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Motherhood and the onset of intimate-partner violence*

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Abstract

This paper estimates the causal impact of first-time motherhood on the onset of physical or sexual intimate partner violence (IPV). We use retrospective data from Peru's Demographic and Health Surveys (2004–2019), and conduct a stacked difference-in-differences estimation comparing first-time mothers to observationally similar women who conceive three years later, matching on pre-conception characteristics. Unlike previous studies based on administrative data, we address the key empirical challenge of constructing a credible counterfactual by exploiting detailed survey data on pre-conception relationship characteristics and incorporating these characteristics into the matching procedure. We find that motherhood increases the likelihood of IPV onset by 4.3 percentage points (a 29% increase) within two years after conception. Importantly, we find that failing to account for pre-conception relationship characteristics when constructing the comparison group leads to substantial upward bias, highlighting their importance for credibly identifying the causal effect of motherhood on IPV.

JEL Codes: J12, J13, J16, I12.
Keywords: Intimate partner violence, motherhood, cohabitation.

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1 Introduction

Intimate partner violence (IPV) is widespread and costly: nearly one in three women report having experienced some form of physical or sexual IPV in their lifetime (World Health Organization, 2025). Beyond immediate harm, IPV affects mental and physical health, labor supply, productivity, and children's outcomes (Bhuller et al., 2024; Bindler and Ketel, 2022; Aizer, 2011; Currie et al., 2022). Most policy responses, however, address IPV only after violence has occurred.[1] This raises a natural question: are there identifiable moments that systematically increase women's risk of experiencing IPV for the first time? If IPV onset is concentrated around predictable high-risk periods, preventive interventions could target periods of heightened vulnerability rather than focusing primarily on mitigating consequences after violence has already occurred. This paper studies whether the transition to motherhood causally triggers IPV onset.

The effect of motherhood on IPV is theoretically ambiguous. On the one hand, the transition to parenthood may increase exposure to a partner and intensify financial constraints, time pressure, and sleep deprivation, potentially heightening household conflict (Buddelmeyer et al., 2018; Card and Dahl, 2011; Bhalotra et al., 2025). Similarly, motherhood could reduce women's outside option due to lower labor market participation (Kleven et al., 2025; Berniell et al., 2021), allowing men to perpetrate violence with fewer consequences (Aizer, 2010; Anderberg et al., 2016). More broadly, motherhood may increase the costs of leaving a violent relationship (Deschamps, 2026). Conversely, motherhood may reduce IPV if children increase the value of relationship continuation (Becker et al., 1977). It may also reduce violence if motherhood reinforces gender-role conformity in settings where IPV partly reflects backlash against women's economic empowerment or deviations from traditional roles (Tur-Prats, 2019; Bhalotra et al., 2021; Erten and Keskin, 2021). Because theory points in opposing directions, the question is ultimately empirical.

We study the effect of first-time motherhood on the onset of IPV using rich data from the Peruvian Demographic and Health Survey (ENDES) in 2004–2019.[2] Taking advantage of the detailed history of fertility, cohabitation, and the timing of women's first experience

[1] A subset of interventions operate ex ante, notably economic-support programs that relax constraints and increase women's outside options (Buller et al., 2018) and community-based programs that shift norms and reduce tolerance for abuse (Abramsky et al., 2014). Many widely used policy tools, however, are implemented conditional on prior violence, disclosure, or assessed risk. Protection orders, specialized courts, and coordinated justice-and-support centers therefore primarily function as ex post protection and secondary prevention, aiming to deter recurrence and reduce escalation rather than preventing IPV onset (García-Hombrados et al., 2024; Sviatschi and Trako, 2024; Asik et al., 2026).

[2] We focus on the effect of motherhood on IPV at the extensive margin. A different but still relevant research question is how motherhood affects the intensity of IPV. As we explain in Section 2, we are unable to explore this question with our data.

of physical or sexual IPV by their most recent partner, we construct a panel dataset with information on the onset of IPV around the time of first conception. We implement a stacked difference-in-differences design, matching women who conceive their first child in a given year to observationally similar women who conceive three years later. Our identifying assumption is that in the absence of conception, treated mothers would have followed the same IPV trajectory as comparable women who conceived later. The key empirical challenge is therefore to identify a comparison group that credibly captures this counterfactual trajectory.

