2022). *The Geography of Child Penalties and Gender Norms: A Pseudo-Event Study Approach*. Tech. rep. Working Paper No. 30176. Revised August 2025. National Bureau of Economic Research. DOI: 10.3386/w30176.

Kleven, Henrik, Camille Landais, and Gabriel Leite-Mariante (2025). "The Child Penalty Atlas". In: *Review of Economic Studies*.

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

Kotsadam, Andreas and Espen Villanger (2025). "Jobs and Intimate Partner Violence: Evidence from a Field Experiment in Ethiopia". In: *Journal of Human Resources* 60(2), pp. 469–495.

La Mattina, Giulia (2017). "Civil Conflict, Domestic Violence and Intra-Household Bargaining in Post-Genocide Rwanda". In: *Journal of Development Economics* 124(C), pp. 168–198.

Luke, Nancy and Kaivan Munshi (2011). "Women as Agents of Change: Female Income and Mobility in India". In: *Journal of Development Economics* 94(1), pp. 1–17.

Manski, Charles F. (1990). "Nonparametric Bounds on Treatment Effects". In: *American Economic Review* 80(2), pp. 319–323.

Manski, Charles F. (2005). "Partial Identification with Missing Data: Concepts and Findings". In: *International Journal of Approximate Reasoning* 39(2–3), pp. 151–165.

Manski, Charles F. and John V. Pepper (2000). "Monotone Instrumental Variables: With an Application to the Returns to Schooling". In: *Econometrica* 68(4), pp. 997–1010.

Massenkoff, Maxim N. and Evan K. Rose (2024). "Family Formation and Crime". In: *American Economic Journal: Applied Economics* 16(4), pp. 444–483.

McLindon, E. et al. (2025). "Timelines of Psychological, Physical and Sexual Intimate Partner Violence Among a Nationally Representative Sample of Australian Women". In: *Women's Health (London)* 21, p. 17455057251329640.

Miller, Grant (2010). "Contraception as Development? New Evidence from Family Planning in Colombia". In: *The Economic Journal* 120(545), pp. 709–736.

Pearl, Judea (1995). "Causal Diagrams for Empirical Research". In: *Biometrika* 82(4), pp. 669–688.

Rosenzweig, Mark R. and Kenneth I. Wolpin (1980). "Testing the Quantity-Quality Fertility Model: The Use of Twins as a Natural Experiment". In: *Econometrica* 48(1), pp. 227–240.

Roy, Shalini et al. (2019). "Transfers, Behavior Change Communication, and Intimate Partner Violence: Postprogram Evidence from Rural Bangladesh". In: *Review of Economics and Statistics* 101(5), pp. 865–877.

Sanin, Deniz (2023). "When Do Domestic Violence Laws Work? The Role of Social Norms in Household Bargaining". Working paper.

Sanin, Deniz (2024). "Women's Employment, Husbands' Economic Self-Interest and Domestic Violence". Revise and resubmit, *American Economic Review*.

Sardinha, Lynnmarie et al. (2022). "Global, regional, and national prevalence estimates of physical or sexual, or both, intimate partner violence against women in 2018". In: *The Lancet* 399(10327), pp. 803–813.

Shah, Manisha et al. (2023). *Sex, Power, and Adolescence: Intimate Partner Violence and Sexual Behaviors*. Tech. rep. NBER Working Paper No. 31624. Working paper. National Bureau of Economic Research.

Stevenson, Betsey and Justin Wolfers (2007). "Marriage and Divorce: Changes and Their Driving Forces". In: *Journal of Economic Perspectives* 21(2), pp. 27–52.

Tur-Prats, Ana (2019). "Family Types and Intimate Partner Violence: A Historical Perspective". In: *Review of Economics and Statistics* 101(5), pp. 878–891.

Tur-Prats, Ana (2021). "Unemployment and Intimate Partner Violence: A Cultural Approach". In: *Journal of Economic Behavior & Organization* 185, pp. 27–49.

World Health Organization (2021). *Violence against Women Prevalence Estimates, 2018: Global, Regional and National Prevalence Estimates for Intimate Partner Violence Against Women and Global and Regional Prevalence Estimates for Non-Partner Sexual Violence Against Women*. Licence: CC BY-NC-SA 3.0 IGO. Geneva. URL: https://www.who.int/publications/i/item/9789240022256.

Online Supplementary Material

Motherhood and Violence

Gabriela Deschamps

A Tables

Table A1: Summary of IPV Papers in the Literature (continued on next page)

Paper	Main IPV Variable	Partnered Sample	Other Control
A. Administrative Data			
Adams et al. (2024)	Police reports		
Aizer (2010)	Female hospitalizations		
Aizer and Dal Bó (2009)	Police reports; Homicides; Hospitalizations		✓
Bergvall and Rodríguez-Planas (2024)	Hospital records		✓
Bhalotra et al. (2025)	DV court cases; Shelters		✓
Bhalotra et al. (2024)	Police calls and reports; Public shelters		
Britto et al. (2024)	Criminal cases; Health units		
Bullinger et al. (2021)	Police calls, reports, and arrests		
Card and Dahl (2011)	Police reports		
Dave et al. (2025)	Police reports		
Hsu and Henke (2021)	Police reports		
Ivandić et al. (2024)	Police calls		✓
Iyengar (2009)	Homicides		
Massenkoff and Rose (2024)	Police arrests		✓
Stevenson and Wolfers (2007)	Homicides; Suicides		
B. Survey Data			
Alesina et al. (2021)	IPV ever	✓	
Anderberg et al. (2015)	IPV last 12 months		
Anderberg et al. (2023)	IPV last 12 months		
Angelucci (2008)	Other	✓	
Arenas-Arroyo et al. (2021)	Other	✓	
Bhalotra et al. (2025)	IPV last 12 months		
Bobonis et al. (2013)	IPV last 12 months	✓	
Bowlus and Seitz (2006)	IPV last 12 months		
Brassiolo (2016)	IPV last 12 months		✓
Calvi and Keskar (2023)	IPV ever		
Chin (2012)	IPV last 12 months		
Erten and Keskin (2018)	IPV ever		
Erten and Keskin (2021)	IPV last 12 months	✓	
Erten and Keskin (2024)	IPV last 12 months		
Erten and Stern (2024)	IPV last 12 months		✓

Table A1: Summary of IPV Papers in the Literature (continued)

Paper	Main IPV Variable	Partnered Sample	Other Control
García-Ramos (2021)	IPV last 12 months		✓
Guarnieri and Rainer (2021)	IPV last 12 months		
Haushofer et al. (2019)	IPV last 12 months	✓	
Heath (2014)	IPV ever	✓	
Heath et al. (2020)	IPV last 12 months		
Hidrobo and Fernald (2013)	Other	✓	
Hidrobo et al. (2016)	IPV last 6 months	✓	
Kotsadam and Villanger (2025)	IPV last 3 months		
La Mattina (2017)	IPV last 12 months	✓	
Luke and Munshi (2011)	IPV last year	✓	
Roy et al. (2019)	IPV last 6 months		
Sanin (2023)	IPV last 12 months		✓
Sanin (2024)	IPV last 12 months		
Shah et al. (2023)	IPV last year + IPV often		
Stevenson and Wolfers (2007)	IPV last 12 months	✓	
Tur-Prats (2019)	At the moment		✓
Tur-Prats (2021)	At the moment		

Notes: This table summarizes empirical papers in the economics literature studying intimate partner violence (IPV). "Main IPV Variable" reports the main outcome used in the analysis. "Partnered Sample" indicates whether the main analysis is restricted to partnered or married women. "Other Control" indicates that some section of the paper uses a sample of partnered women or includes controls for civil status.

Table A2: Summary Statistics

	Control Group	Treated Group	All Women
A. Individual Characteristics			
Mean Age	20.44	20.88	20.51
Mean Schooling	9.71	10.91	9.92
Mean Wealth Quintile	2.43	2.92	2.52
% Work (lw)	0.26	0.35	0.27
% Indigenous	0.09	0.05	0.08
B. Pharmacy Access			
Dist. to nearest pharmacy 2011	5461.69	2038.67	4856.18
Dist. to nearest pharmacy 2016	5360.11	562.75	4511.49
C. Outcome Variables			
% Mothers	0.37	0.34	0.37
% Violence Exposure	0.04	0.05	0.04
% Violent Partner	0.03	0.03	0.03
% Separation	0.23	0.29	0.24
N	7100	1535	8635

Notes: This table reports summary statistics for the main sample used in the analysis. The sample includes women younger than 30 years old who were not mothers five years before the survey and belong either to the treatment or control group. Treated women are those who did not have a pharmacy within a one-kilometer radius in 2011 but had at least one in 2016. Women in the control group did not have a pharmacy within a one-kilometer radius in either 2011 or 2016. *Violence Exposure* equals one if the woman experienced physical intimate partner violence (IPV) in the last 12 months. *Violent Partner* equals one if her partner from five years prior was ever physically violent. *Separation* equals one if the woman is separated or single with an ex-boyfriend at the time of the survey.

Table A3: Effect of Pharmacy Access on Motherhood

	Mother			
	(1)	(2)	(3)	(4)
Constant	0.427***			
	(0.012)			
Post	-0.082***	-0.032**	-0.034**	-0.035**
	(0.016)	(0.015)	(0.015)	(0.015)
Treat	0.035	0.073**	0.051	0.047
	(0.033)	(0.034)	(0.035)	(0.034)
Post × Treat	-0.087**	-0.081**	-0.091**	-0.095**
	(0.040)	(0.039)	(0.038)	(0.038)
Observations	8,635	8,635	8,635	8,635
Mean Dep.Var	0.366	0.366	0.366	0.366
$\hat{\beta}_0 + \hat{\beta}_1 + \hat{\beta}_2$	0.381			
Age fixed effects		✓	✓	✓
Wealth Quintile fixed effects		✓	✓	✓
Rural/Urban fixed effects			✓	✓
State fixed effects			✓	✓
School years		✓	✓	✓
Δ Mkt. Access				✓

Notes: Each column reports the coefficient β_3 from Equation 3. The sample includes women aged 15–30 who were not mothers five years before the survey. *Mother* equals one if the woman is a mother at the time of the survey. Treated women are those who did not have a pharmacy within one kilometer in 2011 but had at least one in 2016; the control group includes women who did not have a pharmacy within one kilometer in either year. *Post* equals one if the survey year is 2016. Column (2) includes age and wealth quintile fixed effects and controls for years of schooling. Column (3) adds state and urban–rural fixed effects. Column (4) additionally controls for the change in market access, defined as the increase in the number of businesses within three kilometers between 2011 and 2016. All regressions are weighted using survey weights, and standard errors clustered at the AGEB level are reported in parentheses. Significance levels: *** 0.01, ** 0.05, * 0.10.

Table A4: Effect of Pharmacy Access on Violence Exposure V^W

	Violence Exposure			
	(1)	(2)	(3)	(4)
Constant	0.033***			
	(0.005)			
Post	0.012*	0.016**	0.019**	0.018**
	(0.007)	(0.007)	(0.008)	(0.008)
Treat	0.034**	0.040**	0.041**	0.040**
	(0.017)	(0.016)	(0.017)	(0.017)
Post × Treat	-0.041**	-0.041**	-0.041**	-0.042**
	(0.019)	(0.018)	(0.018)	(0.018)
Observations	8,635	8,635	8,635	8,635
Mean Dep.Var	0.042	0.042	0.042	0.042
$\hat{\beta}_0 + \hat{\beta}_1 + \hat{\beta}_2$	0.079			
Age fixed effects		✓	✓	✓
Wealth Quintile fixed effects		✓	✓	✓
Rural/Urban fixed effects			✓	✓
State fixed effects			✓	✓
School years		✓	✓	✓
Δ Mkt. Access				✓

Notes: Each column reports the coefficient β_3 from Equation 3. The sample includes women aged 15–30 who were not mothers five years before the survey. *Violence Exposure* equals one if the woman experienced physical intimate partner violence in the last 12 months. Treatment and control groups are defined as in Table A3. Column (2) includes age and wealth quintile fixed effects and controls for years of schooling. Column (3) adds state and urban–rural fixed effects. Column (4) additionally controls for the change in market access, defined as the increase in the number of businesses within three kilometers between 2011 and 2016. All regressions are weighted using survey weights, and standard errors clustered at the AGEB level are reported in parentheses. Significance levels: *** 0.01, ** 0.05, * 0.10.

Table A5: Effect of Pharmacy Access on Violent Partner V^M

	Violent Partner			
	(1)	(2)	(3)	(4)
Constant	0.022***			
	(0.004)			
Post	0.009*	0.013**	0.013**	0.013**
	(0.005)	(0.006)	(0.006)	(0.006)
Treat	0.001	0.003	0.006	0.006
	(0.012)	(0.012)	(0.012)	(0.013)
Post × Treat	0.005	0.005	0.004	0.004
	(0.016)	(0.016)	(0.017)	(0.017)
Observations	8,635	8,635	8,635	8,635
Mean Dep.Var	0.029	0.029	0.029	0.029
$\hat{\beta}_0 + \hat{\beta}_1 + \hat{\beta}_2$	0.032			
Age fixed effects		✓	✓	✓
Wealth Quintile fixed effects		✓	✓	✓
Rural/Urban fixed effects			✓	✓
State fixed effects			✓	✓
School years		✓	✓	✓
Δ Mkt. Access				✓

Notes: Each column reports the coefficient β_3 from Equation 3. The sample includes women aged 15–30 who were not mothers five years before the survey. *Violent Partner* equals one if the woman experienced physical violence at any point in the relationship with her partner from five years before the survey. Treatment and control groups are defined as in Table A3. Column (2) includes age and wealth quintile fixed effects and controls for years of schooling. Column (3) adds state and urban–rural fixed effects. Column (4) additionally controls for the change in market access, defined as the increase in the number of businesses within three kilometers between 2011 and 2016. All regressions are weighted using survey weights, and standard errors clustered at the AGEB level are reported in parentheses. Significance levels: *** 0.01, ** 0.05, * 0.10.

Table A6: Effect of Pharmacy Access on Separation L

	Separation			
	(1)	(2)	(3)	(4)
Constant	0.188***			
	(0.011)			
Post	0.056***	0.043***	0.037**	0.037**
	(0.015)	(0.015)	(0.016)	(0.016)
Treat	0.018	-0.003	-0.024	-0.025
	(0.031)	(0.030)	(0.034)	(0.035)
Post × Treat	0.062	0.068*	0.072*	0.071*
	(0.040)	(0.038)	(0.039)	(0.040)
Observations	8,635	8,635	8,635	8,635
Mean Dep.Var	0.238	0.238	0.238	0.238
$\hat{\beta}_0 + \hat{\beta}_1 + \hat{\beta}_2$	0.262			
Age fixed effects		✓	✓	✓
Wealth Quintile fixed effects		✓	✓	✓
Rural/Urban fixed effects			✓	✓
State fixed effects			✓	✓
School years		✓	✓	✓
Δ Mkt. Access				✓

Notes: Each column reports the coefficient β_3 from Equation 3. The sample includes women aged 15–30 who were not mothers five years before the survey. *Separation* equals one if the woman is separated or single with an ex-boyfriend at the time of the survey. Treatment and control groups are defined as in Table A3. Column (2) includes age and wealth quintile fixed effects and controls for years of schooling. Column (3) adds state and urban–rural fixed effects. Column (4) additionally controls for the change in market access, defined as the increase in the number of businesses within three kilometers between 2011 and 2016. All regressions are weighted using survey weights, and standard errors clustered at the AGEB level are reported in parentheses. Significance levels: *** 0.01, ** 0.05, * 0.10.

Table A7: Effect of Pharmacy Access on Civil Status

	Partner (1)	Separated (2)	Boyfriend (3)	Ex-boyfriend (4)	Never bf (5)
Post × Treat	-0.057*	-0.023	-0.031	0.094**	0.016
	(0.033)	(0.021)	(0.035)	(0.039)	(0.030)
Observations	8,635	8,635	8,635	8,635	8,635
Mean Dep.Var	0.403	0.049	0.176	0.188	0.182
Age fixed effects	✓	✓	✓	✓	✓
Rural/Urban fixed effects	✓	✓	✓	✓	✓
State fixed effects	✓	✓	✓	✓	✓
Wealth Quintile fixed effects	✓	✓	✓	✓	✓
School years	✓	✓	✓	✓	✓
Δ Mkt. Access	✓	✓	✓	✓	✓

Notes: This table reports the coefficient β_3 from Equation 3, using binary indicators for women's reported civil status as outcome variables. The sample includes women aged 15–30 who were not mothers five years before the survey. Columns (1)–(2) refer to women who have ever been in a cohabiting relationship, while columns (3)–(5) refer to women who have never cohabited. All regressions include controls for years of schooling and change in market access, as well as age, wealth quintile, state, and urban–rural fixed effects. All regressions are weighted using survey weights, and standard errors clustered at the AGEB level are shown in parentheses. Significance levels: *** 0.01, ** 0.05, * 0.10.

Table A8: Summary Statistics Businesses

Business Name	**Code**	**N. 2011**	**N. 2016**	**Growth Rate**	**N. Treated Women**
Bakery	311812	44965	55385	0.23	1721
Beauty Salon	812110	152355	181269	0.19	838
Candy Shop	461160	36641	43953	0.20	2601
Car Workshop	811111	69891	84135	0.20	1507
Carparts	468211	30154	38436	0.27	3050
Dentist	621211	45459	54299	0.19	2424
Hardware	467111	46078	61070	0.33	1886
Pawnshop	466410	32605	44418	0.36	4362
Pharmacy	464111/464112	46341	56218	0.21	1535
Poultry Shop	461122	36727	46572	0.27	2497
Religious Center	813210	59237	76838	0.30	844
Restaurant	72251*	379962	519874	0.37	603
Tortilleria	311830	85837	100587	0.17	767

Notes: This table reports summary statistics for the business categories used in the analysis. The column *Code* corresponds to the NAICS industry code of each business. For restaurants, I include all establishments with codes beginning with 72221 in DENUE 2011 and 72251 in DENUE 2016. *N. 2011* and *N. 2016* represent the total number of businesses in Mexico in each year. The *Growth Rate* is calculated as the proportional change in the number of businesses between 2011 and 2016, defined as $(N_{2016} - N_{2011})/N_{2011}$. The column *N. Treated Women* refers to women in the ENDIREH survey who were under age 30, not mothers five years before the survey, and who lived within one kilometer of a newly opened business—i.e., no such business within one kilometer in 2011 but at least one in 2016.

Table A9: Bounds on the Effect of Pharmacy Access on Violent Behavior V^M

	Violent Partner (LB)		Violent Partner (UB)	
	(1)	(2)	(3)	(4)
Post × Treat	-0.019	-0.020	0.032*	0.031*
	(0.019)	(0.019)	(0.017)	(0.018)
Observations	8,635	8,635	8,635	8,635
Mean Dep.Var	0.036	0.036	0.034	0.034
Rural/Urban fixed effects		✓		✓
State fixed effects		✓		✓
Wealth Quintile fixed effects		✓		✓
School years		✓		✓
Age		✓		✓
Δ Mkt. Access		✓		✓

Notes: Each column reports the coefficient β_3 from Equation 3. The sample includes women aged 15–30 who were not mothers five years before the survey. The dependent variable, *Violent Partner* (V^M), equals one if the woman experienced physical violence at any point in her relationship with the partner she had five years before the survey. Columns (1)–(2) report lower-bound estimates, imputing violence outcomes to minimize the treatment effect, while Columns (3)–(4) report upper-bound estimates, imputing outcomes to maximize the treatment effect (see Appendix E.E for details). Treatment and control groups are defined as in Table A3. Column (2) and (4) control for school and change in market access, as well as age, wealth quintile, state, and urban–rural fixed effects. All regressions are weighted using survey weights, and standard errors clustered at the AGEB level are reported in parentheses. Significance levels: *** 0.01, ** 0.05, * 0.10.