To address this challenge, we exploit the rich survey data available in ENDES and match mothers to women of the same age who share pre-conception characteristics that are directly related to future IPV risk—namely the couple's relationship history and the quality of the relationship prior to conception.[3] Unfortunately, even rich surveys such as ENDES collect retrospective information on relationship histories only from the start of cohabitation onward.[4] Thus, for women who conceive before cohabiting—a common pattern in many low- and middle-income countries (LMICs) (Appendix Figure A.1)—the only information we observe about their pre-conception partnership status is that they were not cohabiting. Matching on this information alone may pair treated women who were already in stable non-cohabiting relationships and would have begun cohabiting soon even in the absence of childbirth, with comparison women who were single at the corresponding point in time. Such matches would fail to reproduce the partnership—and therefore IPV—trajectories that treated women would have followed in the absence of conception, potentially biasing the estimated effect of motherhood upward. We address this issue by restricting our sample to women who were already cohabiting before conception, for whom detailed pre-conception relationship histories allow us to construct a comparison group with similar expected IPV trajectories in the absence of motherhood.

An additional challenge when studying the effect of motherhood on IPV is that IPV often goes unreported. A common source of information on IPV is administrative data, such as police reports and hospital records. However, these sources tend to capture only the most severe cases, representing the "tip of the iceberg" of IPV incidence. This

[3]We use civil status over the pre-conception period as a measure of the couple's relationship history, and whether IPV began during the first year of cohabitation as a proxy for relationship quality. As an alternative measure of relationship quality, we replace the indicator for IPV onset during the first year of cohabitation with indicators for the pre-conception educational attainment of the woman and her partner. The resulting estimates are similar to those from our baseline specification.

[4]Demographic and health surveys normally provide detailed cohabitation histories, including the timing of co-residence, but do not contain information on the relationship histories of women who are not cohabiting, such as whether they are in a stable non-cohabiting relationship or how long that relationship had been in place.

concern may be especially pronounced in LMICs, where weak state capacity and prevailing social norms can discourage formal reporting of gender-based violence. Consistent with this, Palermo et al. (2014) document that only seven percent of women who self-report physical or sexual violence in Demographic and Health Surveys (DHS) interviews report these incidents to formal authorities.[5] Underreporting may be particularly problematic among recent mothers, who may face financial or time constraints that reduce their ability or willingness to seek assistance from law enforcement or medical professionals. We address this challenge by relying on self-reported survey data from the domestic violence module of the ENDES survey, which captures IPV that remains largely invisible to formal institutions.

We find that first-time motherhood increases the likelihood of IPV onset by 4.3 percentage points within two years of conception, equivalent to a 29% increase relative to the comparison-group mean. This increase is driven primarily by moderate physical violence, rather than severe physical or sexual violence, underscoring the value of self-reported measures that capture forms of IPV less likely to appear in administrative records. The effects are particularly pronounced among younger and less-educated women, as well as among those with a family history of violence. These findings are robust to using women who conceived their first child two years later as the comparison group and to alternative matching specifications, outcome definitions, and sample restrictions.

We next examine how the construction of the comparison group affects the estimated effect by benchmarking our preferred specification against the approach used in a small recent literature examining the effect of motherhood on IPV with administrative data (Massenkoff and Rose, 2024; Bergvall and Rodríguez Planas, 2024; Britto et al., 2025). There are two main distinctions relative to our approach. First, because administrative records generally contain limited information on couples' relationship history, these studies typically construct the comparison group using only age.[6] Second, because their matching does not account for pre-conception relationship characteristics, they do not restrict their sample to women who cohabit prior to conception. We conduct two exercises to assess how these two departures from our preferred specification affect the estimated effect of motherhood on IPV.

In the first, we perform the stacked difference-in-differences estimation without any sample restrictions and matching only on age. We find a 265% increase in the likelihood of experiencing IPV after two years, compared with a 29% increase in our preferred

[5]For a comprehensive discussion on the challenges involved in measuring IPV, see Shah and Barski (2026).

[6]Britto et al. (2025) also match on the child's gender, municipality, and indicators for whether the mother had ever held a formal-sector job or been registered in a national registry of low-income households used to determine eligibility for social programs.

specification. However, treated women and their matched controls exhibit divergent IPV trajectories prior to motherhood, as well as sizable differences in pre-conception civil status. In the second exercise, we retain the pooled sample but exploit the additional relationship information available in ENDES to improve the identification of the comparison group. For women who cohabited before conception, we use the same matching strategy as in our preferred specification. For women who had not yet begun cohabiting, the best one can do even with detailed survey data is to match them to future mothers of the same age who were also not cohabiting during the pre-conception period and had the same age at first sexual intercourse, an imperfect proxy for relationship status. Accounting for these pre-conception relationship characteristics reduces the estimated effect to 97%.

There are two main takeaways from these exercises. First, the decline in the estimated effect from 265% to 97% when pre-conception relationship characteristics are incorporated indicates that matching only on age substantially overstates the effect of motherhood on IPV. Second, the 97% estimate from the improved pooled specification may still not identify the causal effect of motherhood on IPV, because the pre-conception relationship status of women who conceive before cohabiting remains only imperfectly observed. The gap between this estimate and the 29% effect in our preferred specification, which focuses only on women who were already cohabiting before conception, may reflect genuine treatment-effect heterogeneity, but it may also reflect residual upward bias from an inadequate counterfactual. Because we cannot distinguish between these explanations, we view our preferred specification as providing the more credible estimate, since it is based on a sample for which we observe the pre-conception relationship characteristics needed to construct a credible comparison group. Although this sample restriction limits external validity, we view the resulting gain in internal validity as essential for credibly identifying the causal effect of motherhood on IPV.

This paper contributes to the literature on the effects of motherhood on gender inequality. Most existing work in this area focuses on labor market outcomes, such as employment (Kleven et al., 2025), wages (Cukrowska-Torzewska and Matysiak, 2020), labor informality (Berniell et al., 2021), and on-the-job training (Ma et al., 2025). We extend this literature by examining IPV—an outcome that both reflects and exacerbates gender inequality, as it is disproportionately experienced by women.

This paper also relates to the literature on the determinants of IPV. Prior studies analyze the impact of factors such as the gender wage gap (Aizer, 2010), divorce laws (Stevenson and Wolfers, 2006; Brassiolo, 2016), cash transfer programs (Bobonis et al., 2013; Hidrobo and Fernald, 2013), employment shocks (Anderberg et al., 2016; Tur-Prats, 2021; Bhalotra et al., 2025; Kotsadam and Villanger, 2025), or emotional cues (Card and

Dahl, 2011), among others. We contribute to this literature by analyzing the effects of motherhood, which allows us to identify not only a key driver of IPV but also its timing and dynamics over the life cycle—an aspect that is crucial for effective prevention policies.

Finally, only a few papers lie at the intersection of these two strands of the literature and examine the effects of motherhood on IPV. Massenkoff and Rose (2024) use administrative criminal records to study the impact of family formation on crime. For domestic violence, they find that arrests increase right after birth, concentrated among fathers who eventually divorce. Britto et al. (2025) conduct a related analysis using administrative data from Brazil, and find an increase in the likelihood that men are prosecuted for domestic violence in the two years following childbirth, as well as a rise in third-party reports of domestic violence filed by health providers. In contrast, Bergvall and Rodríguez Planas (2024) use population-wide hospital records from Sweden and find that hospital visits for assault-related injuries decline during pregnancy and after childbirth. All three studies rely on administrative data—police reports, court records, or hospital visits—which capture only the most severe or formally reported cases of IPV, and provide scarce information on pre-conception relationship dynamics. We contribute to this literature in two ways. First, we use self-reported IPV data that capture violence that is invisible to formal institutions. Second, we show that using pre-conception relationship characteristics to construct the comparison group is important for credibly identifying the causal effect of motherhood on IPV.

The remainder of the paper proceeds as follows. Section 2 describes the data and sample, and Section 3 presents the empirical strategy. Section 4 reports the results, and Section 5 examines the importance of accounting for pre-conception relationship dynamics when constructing the comparison group. Section 6 concludes.

2 Context and data

2.1 The Peruvian Context

Peru is a useful setting for studying the effect of motherhood on IPV, as it exhibits high levels of intimate partner violence. As shown in the top panel of Figure A.2, the share of women aged 15–49 who reported having ever experienced physical violence from their current or most recent partner was 24% in 2019, while 5% reported having ever-experienced sexual violence. Although declining over the past two decades, this level of violence places Peru among the higher-prevalence settings worldwide: as of 2018, the Andean Latin America region ranked third globally in lifetime physical and/or sexual

IPV, with an estimated prevalence of 38% (Sardinha et al., 2022).[7]

Violence begins early in women's lives: the share reporting physical or sexual IPV in the past twelve months already exceeds 10% among those aged 20–24 and remains elevated through the mid-thirties, suggesting that much of the onset of violence occurs during the reproductive years (Appendix Figure A.3).