Table A10: Help-Seeking Behavior

	Judicial	Municipality	Social Services
	(1)	(2)	(3)
Post × Treat	-0.016	-0.003	-0.022*
	(0.012)	(0.012)	(0.011)
Observations	8,597	8,597	8,597
Mean Dep.Var	0.005	0.005	0.005
Age fixed effects	✓	✓	✓
State fixed effects	✓	✓	✓
Rural/Urban fixed effects	✓	✓	✓
Wealth fixed effects	✓	✓	✓
School years	✓	✓	✓
Δ Mkt. Access	✓	✓	✓

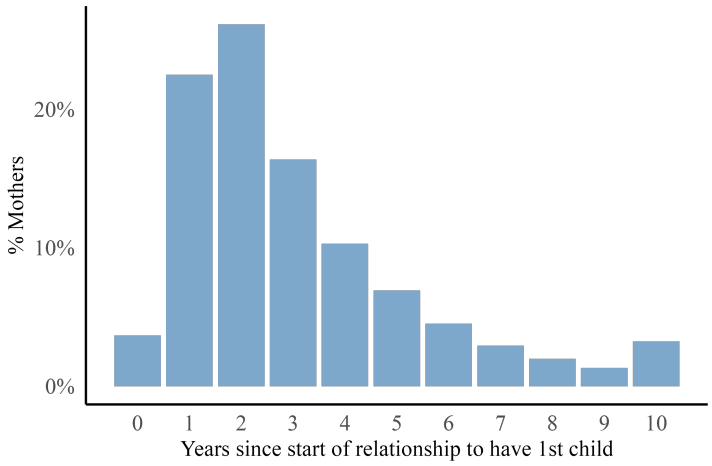
Notes: Each column reports the coefficient β_3 from Equation 3, using binary indicators for where women report violence as outcome variables. The sample includes women aged 15–30 who were not mothers five years before the survey. *Judicial* refers to reporting to the police or the public prosecutor's office; *Municipality* refers to reporting to the local municipal government; and *Social Services* refers to reporting to the *DIF* (public agency for family and child welfare) or *INMUJERES* (national women's institute). All regressions control for years of schooling and change in market access, and include age, wealth quintile, state, and urban–rural fixed effects. Regressions are weighted using survey weights, and standard errors clustered at the AGEB level are reported in parentheses. Significance levels: *** 0.01, ** 0.05, * 0.10.

Table A11: Alternative Control Group

	Mother (1)	Violence Exposure (2)	Violent Partner (3)	Separation (4)
Post × Treat	-0.062*	-0.030*	0.015	0.053
	(0.037)	(0.018)	(0.017)	(0.037)
Observations	34,052	34,054	34,054	34,054
Mean Dep.Var	0.286	0.044	0.029	0.274
State fixed effects	✓	✓	✓	✓
Rural/Urban fixed effects	✓	✓	✓	✓
Wealth Quintile fixed effects	✓	✓	✓	✓
School Years	✓	✓	✓	✓
Age	✓	✓	✓	✓
Δ Mkt. Access	✓	✓	✓	✓

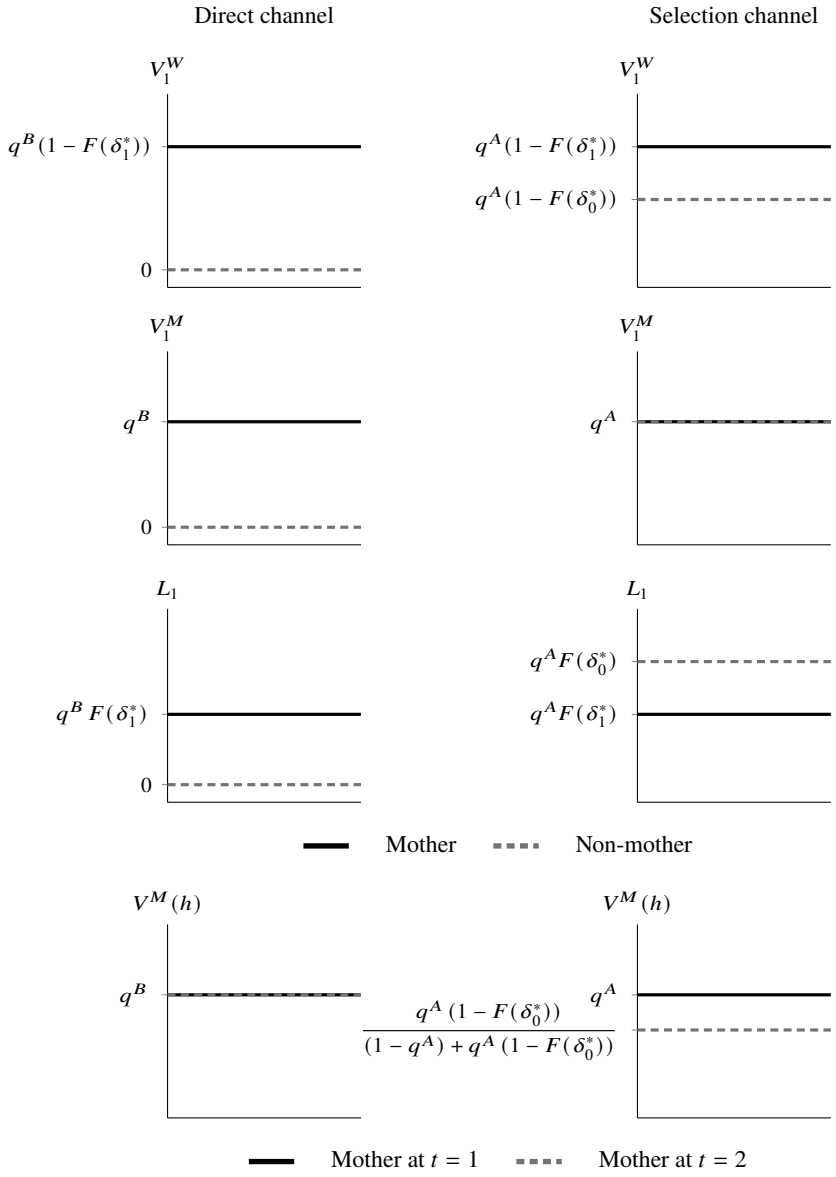
Notes: Each column reports the coefficient β_3 from Equation 3. The sample includes women aged 15–30 who were not mothers five years before the survey. *Mother* equals one if the woman is a mother at the time of the survey; *Violence Exposure* equals one if she experienced physical intimate partner violence in the last 12 months; *Violent Partner* equals one if she experienced physical violence at any point in the relationship with the partner she had five years before the survey; and *Separation* equals one if the woman is separated or single with an ex-boyfriend at the time of the survey. Treated women are those who did not have a pharmacy within one kilometer in 2011 but had at least one in 2016, while the control group includes women who had a pharmacy within one kilometer in both years. All regressions control for years of schooling and change in market access, and include age, wealth quintile, state, and urban–rural fixed effects. Regressions are weighted using survey weights, and standard errors clustered at the AGEB level are reported in parentheses. Significance levels: *** 0.01, ** 0.05, * 0.10.

B Figures



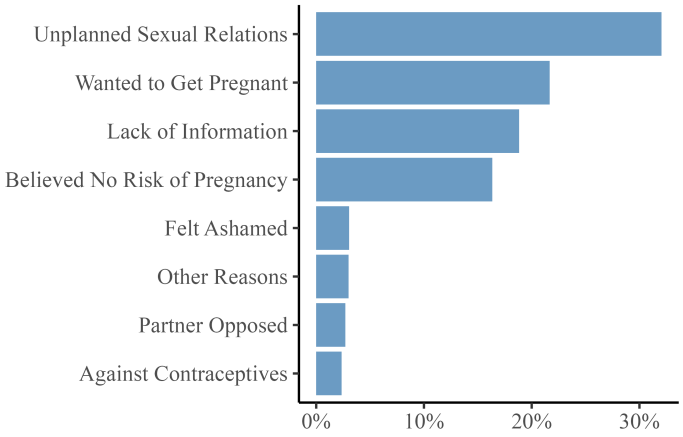
Notes: This figure shows the weighted distribution of the number of years between the start of the relationship and the birth of the first child. The variable on the horizontal axis is calculated as the woman's age at first birth minus her age when she began the relationship with her reference partner. Observations with a waiting time longer than ten years are grouped in the last category. The sample includes all women who had their first child with their reference partner.

Figure A1: Time between Start of Relationship and First Child



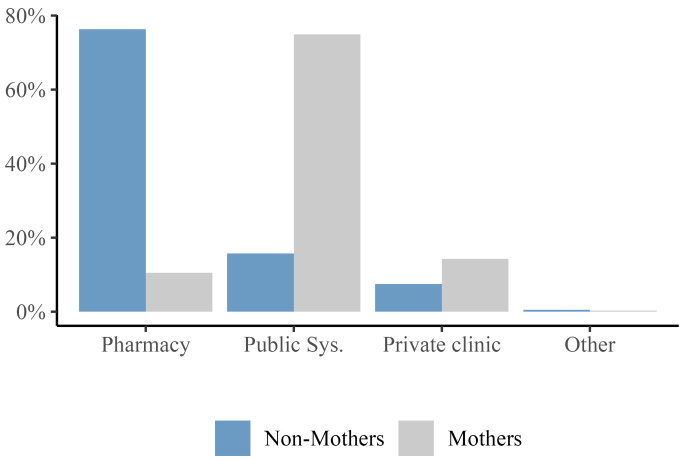
Notes: The figure summarizes model predictions under state-dependent preferences. The left column shows the direct channel ($q^B > 0$, $q^A = 0$); the right column shows the selection channel ($q^B = 0$, $q^A > 0$). Rows report, from top to bottom: women's exposure to violence in period 1 (V_1^W), men's violent behavior in period 1 (V_1^M), separation in period 1 (L_1), and cumulative exposure to a violent partner across periods ($V^M(h)$). Solid lines denote mothers / don't-wait; dashed lines denote non-mothers / wait. Symbolic y-ticks indicate levels as functions of model objects (q^A, q^B, $F(\delta_1^*)$, $F(\delta_0^*)$; see text). The plots are schematic (not data) and illustrate that under the direct channel motherhood raises V_1^M and V_1^W and increases L_1, whereas under the selection channel motherhood leaves V_1^M unchanged, raises V_1^W through selection (lower exit), lowers L_1, and delaying reduces $V^M(h)$.