Despite its high prevalence, IPV in Peru largely remains invisible to formal institutions. According to ENDES data, only 27% of victims of physical or sexual IPV sought help from the police or another judicial authority, and just 14% sought care from a doctor or health facility. These patterns highlight the limitations of administrative records—such as police reports or hospital data—for measuring IPV, and underscore the value of relying on self-reported survey data.

Peru is also particularly informative for studying the link between motherhood and IPV because most women become mothers, and they tend to do so early. In the 2004–2019 ENDES, approximately 80% of women aged 15–45 had at least one child, with an average age at first birth of 20.15 years. Informal unions are the predominant form of partnership: among partnered women, 65% are in informal unions, while only 35% are legally married. The transition to motherhood and the transition to co-residence often overlap. Approximately half of the mothers in ENDES conceived their first child before beginning to live with a partner (Appendix Figure A.4), a pattern that is not unique to Peru but common across Latin America and other LMICs (Appendix Figure A.1).

2.2 Data and IPV measurement

Our main data source is Peru's DHS for the 2004–2019 period, known as the *Encuesta Demográfica y de Salud Familiar* (ENDES).[8] ENDES is a repeated cross-sectional survey that is representative at both the national and the regional levels. The survey primarily collects demographic and health data from women aged 15 to 49. It covers women's entire birth history, their marital and cohabitation history, employment status, household decision-making, gender norms, and socio-economic characteristics, among others. Additionally, it

[7]The likelihood of experiencing violence in the last twelve months was also high, albeit decreasing over the past two decades, as shown in Appendix Figure A.2. Specifically, the share of women reporting physical IPV in the last twelve months hovered around 10–12% during the first half of the period before declining to approximately 8% by 2019, while recent sexual violence remained stable at around 3%.

[8]We stop the sample before 2020 because during the COVID-19 pandemic, ENDES changed its mode of data collection, relying partly on telephone interviews before gradually returning to in-person surveys. In addition, several studies document sharp increases in intimate partner violence during lockdowns and mobility restrictions (e.g., Berniell and Facchini, 2021; Bhalotra et al., 2026; Agüero, 2021; Perez-Vincent and Carreras, 2022; Silverio-Murillo et al., 2023; Arenas-Arroyo et al., 2021). Including pandemic years could confound the motherhood effect with pandemic-related shocks to violence.

collects demographic and socio-economic characteristics for each member of the women's households, including husbands or partners and children. In addition to the standard survey, the ENDES includes a domestic violence module that focuses on women's experiences of physical, sexual, or emotional abuse from their most recent partner. While all women aged 15 to 49 participate in the standard survey, only one woman in each household is randomly selected to complete the domestic violence module.

In the domestic violence module, women are asked whether they have ever experienced different forms of IPV (emotional, sexual, or physical, including severe and moderate forms) by their current partner, or by a previous partner if they are separated.[9] Women who report experiencing any form of physical or sexual violence are asked how many years after the start of cohabitation the violence began. Since women also report when they began cohabiting, we can reconstruct the timing of physical/sexual IPV onset. Only 2% of women aged 15–49 who report ever having experienced physical or sexual violence from their current or most recent partner report that it began before cohabitation. This suggests that, in this setting, IPV onset is overwhelmingly observed within cohabiting relationships, rather than before cohabitation. This timing information is central to our empirical strategy, because, as explained in Section 3, our analysis requires reconstructing violence trajectories around conception. We take advantage of this retrospective information to reconstruct a panel dataset, and thus define our outcome as the cumulative probability of ever experiencing IPV. Since ENDES records the timing of onset only for physical and sexual violence, but not for emotional violence, we restrict the analysis to the onset of physical or sexual IPV.

A potential concern with using self-reported data is the possibility that women under-report having experienced IPV.[10] The DHS program employs several design features to reduce under-reporting of domestic violence. Interviews are conducted by trained female enumerators to foster trust, and the survey follows strict ethical and safety protocols. In particular, questions on domestic violence are asked only when privacy can be ensured; if privacy is not possible, the interviewer does not proceed with this module. In practice, this constraint led to the exclusion of only about 1% of otherwise eligible women. In addition, the fact that the domestic violence module is administered to only one eligible woman per household further reduces the risk of security breaches arising from other household members learning that sensitive information was disclosed.