Figure A2: Predictions under the State-Dependent Preferences Model



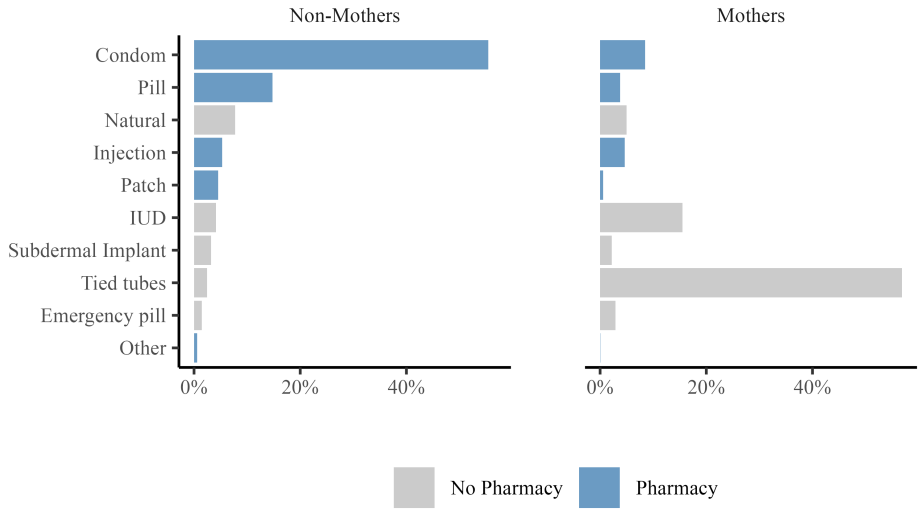
Notes: This figure shows the reported reasons for not using contraception at first sexual encounter. The sample includes women younger than 25 who have ever had a sexual relationship and reported not using any contraceptive method at first sex. Estimates use survey weights.

Figure A3: Reasons for Not Using Contraception at First Sexual Encounter



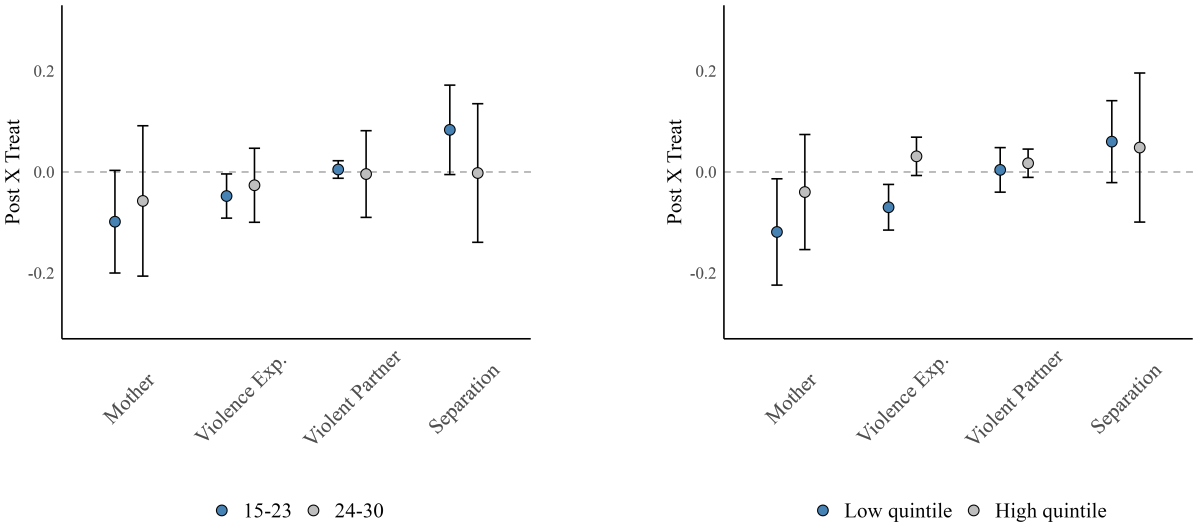
Notes: This figure shows the population-weighted share of women obtaining contraceptives from pharmacies, the public health system, private clinics, or other sources. The analysis is split by motherhood status at the time of the survey. The sample includes women who report currently using a contraceptive method. Estimates use survey weights.

Figure A4: Place of Access to Contraceptives



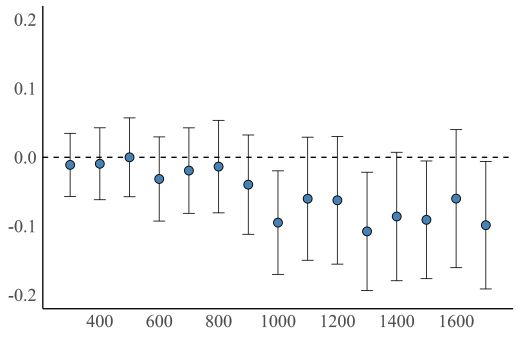
Notes: This figure shows the population-weighted share of women using each type of contraceptive method, by motherhood status at the time of the survey. The sample includes all women who report currently using contraception. Blue bars represent contraceptives that can be obtained in pharmacies, while gray bars correspond to methods that are not available in pharmacies. "Other" includes vasectomy, sponge, female condom, and other methods. "Natural" includes withdrawal and rhythm methods. Estimates use survey weights.

Figure A5: Contraceptive Use by Motherhood Status

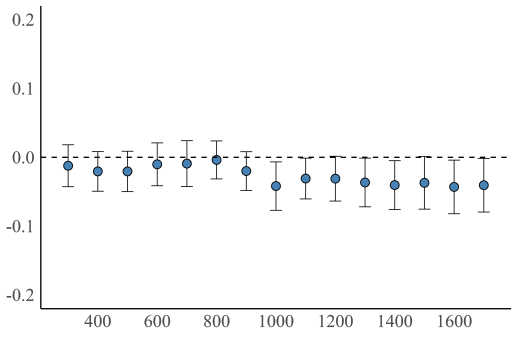


Notes: Each panel plots the coefficient β_3 from Equation 3 for women under age 30 who were not mothers five years before the survey. The left panel shows heterogeneity by age: blue dots represent women aged 15–23 and gray dots represent women aged 24–30. The right panel shows heterogeneity by wealth: blue dots correspond to women in wealth quintiles 1–3 and gray dots to those in quintiles 4–5. *Mother* equals one if the woman is a mother at the time of the survey; *Violence Exp.* equals one if she experienced physical IPV in the last 12 months; *Violent Partner* equals one if her partner from five years before the survey ever exerted violence; and *Separation* equals one if the woman is separated or single with an ex-boyfriend. All regressions control for years of schooling and change in market access, and include age, wealth quintile, urban-rural, and state fixed effects. Regressions are weighted using survey weights, and standard errors are clustered at the AGEB level.

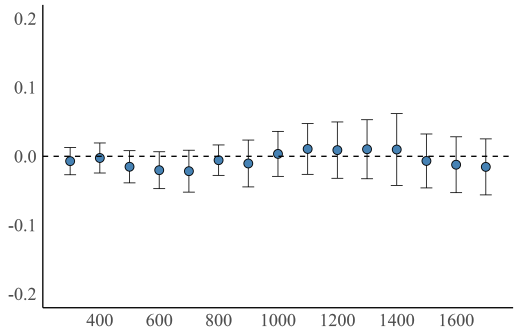
Figure A6: Heterogeneity by Age and Wealth Quintile



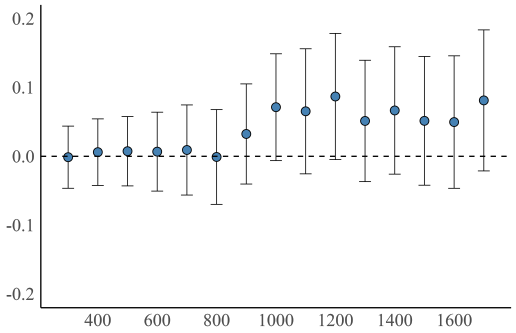
(a) Mother



(b) Violence Exposure



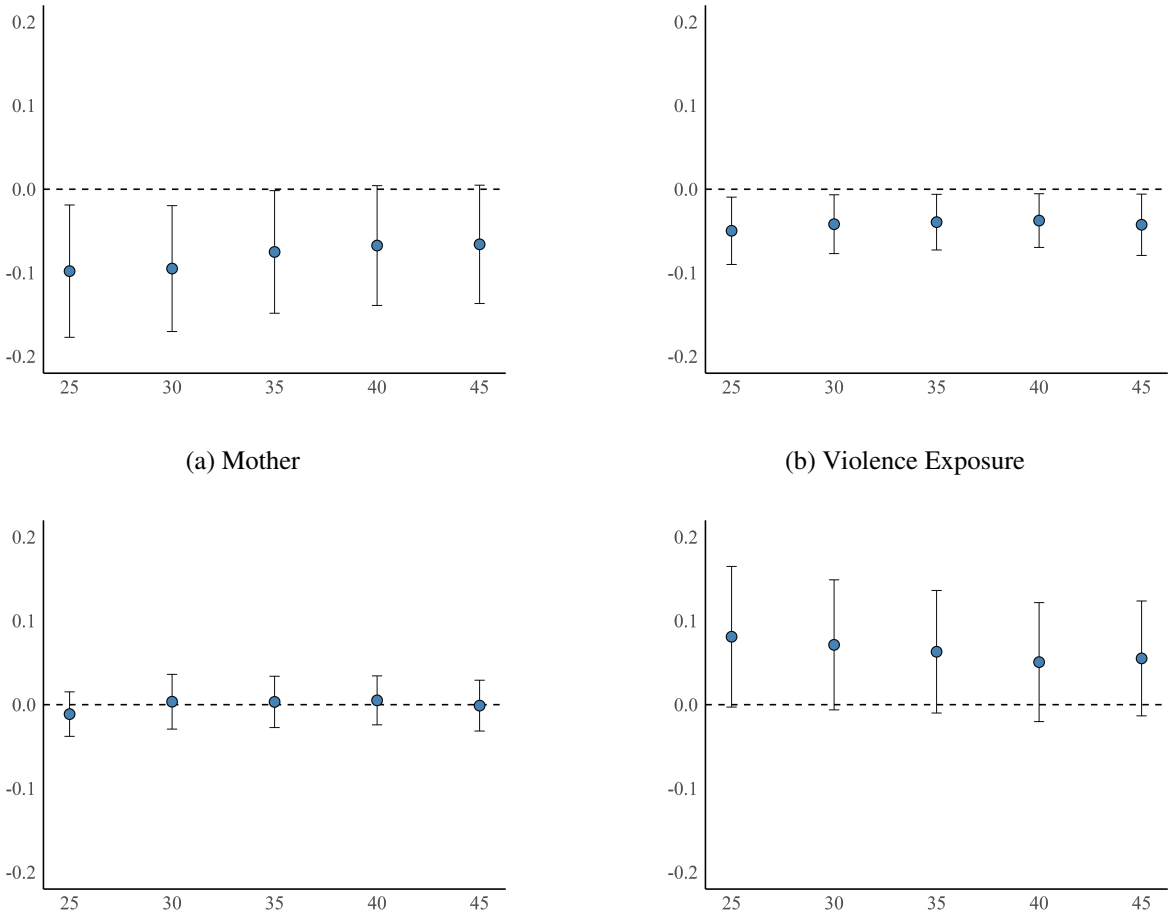
(c) Violent Partner



(d) Separation

Notes: Each panel plots the coefficient β_3 from Equation 3 using alternative distance thresholds to define treatment and control areas. The vertical axis shows the estimated effect of pharmacy openings (*Post* × *Treat*) on each outcome, and the horizontal axis indicates the distance threshold used to define treatment status. *Mother* equals one if the woman is a mother at the time of the survey; *Violence Exposure* equals one if she experienced physical IPV in the last 12 months; *Violent Partner* equals one if her partner from five years before the survey ever exerted violence; and *Separation* equals one if the woman is separated or single with an ex-boyfriend. All regressions control for years of schooling and change in market access, and include age, wealth quintile, urban-rural, and state fixed effects. Regressions are weighted using survey weights, and standard errors are clustered at the AGEB level.

Figure A7: Alternative Definition of Pharmacy Access



(a) Mother

(b) Violence Exposure

(c) Violent Partner

(d) Separation

Notes: Each panel plots the coefficient β_3 from Equation 3 estimated using alternative maximum age thresholds for the sample restriction. The vertical axis shows the estimated effect of pharmacy openings (*Post* × *Treat*) on each outcome, and the horizontal axis indicates the maximum age included in the sample. *Mother* equals one if the woman is a mother at the time of the survey; *Violence Exposure* equals one if she experienced physical IPV in the last 12 months; *Violent Partner* equals one if her partner from five years before the survey ever exerted violence; and *Separation* equals one if the woman is separated or single with an ex-boyfriend. All regressions control for years of schooling and change in market access, and include age, wealth quintile, urban–rural, and state fixed effects. Regressions are weighted using survey weights, and standard errors are clustered at the AGEB level.

Figure A8: Alternative Age Thresholds

C Theoretical Appendix

C.A Strategies

Let $h_t^m \in H_t^m$ be the information set that the man has at period t when he decides whether to exert violence. In period 1, he observes the fertility outcome. In period 2, h_2^m consists of the fertility outcomes and the woman's decision to stay or leave in the first period. Let $h^w \in H^w$ be the information set of women when they make their decision to stay or leave in the first period, after observing fertility and violence outcomes.

Let $\sigma(\delta)$ be the strategy of a woman with outside option δ and $\sigma(\alpha)$ be the strategy of a man with abusive parameter α. For convenience, I introduce notation for behavior strategies at each information set. A man's strategy is $\sigma_t^\alpha : H_t^m \to [0,1]$, the probability of exerting violence in period t for type $\alpha \in \{\alpha^{NA}, \alpha^A\}$. A woman's strategy is $\sigma^\delta : H^w \to [0,1]$, the probability of staying in period 1 for a woman with outside option δ.

Let $\hat{q}(p_1, v_1)$ denote the posterior probability that the man is abusive after observing (p_1, v_1). As I am focusing on a Perfect Bayesian Nash Equilibrium, beliefs are determined by Bayes' rule whenever possible. Hence:

$$\begin{aligned}\hat{q}(p_1, v_1) &= \text{Prob}(\alpha^A | p_1, v_1) \\ &= \frac{\text{Prob}(v_1 | p_1, \alpha^A) q}{\text{Prob}(v_1 | p_1, \alpha^{NA})(1-q) + \text{Prob}(v_1 | p_1, \alpha^A) q}\end{aligned}$$

Equilibrium: I study Perfect Bayesian Equilibrium (PBE). Formally, I solve for i) a woman's strategy σ^δ which consists of her separation decision in period one, ii) σ_t^α: the man's decision to exert violence in period 1 and period 2 and iii) $\hat{q}$: a woman's belief about her partner's violence type, after observing his behavior in period 1. The conditions for on-equilibrium behavior for i-iii are:

1. Beliefs $\hat{q}(p_1, v_1)$ respect Bayes' rule.
2. A woman's strategy $\sigma(\delta)$ maximizes her expected utility, given beliefs and her partner's strategy.
3. A man's strategy $\sigma(\alpha)$ maximizes his expected utility, given beliefs and his partner's strategy.

C.B Proof of Lemma 1

In the second period, non-abusive men compare utilities from exerting violence and from not exerting violence. Since violence yields a cost $\alpha^{NA} < 0$, the condition

$$U_2^m(p_2, v_2 = 1) = 1 + \alpha^{NA} \leq 1 = U_2^m(p_2, v_2 = 0)$$

always holds. Thus, non-abusive men never exert violence in $t = 2$.

In the first period, non-abusive men compare expected utilities from $v_1 = 0$ and $v_1 = 1$. Let $\hat{s}(p_1, v_1)$ denote the probability that the woman stays, conditional on fertility outcome p_1 and violent behavior v_1.

Expected utility from not exerting violence is

$$\mathbb{E}\big(U^m(p_1, v_1 = 0)\big) = \hat{s}(p_1, 0)(p_1 + \beta) + (1 - \hat{s}(p_1, 0))(-\bar{\delta} + p_1)(1 + \beta),$$

while expected utility from exerting violence is

$$\mathbb{E}\big(U^m(p_1, v_1 = 1)\big) = \hat{s}(p_1, 1)(p_1 + \alpha^{NA} + \beta) + (1 - \hat{s}(p_1, 1))(-\bar{\delta} + p_1)(1 + \beta).$$

A non-abusive man does not exert violence if

$$\mathbb{E}\big(U^m(p_1, v_1 = 0)\big) \geq \mathbb{E}\big(U^m(p_1, v_1 = 1)\big),$$

which rearranges to

$$\alpha^{NA} \leq \frac{\hat{s}(p_1, 0) - \hat{s}(p_1, 1)}{\hat{s}(p_1, 1)}\big(\beta(1 - p_1) + \bar{\delta}(1 + \beta)\big).$$

The right-hand side of this inequality is bounded below by $-(\beta + \bar{\delta}(1 + \beta))$. By Assumption 2, α^{NA} is strictly less than this bound. Hence, non-abusive men never exert violence in $t = 1$ either.

Therefore, their equilibrium strategy is

$$\sigma_t^{NA}(p_t) = 0 \quad \text{for } p_t \in \{0, 1\},\ t \in \{0, 1\}. \tag{C.1}$$

□

C.C Proof of Proposition 1

At the end of the first period, women decide whether to stay or leave the relationship, after observing fertility and violent outcomes. Expected utility from staying is

$$\mathbb{E}(U^{w,r}) = p_1 - v_1\bar{\alpha} + \beta\big(1 - \hat{q}(p_1, v_1)\bar{\alpha}\big),$$

while expected utility from leaving is

$$\mathbb{E}(U^{w,s}) = (1 + \beta)(-\delta + p_1 - p_1\bar{c}),$$

where $\hat{q}(p_1, v_1)$ is the updated probability that the partner is abusive after observing his behavior in period one. Women leave whenever

$$\mathbb{E}(U^{w,r}) \leq \mathbb{E}(U^{w,s}). \tag{C.2}$$

Rearranging condition (C.2), the decision rule can be expressed as a threshold on the dissolution cost:

$$\delta \leq \bar{\alpha}\frac{v_1 + \beta\hat{q}(p_1, v_1)}{1 + \beta} - p_1\bar{c} - (1 - p_1)\frac{\beta}{1 + \beta}. \tag{C.3}$$

We now consider the two possible cases:

Case 1. $v_1 = 0$**.** There are two possible scenarios. If couples are in a separating equilibrium, abusive men exert violence in the first period while non-abusive men do not. Then women perfectly infer their

partner's type, so $\hat{q}(p_1, 0) = 0$. Substituting into (C.3), the right-hand side is negative. Given that $\delta > 0$ and the right-hand side is negative, all women stay. If couples are in a pooling equilibrium, abusive men choose not to exert violence in the first period, so women cannot update beliefs and $\hat{q}(p_1, 0) = q$. By Assumptions 1 and 3, we have

$$q\bar{\alpha}\frac{\beta}{1+\beta} < \frac{\beta}{1+\beta} \leq \bar{c},$$

implying that the right-hand side of (C.3) is again negative. Thus, all women stay.