Previous research has tried to measure the extent of IPV misreporting, for example,

[9] Appendix Table B.1 provides the exact wording of the questions.

[10] A number of explanations have been proposed for this, including reluctance to revisit traumatic experiences, the perception of IPV as a private matter, anticipated stigma and shame, concerns about legal consequences for the perpetrator, and the risk of economic or physical retaliation, among others.

by comparing direct methods like the DHS against experimental methods, although the evidence is mixed. On one hand, Cullen (2023) finds that, in Nigeria, an indirect list experiment yields an IPV prevalence estimate 35% higher than that obtained from face-to-face surveys. Similarly, Peterman et al. (2024) find that an audio computer-assisted self-interview increases measured IPV prevalence by 4–7 percentage points relative to the face-to-face benchmark. In contrast, other studies find that direct and experimental methods yield similar estimates of IPV in Ethiopia (Gilligan et al., 2024; Kotsadam and Villanger, 2025). Importantly for our paper, evidence from Peru indicates that IPV measures in this survey are reliable: comparing direct questions to list experiments, Agüero and Frisancho (2022) find no significant differences in reports of physical and sexual violence, supporting the use of self-reported IPV in our setting.

One concern specific to our research question is whether motherhood affects how IPV is reported. For example, women with children might fear retaliation against themselves or their children, or worry about losing custody if the household comes to the attention of authorities, and so be less likely to disclose violence truthfully (Palermo et al., 2014). In that case, our estimates would be a lower bound on the true effect of motherhood on IPV. However, mothers might also be more willing to disclose violence out of concern for their children's wellbeing (Meyer, 2010), which would instead lead to overestimates. Although this concern may be more salient for administrative data, where disclosure is more directly tied to institutional intervention, it could still affect survey-based measures. While Agüero and Frisancho (2022) do not directly examine motherhood as a source of differential misreporting, they find no significant differences between direct and indirect reporting methods across characteristics correlated with motherhood, such as age and marital status. Therefore, this absence of differential misreporting along these dimensions provides suggestive evidence that misreporting is also unlikely to vary systematically by motherhood status in this setting.

3 Estimation strategy

The retrospective reporting of IPV onset in the ENDES data allows us to move beyond cross-sectional comparisons and instead reconstruct annual trajectories around first conception. This, in turn, makes it possible to implement a stacked difference-in-differences estimation to identify the causal effect of motherhood on IPV. Following related studies (Massenkoff and Rose, 2024; Britto et al., 2025; Bergvall and Rodríguez Planas, 2024), we use women with similar pre-conception characteristics who conceived their first child three years later as the control group. As a result of this design, our treatment group

consists of women who conceived their first child between 2000 and 2016, while the pool of potential controls includes women who conceived between 2004 and 2019.[11]

We implement this stacked difference-in-differences design by defining 17 "sub-experiments," each indexed by a conception year c. For each $c \in \{2000, \ldots, 2016\}$, the treated group consists of women who conceived their first child in year c, and the control group consists of matched women who conceived exactly three years later, in year $c+3$, and shared comparable characteristics prior to c. Within each sub-experiment, we assemble an annual panel spanning four pre-conception and three post-conception years (t). For example, for a woman who conceived her first child in $c = 2010$, we build a panel for $t \in \{2006, \ldots, 2012\}$; her counterfactual over this window is obtained from women who conceived their first child in 2013 and had similar characteristics before 2010.

Using the sample of treated women and their matched controls, we estimate the following equation:

$$Y_{ict} = \sum_{j \in [-4,2], j \neq -1} \beta_j \, Treated_{ic} \times \mathbf{1}\{t - c = j\} + \gamma_{ic} + \delta_{ct} + u_{ict} \tag{1}$$

As explained in detail below, Y_{ict} is an indicator for whether woman i belonging to the sub-experiment of conception year c had experienced physical or sexual IPV from her current or former partner by year t. $Treated_{ic}$ is a binary variable taking value 1 if woman i conceived her child in year c, and 0 for her matched control who conceived three years later. Finally, γ_{ic} and δ_{ct} are individual-by-sub-experiment and sub-experiment-by-year fixed effects. We cluster our standard errors at the individual level.

Under the identifying assumption, the coefficients β_j capture the effect of first conception on the cumulative probability of having experienced IPV by period j, relative to the year before conception ($j = -1$). Because the panel is centered on conception rather than birth, β_0 captures the combined effect of pregnancy and the first three months after birth. β_1 captures the cumulative effect one year after conception, when the child has already been born, and β_2 captures the cumulative effect two years after conception.[12]

[11] The lower bound (2000) follows from data availability and our sampling window: since ENDES starts in 2004 and we include women interviewed up to four years after first conception, the earliest observable conception year is 2000. The upper bound (2016) ensures that, for each treated cohort, we can observe comparison women who conceive three years later within our data period. Treated women can also serve as controls for other treated women. In fact, almost 98% of our treated sample is also part of the control pool.

[12] For 24% of treated women, the interview takes place before the end of the two-year window following conception, so we do not observe their outcomes over the full post-conception period. Specifically, 8% are interviewed within the first twelve months after conception, while 16% are interviewed between one and

The validity of this research design rests on the parallel-trends assumption: absent conception, treated women's IPV trajectories would have evolved similarly to those of their matched controls. This is plausible in our setting because we compare women whose first births occur only a few years apart and who share similar pre-conception characteristics. Consistent with this, we find no evidence of differential trends in domestic violence outcomes prior to conception (see Section 4 below).

3.1 Panel and outcome construction

For each mother in our sample, we construct a yearly panel centered around the conception of her first child.[13] In each period, we identify whether the woman had already experienced physical or sexual IPV by her most recent partner. In order to do this, we combine retrospective information on cohabitation histories, particularly the month-year when the woman started cohabiting with her partner, with the reported timing of IPV onset relative to the start of cohabitation.

To avoid errors in recall, the ENDES survey inquires about the timing of IPV onset in yearly intervals since the beginning of cohabitation (e.g., "before the first anniversary" or "after one year"), rather than at an exact date. Because our analysis is conducted in one-year bins around conception (rather than one-year bins around cohabitation), we must assign a specific timing of onset within each reported 12-month interval in order to determine the period in which the outcome switches from zero to one. In our baseline specification, when a woman reports that violence began within a given year of cohabitation (for example, during the first year), we assign onset to the middle of that 12-month window (e.g., six months after cohabitation began). In Section 4.1, we show that our results are robust to assigning onset instead to the beginning of the 12-month window (e.g., the first month of cohabitation if violence started during the first year).

Our outcome variable is thus an indicator for whether, by that year, the woman had already experienced physical or sexual IPV from her most recent partner. We focus on the extensive margin (IPV onset) as this allows us to reconstruct the outcome variable throughout our panel with relatively few assumptions. For women who report never having experienced violence, the outcome variable is coded as zero in all periods. For

two years after conception. As a result, β_1 and β_2 are estimated using progressively smaller samples and may be affected by changes in sample composition. Reassuringly, the estimates are virtually unchanged when we restrict the sample to women observed throughout the full two-year period following conception (see Section 4.1).

[13] For women who are pregnant with their first child at the time of the interview, we back out the date of conception (month-year) using the interview date and gestational duration. For women who already had their first child, we use the date of birth and assume a gestational length of nine months.

women who report having experienced violence, the outcome is coded as zero prior to the start of cohabitation and switches to one in the year corresponding to the assigned onset of violence, remaining equal to one thereafter. For women who report experiencing physical or sexual violence prior to cohabitation, there is no information on when this violence began. Importantly, in this context IPV is intrinsically related to cohabitation, as only 2% of those who report ever having experienced physical/sexual IPV report that it began before co-residing. Given the lack of information on the timing of IPV onset for these women, we exclude from the sample the 2% who report that IPV began prior to cohabitation, as explained in further detail below. Appendix C provides additional details on the construction of the panel.

3.2 Sample and matched controls

Identifying the causal effect of motherhood on IPV requires observing the counterfactual for each woman—namely, what her experience of violence would have been had she not had a child. As is standard in this literature, we build a counterfactual for each mother using the outcomes of women who share similar characteristics up to the time of conception but have their first child later. Because women whose first births occur relatively close in time are more comparable, we use as controls women who conceive their first child three years later, which allows us to estimate the effect of motherhood up to two years after conception.[14] This implies matching treated women in each sub-experiment of conception year c to comparison women who conceived in c+3 and are comparable prior to c. In Section 4.1, we show that our results are robust to using women who conceived their first child two years later as the comparison group.