Case 2. $v_1 = 1$**.** By Lemma 2, non-abusive men never exert violence in period one. Hence, a woman who observes $v_1 = 1$ infers with certainty that her partner is abusive, i.e. $\hat{q}(p_1, 1) = 1$. Substituting into (C.3), women leave if

$$\delta \leq \bar{\alpha} - p_1\bar{c} - (1 - p_1)\frac{\beta}{1+\beta}.$$

The strategy of women can be summarized as

$$\sigma^{\delta} = \begin{cases} 0, & \text{if } v_1 = 1 \text{ and } \delta \leq \bar{\alpha} - p_1\bar{c} - (1 - p_1)\frac{\beta}{1+\beta}, \\ 1, & \text{otherwise.} \end{cases} \tag{C.4}$$

□

C.D Proof of Propositions 2

In the second period, abusive men compare utilities from exerting violence and from not exerting violence. Since violence yields a payoff $\alpha^A > 0$, the condition

$$U_2^m(p_2, v_2 = 1) = 1 + \alpha^A \geq 1 = U_2^m(p_2, v_2 = 0)$$

always holds. Thus, abusive men always exert violence in $t = 2$.

In the first period, the decision is more involved. He calculates the expected probability $\hat{s}(p_1, v_1)$ that she stays, conditional on fertility outcome p_1 and his violent choice v_1. By Proposition 1, this probability is

$$\begin{aligned} &\hat{s}(p_1, 0) = 1, \\ &\hat{s}(p_1, 1) = 1 - F\left(\bar{\alpha} - p_1\bar{c} - (1 - p_1)\tfrac{\beta}{1+\beta}\right). \end{aligned} \tag{C.5}$$

If an abusive man does not exert violence, he knows she will stay for certain. If he is violent, however, there is uncertainty about whether she will stay because δ is private information. Note that $\hat{s}(p_1, 1)$ differs between fathers and non-fathers, so I divide the analysis by fatherhood status.

Abusive fathers ($p_1 = 1$)**.** They exert violence if

$$\begin{aligned} \mathbb{E}\big(U^m(1,1)\big) &= \hat{s}(1,1)(1+\alpha^A)(1+\beta) + (1-\hat{s}(1,1))(-\bar{\delta}+1)(1+\beta) \\ &\geq 1 + \beta(1+\alpha^A) = \mathbb{E}\big(U^m(1,0)\big). \end{aligned} \tag{C.6}$$

Substituting (C.5) and rearranging, abusive fathers exert violence whenever

$$\alpha^A \geq \frac{\bar{\delta}(1+\beta)F(\bar{\alpha}-\bar{c})}{(1+\beta)(1-F(\bar{\alpha}-\bar{c}))-\beta} \equiv \alpha_1^*. \tag{C.7}$$

Abusive non-fathers ($p_1 = 0$). They exert violence if

$$\begin{aligned}\mathbb{E}\big(U^m(0,1)\big) &= \hat{s}(0,1)(\alpha^A + \beta(1+\alpha^A)) + (1-\hat{s}(0,1))(-\bar{\delta})(1+\beta) \\ &\geq \beta(1+\alpha^A) = \mathbb{E}\big(U^m(0,0)\big).\end{aligned} \tag{C.8}$$

Substituting (C.5) and rearranging, abusive non-fathers exert violence whenever

$$\alpha^A \geq \frac{\bar{\delta}(1+\beta)F\left(\bar{\alpha}-\frac{\beta}{1+\beta}\right)}{(1+\beta)(1-F(\bar{\alpha}-\frac{\beta}{1+\beta}))-\beta} + \frac{\beta F\left(\bar{\alpha}-\frac{\beta}{1+\beta}\right)}{(1+\beta)(1-F(\bar{\alpha}-\frac{\beta}{1+\beta}))-\beta} \equiv \alpha_0^*. \tag{C.9}$$

Comparison of thresholds. If $\alpha^A < \min\{\alpha_1^*, \alpha_0^*\}$, all abusive men choose $v_1 = 0$. If $\alpha^A > \max\{\alpha_1^*, \alpha_0^*\}$, all abusive men choose $v_1 = 1$. To establish the intermediate region, we need to know which threshold is larger.

$$\alpha_0^* - \alpha_1^* = \frac{\bar{\delta}(1+\beta)\Big[F(\bar{\alpha}-\frac{\beta}{1+\beta}) - F(\bar{\alpha}-\bar{c})\Big] + \beta F(\bar{\alpha}-\frac{\beta}{1+\beta})\Big((1+\beta)(1-F(\bar{\alpha}-\bar{c}))-\beta\Big)}{\Big[(1+\beta)(1-F(\bar{\alpha}-\frac{\beta}{1+\beta}))-\beta\Big]\Big[(1+\beta)(1-F(\bar{\alpha}-\bar{c}))-\beta\Big]}. \tag{C.10}$$

The denominator is positive by Assumptions 1 and 4. In the numerator, the first term is weakly positive by Assumption 1, and the second term is strictly positive by Assumption 4. Hence $\alpha_1^* < \alpha_0^*$[38]. For $\alpha^A \in [\alpha_1^*, \alpha_0^*]$, abusive fathers choose $v_1 = 1$ and abusive non-fathers choose $v_1 = 0$.

Equilibrium strategies and beliefs as a function of α^A. There are three possible ranges for α^A:

- *Low satisfaction:* $\alpha^A < \alpha_1^*$. Strategies for non-abusive men and women are as in (C.1) and (C.4). The strategy of abusive men is $\sigma_1^A(p_1) = 0$ and $\sigma_2^A(p_2) = 1$. The beliefs that support this equilibrium are $\hat{q}(p_1, 0) = q$ and $\hat{q}(p_1, 1) = 1$ (off-equilibrium).

- *Medium satisfaction:* $\alpha_1^* \leq \alpha^A \leq \alpha_0^*$. Strategies for non-abusive men and women are as in (C.1) and (C.4). Abusive men follow the following strategy

$$\sigma_t^A(p_t) = \begin{cases} 1, & \text{if } t = 1 \text{ and } p_1 = 1, \text{ or if } t = 2, \\ 0, & \text{otherwise.} \end{cases}$$

  Beliefs that support this equilibrium are $\hat{q}(1, v_1) = v_1$, $\hat{q}(0,0) = q$, and $\hat{q}(0,1) = 1$ (off-equilibrium).

- *High satisfaction:* $\alpha^A > \alpha_0^*$. Strategies for non-abusive men and women are as in (C.1) and (C.4). Abusive men's strategy is $\sigma_t^A(p_t) = 1$ for $t = 1, 2$. Beliefs that support this equilibrium are $\hat{q}(p_1, v_1) = v_1$.

[38] Note that this inequality holds for any $\bar{c} \geq \frac{\beta}{1+\beta}$. If $\bar{c} = \frac{\beta}{1+\beta}$, the first term in the numerator is zero but the second remains positive.

□

C.E Alternative model: State-dependent preferences

The proof structure of this alternative model closely mirrors that of the baseline model. I highlight where the new assumptions enter, but the reader should not consider this as a standalone model. Its purpose is simply to show that the qualitative predictions of the baseline carry over under an alternative microfoundation.

In the baseline model, men's utility from violence is fixed; the direct channel arises because abusive non-fathers strategically hide their type in t=1. Here I replace that microfoundation with state-dependent preferences: a man's payoff from violence $\alpha(p_t)$ depends on fatherhood. I assume the payoff is high enough that any man with $\alpha(p_t) = \alpha^A > 0$ exerts violence, and that a subset of men derive satisfaction from violence only when they are fathers. This removes strategic concealment and makes fatherhood itself the trigger for violent behavior. The qualitative predictions match the baseline.

Environment

The setting is as in the baseline model, except that satisfaction from violence depends on fatherhood status, $\alpha(p_t)$. A partnered man's period payoff is

$$U_t^{m,r} \;=\; p_t \;+\; v_t\,\alpha(p_t).$$

There are three types of men:

- *Always violent*: $\alpha(0) = \alpha(1) = \alpha^A > 0$.
- *Violent if father*: $\alpha(0) = \alpha^{NA} < 0$ and $\alpha(1) = \alpha^A > 0$.
- *Never violent*: $\alpha(0) = \alpha(1) = \alpha^{NA} < 0$.

Let $q^A, q^B, q^N \geq 0$ with $q^A + q^B + q^N = 1$ denote the population shares. I rule out the case where men are violent as non-fathers but non-violent as fathers.

Information follows fertility timing. Fathers observe $\alpha(1)$ in t=1. Non-fathers observe $\alpha(0)$ in t=1; if $\alpha(0) = \alpha^{NA}$, they understand that upon becoming fathers they will draw $\alpha(1) = \alpha^A$ with posterior probability $q' = \frac{q^B}{1-q^A}$. This captures uncertainty about stress-induced changes at fatherhood.

Assumptions. This version of the model relies on the same Assumptions 1 and 4 as in the baseline model. Three modifications are required:

2′. $\alpha^{NA} < -(\beta + \bar{\delta}(1+\beta) + \beta q' \alpha^A)$.

3′. $q'\bar{\alpha} < 1$.

5. $\alpha^A > \underline{\alpha}_0^*$, where $\underline{\alpha}_0^*$ is defined in (C.9).

Assumptions 2' and 3' parallel Assumptions 2 and 3 in the baseline model: they ensure that men with negative satisfaction for violence do not exert it and that the share of men who become violent only once fathers is low enough. Assumption 5 guarantees that men with $\alpha(p_t) = \alpha^A$ always exert violence, ruling out strategic concealment of type.

Equilibrium

Men with disutility for violence. All men with $\alpha(p_t) = \alpha^{NA}$ do not exert violence in either period. In $t = 2$, violence yields a direct negative payoff. In $t = 1$, if $p_1 = 1$ he is certain to be never-violent, so again he does not exert violence. See Appendix C.B for these proofs.