To identify comparison women with a similar underlying risk of experiencing IPV in the future, we argue that beyond the standard matching variables used in previous studies—namely age—it is crucial to match on pre-conception characteristics that are more directly related to future IPV risk, such as the couple's relationship history and relationship quality (Abramsky et al., 2011).[15] This is challenging for women who had not yet begun cohabiting at the time of conception, because even a rich survey such as ENDES has no information on their relationship status prior to conceiving (whether they

[14] To increase our sample size, our sample includes women interviewed up to four years after conceiving their first child. However, we estimate motherhood effects only up to two years after conception. Therefore, for women who are interviewed after these two years, the outcome variable will be fully reconstructed using retrospective information.

[15] Britto et al. (2025) also match on the child's gender, municipality, and indicators for whether the mother had ever held a formal-sector job or been registered in a national registry of low-income households used to determine eligibility for social programs.

were in a stable non-cohabiting relationship and for how long). Even when these mothers are matched to women who were likewise not cohabiting before conception year c, the comparison may remain imperfect. Many treated women may already have had a partner at conception and would have begun cohabiting soon afterward even in the absence of childbirth. By contrast, their matched controls, who would not conceive for another three years, may not yet have been in a stable relationship at the corresponding point in time. In that case, the matched controls would understate the IPV exposure of treated women in the counterfactual, leading to an overestimation of the effect of motherhood on IPV.

This issue is particularly relevant because almost half of the women in our sample had not yet begun cohabiting at the time they conceived their first child, as shown in Appendix Figure A.5. Peru is not an outlier in this respect, as many other developing countries display similar rates of pre-conception cohabitation, particularly in Latin America (Appendix Figure A.1). To mitigate this concern, we focus on women who were already cohabiting prior to conception. For these women, we observe their complete pre-conception cohabitation history and measures of match quality, which help us identify comparison women with a similar underlying propensity to experience IPV in the absence of motherhood.

Restricting the sample in this way allows us to exploit the rich information on pre-conception relationship dynamics available in ENDES when constructing the comparison group. In terms of the couple's relationship history, we match mothers to women with the same civil status in each of the four pre-c years.[16] To proxy match quality we use an indicator variable for whether the woman experienced violence in the first year of cohabitation. Importantly, as these women cohabited before conception, this variable is defined prior to c. As an alternative measure of match quality, we instead use the pre-conception education of both partners, and obtain similar results (see Section 4.1).[17] If there is more than one woman in the comparison group who shares the same characteristics, we use the average outcome of all matched women as the counterfactual.

[16]Women's civil status, which may change over the pre-conception period, is classified in each of the four pre-conception years as married/in union, separated, or single.

[17]We reconstruct each woman's education for the year prior to conception based on her age at that time. For example, if a woman is 19 at the time of the interview and has at least some postsecondary education, we assume that she had at least some postsecondary education when she was 18 years old, incomplete secondary education when she was between 13 and 17 years old, and incomplete primary education when she was younger than 13. We apply the same procedure to the education of their partners. This variable measures the maximum educational attainment of the woman's current or most recent partner. To reconstruct the partner's education at the year prior to conception, we use the partner's age at the time of the interview. For women who are separated at the time of the interview (9%), the age of the former partner is not recorded. In these cases, we assume that the former partner's educational attainment in the year prior to conception was the same as at the time of the interview.

Our identification strategy therefore differs from the existing literature in two important ways. First, rather than matching women only on age, we additionally match on pre-conception relationship history and relationship quality, which are key predictors of future IPV risk. Second, because these variables are observed only for women who were already cohabiting prior to conception, we restrict our main analysis to this subgroup. In Section 5, we examine the implications of both design choices by progressively relaxing these restrictions and showing how the estimated effects change.

We start with the sample of 42,430 women who were either pregnant with their first child at the time of the interview or conceived their first child between 2000 and 2019 and were selected for the domestic violence module. We drop 1,494 women who cohabited with more than one partner, because the ENDES only asks about IPV from the most recent partner. We also drop the 314 women who report having suffered IPV before cohabiting because we are not able to accurately reconstruct the timing of violence for these women. Finally, for the reasons discussed above, we drop 19,527 women (48%) who conceived prior to cohabiting.[18] We end up with a sample of 21,095 women.[19] Of these, 18,875 conceived between 2000 and 2016 and therefore contribute treated observations, while 20,692 conceived between 2004 and 2019 and belong to the pool of potential controls. These groups are not mutually exclusive, as women can serve as controls in some sub-experiments and as treated units in others.