If $t = 1$ and $p_1 = 0$, he is either never-violent or violent-if-father. Expected utility from not exerting violence is

$$\mathbb{E}[U^m(p_1 = 0, v_1 = 0)] = \hat{s}(0,0)\,\beta(1 + q'\alpha^A) - \bar{\delta}(1+\beta)(1 - \hat{s}(0,0)),$$

while from exerting violence it is

$$\mathbb{E}[U^m(p_1 = 0, v_1 = 1)] = \hat{s}(0,1)(\alpha^{NA} + \beta(1 + q'\alpha^A)) - \bar{\delta}(1+\beta)(1 - \hat{s}(0,1)).$$

Note that he is taking into consideration the possibility that he will become violent if father. He does not exert violence if the first expression is weakly greater, which requires

$$\alpha^{NA} \leq \frac{\hat{s}(0,0) - \hat{s}(0,1)}{\hat{s}(0,1)}\left(\beta + \bar{\delta}(1+\beta) + \beta q'\alpha^A\right).$$

The right-hand side is bounded below by $-(\beta + \bar{\delta}(1+\beta) + \beta q'\alpha^A)$. By Assumption 2′, α^{NA} is strictly less than this bound. Hence, all men who have a negative satisfaction for violence do not exert violence in either period.

Women. The strategy of women is identical to the baseline model and is summarized in (C.4). I outline the reasoning; see Appendix C.C for details.

1. If $v_1 = 1$, she knows her partner will be violent again in $t = 2$ ($\hat{q} = 1$). She stays if

$$\delta \;\geq\; \bar{\alpha} - p_1\bar{c} - (1 - p_1)\tfrac{\beta}{1+\beta}.$$

2. If $v_1 = 0$ and $p_1 = 1$, the partner must be never-violent ($\hat{q} = 0$), so all women stay.
3. If $v_1 = 0$ and $p_1 = 0$, the partner is either never-violent or violent-if-father. Then $\hat{q} = q'$, and she stays if

$$\delta \;\geq\; \tfrac{\beta}{1+\beta}(q'\bar{\alpha} - 1).$$

   By Assumption 3′, this RHS is negative, so all women stay.

Men with positive satisfaction for violence. In period 2, all men with $\alpha(1) = \alpha^A$ exert violence, since violence yields a strictly positive payoff and women cannot exit.

In period 1, a man with $\alpha(p_1) = \alpha^A$ compares the utility of exerting violence to the utility of waiting. As shown in Appendix C.D, there exist two thresholds, α_1^* for fathers and α_0^* for non-fathers, with $\alpha_0^* > \alpha_1^*$ such that abusive fathers exert violence if $\alpha^A \geq \alpha_1^*$ and abusive non-fathers exert violence if $\alpha^A \geq \alpha_0^*$. By Assumption 5, $\alpha^A > \alpha_0^*$, so all men with $\alpha(p_1) = \alpha^A$ exert violence in $t = 1$, regardless of fatherhood status.

Channels

In the baseline model, the distinction between channels comes partly from men's strategies. Under the direct channel, all abusive non-fathers conceal their type in t=1 and reveal it only once they become

fathers. Hence, there are violent-if-father men (i.e., abusive men who delay violence until fatherhood) but no always-violent men. Under the selection channel, abusive men exert violence regardless of fatherhood status, so there are always-violent men but no violent-if-father type.

In the alternative model with state-dependent preferences, I preserve this dichotomy by changing only the microfoundation for why men exert violence. This framework could in principle accommodate richer environments with three types of men (always violent, violent-if-father, and never violent). For comparability with the predictions of the baseline model, however, I restrict attention to the two polar cases:

- *Direct channel:* $q^B > 0$, $q^A = 0$, and $\bar{c} \geq \frac{\beta}{1+\beta}$. There are some men who derive a positive satisfaction from violence only once they are fathers, so fatherhood directly triggers violent behavior. The predictions of the model are the same qualitatively for all $\bar{c} \geq \frac{\beta}{1+\beta}$.
- *Selection channel:* $q^B = 0$, $q^A > 0$, and $\bar{c} > \frac{\beta}{1+\beta}$. There are always-violent and never-violent men; fatherhood does not change violent behavior, but mothers are strictly more likely to remain after violence.

Predictions

Figure A2 summarizes the predictions of the alternative model. The outcomes V_1^W, V_1^M, L_1, and $V^M(h)$ are defined in the main text. All predicitions are identical to the predictions of the baseline model.

D Collider Bias from Conditioning on Civil Status

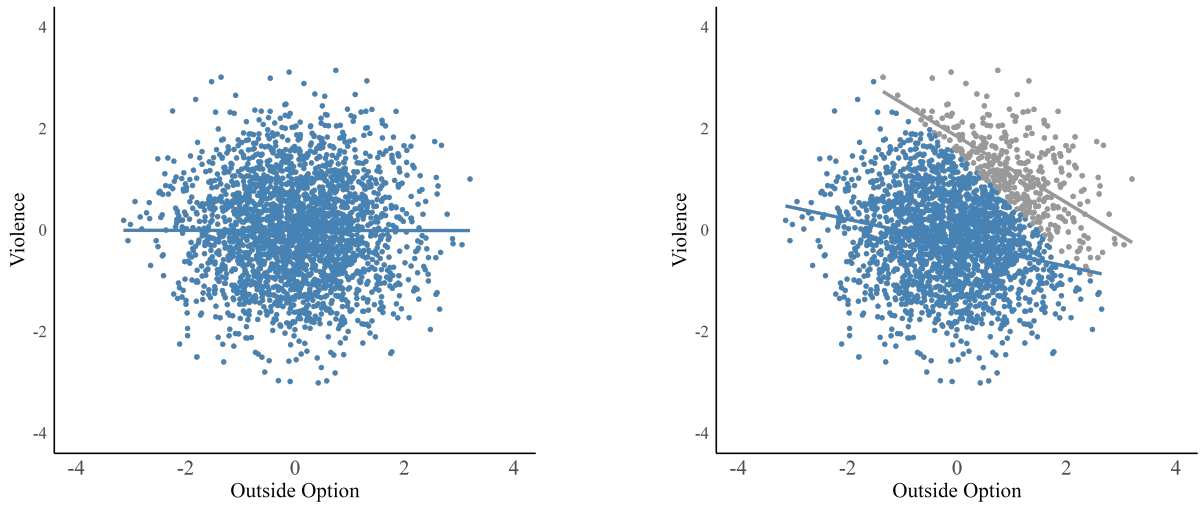
Conditioning on relationship status is common in IPV research. Table A1 documents that approximately half of recent papers in economics journals either restrict their sample to partnered women or control for civil status. While this practice may be appropriate for some research questions, it can introduce a form of sample selection bias known as collider bias when the treatment of interest affects both violence and separation.

The source of this bias can be understood using a Directed Acyclic Graph (DAG), a tool used to represent causal relationships between variables (Pearl, 1995). DAGs are now widely used across disciplines, including computer science and machine learning (Chernozhukov et al., 2024) and, more recently, in economics (Hünermund and Bareinboim, 2023). In a DAG, a variable is a collider if it is jointly determined by two other variables: $X \rightarrow Z \leftarrow Y$. Conditioning on Z induces a non-causal association between X and Y, even when no causal link exists (Cinelli et al., 2024).

In the context of intimate partner violence, separation (L) can be a collider when it is affected both by the treatment of interest (T) and by men's violent behavior (V^M). The structure is $T \rightarrow L \leftarrow V^M$. When researchers restrict the sample to partnered women or control for civil status, they are effectively conditioning on L, which induces a non-causal relationship between T and V^M. Since exposure to violence (V^W) is a function of V^M, conditioning on civil status can also distort inference about changes in exposure to violence.

Consider a treatment that improves women's outside options. If both higher outside options and higher levels of violence increase the probability of separation, then conditioning on relationship status induces a spurious correlation between outside options and violence, even when the two are independent in the population. Figure A9 demonstrates this through simulation following the example in Cunningham (2021). I draw women's outside options (O) and men's violent behavior (V^M) independently from standard normal distributions, so there is no relationship between them by construction. Separation depends on both variables: a woman separates if $O + V^M$ exceeds the 85th percentile of their joint distribution.

Panel (a) shows the full sample. There is no correlation between outside options and violence, as expected. Panel (b) restricts to partnered women (blue dots). A clear negative correlation emerges: among partnered women, those with higher outside options appear to have partners with lower levels of violent behavior. This correlation is entirely spurious. Women with both high outside options and violent partners have left and are excluded from the sample.



(a) Without controlling for separation

(b) Controlling for separation

Notes: This figure illustrates that conditioning on separation induces a spurious negative correlation between O and V^M, even though they are independent in the population. The outside option (O) and violent behavior (V^M) are drawn from independent standard normal distributions, $O, V^M \sim N(0,1)$. Consistent with the DAG, both higher outside options and higher levels of violence increase the probability of separation ($O \rightarrow L \leftarrow V^M$). A woman separates if $O + V^M$ exceeds the 85th percentile of their joint distribution. Blue dots represent women who remain partnered, and gray dots represent those who separate.

Figure A9: Illustration of Collider Bias from Conditioning on Separation

This simulation illustrates the general principle that conditioning on separation creates spurious correlations. In the motherhood context, the direction of the bias reverses. If motherhood reduces women's outside options (making them less likely to separate) and violence increases separation, then among partnered women, mothers will appear more likely to have violent partners. Non-mothers with violent partners are more likely to have left and are therefore excluded from the partnered sample, while mothers with violent partners are more likely to have stayed. Section IV provides empirical evidence of this bias using the first partner sample.

E Data Appendix

E.A IPV Variable Construction

A. Physical Violence

To create the binary variable of current physical violence, I create a binary variable that equals one if the woman answers yes to any of the following questions:

During the last 12 months has you partner (ex-partner)..

- Pushed you or pulled your hair?
- Kicked you?
- Thrown an object at you?
- Hit you with his hands or other objects?
- Tried to strangle or suffocate you?
- Assaulted you with a knife or razor?
- Shot you with a weapon?

To create the binary variables of ever physical violence, I create a binary variable that equals one if the woman answers yes to any of the the question regarding whether the partner (ex-partner) has done any of the previous actions at any point in the relationship.

B. Emotional Violence

To create the binary variable of current emotional violence, I create a binary variable that equals one if the woman answers yes to any of the following questions:

During the last 12 months has you partner (ex-partner)..

- Embarrassed, belittled, or humiliated you?
- Ignored you, not taken you into account or not given you affection?
- Told you that you are cheating on him?
- Made you feel afraid?
- Spied on you?
- Stopped talking to you?
- Become very angry because house work is not ready, because the food is not the way he wants or because he believes you did not fulfill your duties?
- Threatened you with a weapon?
- Threatened to kill you or himself?

C. Sexual Violence

To create the binary variable of current sexual violence, I create a binary variable that equals one if the woman answers yes to any of the following questions:

During the last 12 months has you partner (ex-partner)..

- Demanded that you have sexual relations, even if you didn't want to?
- Used his physical force to force you to have sexual relations?

E.B Sample of first cohabitation

The first partner is the reference partner for 95.2 percent of women in the sample, either as the current partner or as the last partner if separated.

For the 4.8 percent of women who separated from their first partner and subsequently entered a new union, the reference partner in the survey is their most recent partner. Within this group, 61.5 percent report never experiencing physical violence with any previous partner. This implies there is no evidence of violence with their first partner, so I code $V^W = 0$ and $V^M = 0$.

The remaining 38.5 percent of re-partnered women (equivalent to 1.85 percent of the full sample) report having experienced physical violence with a previous partner. For this group, I code $V^M = 1$. For the last-year violence indicator, V^W, I assume that they did not experience violence in the past twelve months from their first partner, consistent with the fact that separation reduces exposure to violence, so I code $V^W = 0$. Results are similar if I instead assume that 10 percent experienced violence in the last year,[39] or if I exclude this group entirely.

E.C Constructing access to pharmacies

This appendix describes how I constructed measures of access to pharmacies while preserving the confidentiality restrictions imposed by INEGI. Respondent locations in the ENDIREH are available only at the block level and can be accessed exclusively through the *Laboratorio de Microdatos*. Each woman in the survey is assigned a unique block identifier, constructed as a concatenation of the state, municipality, locality, AGEB (Basic Geo-Statistical Area), and block codes.

Because external georeferenced data cannot be directly introduced into the secure environment, I created a separate dataset containing block-level measures of proximity to pharmacies and submitted it to INEGI for integration. The steps were as follows:

1. Downloaded the 2010 block-level maps for all 2,456 municipalities from INEGI, covering both urban and rural blocks in pre- and post-census versions (9,824 shapefiles in total), and merged them into a unified geospatial dataset.
2. Extracted the location of all pharmacies and other types of businesses from DENUE (2011 and 2016 versions).

[39] This assumption is motivated by the fact that, on average, these women have been in their new relationship for 2.5 years, implying they have been separated from their first partner for at least three years. Figure 1b shows that among women who have been separated for three years, approximately 10 percent continue to experience violence after separation.

3. Computed the distance between the centroid of each block and the nearest pharmacy (and other types of businesses) using latitude and longitude coordinates, and constructed block-level measures of access for 2011 and 2016.

4. Submitted this dataset to INEGI, which incorporated it into the *Laboratorio de Microdatos*.

5. Within the *Laboratorio de Microdatos*, identified the relevant blocks for each woman in the survey and merged the dataset with the block-level measures of access.

6. Exported the final dataset, which includes measures of access to pharmacies and other types of businesses for each woman, without disclosing any block identifiers.

E.D Sampling frames

The sampling design of ENDIREH differs across survey waves. Because these differences affect who is interviewed within households, I harmonize the data to ensure comparability across periods.

2006. Within each household, interviewers could select up to two women. If at least one single woman was present, one single woman was selected. In addition, one ever-partnered woman was selected, defined as a woman who was either currently partnered (married or cohabiting) or separated/divorced/widowed, with priority given to partnered women when both types were present. As a result, households could contribute one single woman, one ever-partnered woman, or both, depending on their composition.

2011. All eligible women in the household were interviewed.

2016. One eligible woman per household was selected at random.

These differences imply that the distribution of civil status among respondents can vary mechanically across waves. To ensure comparability, I replicate the within-household selection rules as follows. For the main analysis (2011–2016), I randomly select one woman per household in the 2011 data to emulate the 2016 design. For the placebo analysis (2006–2011), I replicate the 2006 selection rule using the 2011 data: each household contributes one single woman (if any) and one ever-partnered woman (partnered if available, otherwise separated/widowed). The resulting resampled 2011 data are combined with the 2006 wave to form the placebo sample. All random draws use a fixed seed (123) for reproducibility.

E.E Bounding the effect of pharmacy openings on violent behavior

For about 37 percent of women, information on whether the partner from five years before the survey ever exerted physical violence is missing. These women began their relationship with their reference partner within the last four years, and the survey lacks sufficient data to determine whether they had a different partner five years earlier and, if so, whether that partner was abusive. Assigning an upper bound of one for all such cases, as in Manski (1990), would be empirically implausible given observed violence rates, which are 38 percent among separated women and 4.7 percent among women with ex-boyfriends, and would yield uninformative results in this context.

Instead, I adopt a bounding strategy inspired by the partial identification framework of Manski and Pepper (2000) and Manski (2005). I assume that the prevalence of violence among women with missing histories is not higher than among otherwise similar women for whom this information is observed. This assumption is plausible because missing information arises mechanically from the survey design rather than from nonresponse: violence outcomes are unobserved simply because women began their current

relationship relatively recently. The resulting upper bound is conservative, since it further assumes that all women with missing information were in a relationship five years earlier, which likely overstates their exposure to potential violence.

The bounding strategy is implemented in three steps.

Step 1. Classifying women into uncertainty categories

Based on the available relationship histories and age at first cohabitation, women are classified into one of three uncertainty categories. These uncertainty categories apply only to women for whom I cannot determine whether they had a partner five years before the survey, namely those who began their relationship with the reference partner within the last four years.

1. Uncertain prior violence: women who report having experienced violence with a previous partner and could plausibly have been with him five years before the survey.

2. Uncertain prior partner: women who could have had a cohabiting partner five years before the survey.

3. Uncertain prior boyfriend: women who could have had a boyfriend five years before the survey.

When a woman could plausibly have had either a boyfriend or a cohabiting partner five years before the survey, I classify her as *uncertain prior partner*, because the prevalence of violence is substantially higher among separated women than among those with ex-boyfriends. Likewise, if I have additional information indicating that she experienced physical violence with a partner who could plausibly have been in a relationship with her five years earlier, I classify her as *uncertain prior violence*. In both cases, I adopt the conservative classification that maximizes the possible prevalence of prior violence.

The categorization varies slightly between survey years:

2011. Partnered women are asked whether they had any previous cohabiting partners (other than the reference partner) and whether the partner immediately preceding the reference partner was violent. The survey does not record when these previous cohabiting relationships started or ended, so the last cohabiting partner may not be the partner she was with five years before the interview. I use information on the time since first cohabitation to determine whether it is plausible that a woman could have had a cohabiting partner five years earlier. Women whose first cohabitation began within the last four years are classified as *uncertain prior boyfriend*. If their first cohabitation began more than five years earlier, a report of violence with the last cohabiting partner is coded as *uncertain prior violence*, while a report of no violence is coded as *uncertain prior partner*.

Separated women have violence information only for their reference partner. Because all women began their reference relationship within the last four years, separated women are classified as *uncertain prior boyfriend* if their first cohabitation also began within the last four years; otherwise they are classified as *uncertain prior partner*. Single women are asked about violence with the reference (ex)-boyfriend only, and since their reference relationship necessarily began within the last four years, all single women are classified as *uncertain prior boyfriend*.

2016. Partnered and separated women are asked whether they have had any other cohabiting relationships and whether there was violence in *any* of them. Women whose first cohabitation began within the last four years are classified as *uncertain prior boyfriend*. If their first cohabitation began more than five years earlier, women who report violence in any previous cohabiting relationship are classified

as *uncertain prior violence*, while those who report no violence are treated as non-violent and excluded from the uncertainty categories. Single women are asked about violence with any previous boyfriends. Those who report violence are classified as *uncertain prior violence*, while those who report no violence are treated as non-violent and are excluded from the uncertainty categories.

Step 2. Assigning individual lower and upper bounds

The lower bound is set to zero for all women. The upper bound depends on the uncertainty category. For women classified as *uncertain prior violence*, it is set to one. For all other women, I assume they are similar to those for whom information is observed within the same treatment-year cell, since the missing information arises from survey design rather than non-response. Women classified as *uncertain prior boyfriend* are assigned the average prevalence of violence among women with ex-boyfriends in the same treatment-year cell. Women classified as *uncertain prior partner* are assigned the average prevalence of violence among separated women in the same treatment-year cell. This approach is conservative because it implicitly assumes that all women with missing information were in a relationship five years earlier, which is almost certainly not the case.

Step 3. Constructing treatment-level lower and upper bounds

Once individual lower and upper bounds are assigned, I aggregate them to obtain bounds for the treatment effect.

To construct the upper bound of the treatment effect, I use the individual lower and upper bounds as follows. Treated women in the post-period are assigned their individual upper bounds, treated women in the pre-period their individual lower bounds, control women in the post-period their individual lower bounds, and control women in the pre-period their individual upper bounds. This configuration maximizes the estimated treatment effect because it maximizes the difference for the treated group and minimizes the difference for the control group, thereby yielding the largest possible difference-in-differences estimate.

To construct the lower bound of the treatment effect, I reverse these assignments. Treated women in the post-period are assigned their individual lower bounds, treated women in the pre-period their individual upper bounds, control women in the post-period their individual upper bounds, and control women in the pre-period their individual lower bounds. Using the same logic, this configuration minimizes the estimated treatment effect.

Together, these two calculations define the range of plausible treatment effects consistent with the observed data and the conservative assumptions used to impute missing information.