We are able to match 14,561 (77%) of the mothers in our treatment group.[20] Table 1 compares treated women and their matched controls in terms of several pre-conception characteristics not used for matching. Importantly for identification, treated women are broadly comparable to their matched controls. In particular, the two groups are similar in whether the woman's mother experienced domestic violence—a strong predictor of women's own risk of domestic violence (Hernandez-Leal, Lu and Navajas-Ahumada, 2025; Bellés-Obrero, Mattina and Ye, 2025; Collins, 2025).

[18] Appendix Table A.1 compares the characteristics of mothers who conceived before cohabitation with those who did not. Women who did not co-reside prior to conceiving their first child are younger, and more likely to report having experienced IPV at the time of the survey. As expected, they are much less likely to be cohabiting at the time of the interview.

[19] Because the analysis is restricted to women who cohabited before conception and who report only one cohabiting union, the partner from whom IPV is reported is likely to be the father of the woman's first child.

[20] Appendix Table A.2 shows that matched and unmatched mothers are broadly similar along many observed dimensions. The main differences are that unmatched mothers have more-educated partners and are more likely to report that IPV began during the first year of cohabitation and that they had ever experienced IPV by the time of the interview.

4 Results

We present our main results in Figure 1. Panel (a) plots the evolution of the share of women who have ever experienced physical or sexual IPV around the time of first conception for treated women and their matched controls. Panel (b) presents the corresponding stacked difference-in-differences estimates.

In the four years prior to conception, the two groups follow nearly identical trajectories, with no visible divergence in IPV onset, providing support for the parallel-trends assumption underlying our identification strategy. In both groups, the likelihood of ever having experienced IPV increases gradually over time, reaching almost 5% in the year just before conception.

After treated women conceive their first child, the cumulative probability of ever experiencing sexual or physical IPV diverges. Starting in the year of conception and becoming more pronounced in the following years, treated women exhibit a steeper increase in the probability of ever experiencing IPV relative to their matched counterparts. We find an increase in the cumulative likelihood of IPV onset of 1.3 percentage points in the year of conception and 3.1 percentage points one year after conception. By two years after conception, the cumulative likelihood of ever experiencing IPV is 4.3 percentage points higher, a 29% increase relative to the control group mean of 14.9%. All of these estimates are statistically significant at the 1% level.

Among the different forms of IPV, moderate physical violence is by far the most prevalent form, affecting 19% of the treated women in our sample (Appendix Table A.3).[21] Severe physical violence (attempted strangling/burning, threatened or attacked with a deadly weapon) and sexual violence (forced sexual intercourse or unwanted sexual acts) are considerably less common (around 1% and 2%, respectively). To shed light on which forms of violence drive the main result, we replicate the stacked difference-in-differences estimation separately for each type of violence. The ENDES records a single onset date for physical or sexual violence, without distinguishing by type. For women who experienced multiple forms, we assign the same onset date to each type. Since violence most likely begins with moderate physical forms and escalates over time, this implies that we are likely assigning the onset of severe physical and sexual violence earlier than it actually occurred. However, this affects only a small share of cases, as 86% of treated women who report any violence experienced only one type. We present the results in Figure 2.

We find that the increase in IPV onset is driven almost entirely by moderate physical

[21] 15.5% report having been pushed, shaken, or had an object thrown at them, 10.3% report being slapped or having their arm twisted, 7.5% were hit with a fist or a harmful object, and 4.4% were kicked or dragged.

violence, which displays a pattern nearly identical to the main result: a gradual divergence starting at conception that reaches approximately 4 percentage points by two years after conception, a 27% increase relative to the control group mean of 14.7%.[22] In contrast, the coefficients for severe physical violence are essentially zero throughout, and those for sexual violence are positive but small, reaching 1.3 percentage points by year two. However, as sexual violence is very uncommon (the control group mean after two years is 1.1%), this effect implies a 118.2% increase relative to the control group.

To better inform the design of preventive interventions during this period of heightened vulnerability, we examine heterogeneity in the effects of motherhood on IPV to identify the women at greatest risk. We first examine heterogeneity by age and education prior to conception. As shown in Panel (a) of Figure 3, we find that the effect is larger for women who conceive before age 20, who are likely to have fewer economic resources, lower educational attainment, and weaker social networks at the time of becoming mothers. Along similar lines, Panel (b) shows that the effect is larger for women with a high school education or less.

We then examine heterogeneity by the woman's family history of IPV, measured by whether the woman's mother experienced IPV. Figure 4 shows that the effect of motherhood on IPV onset is substantially larger—approximately three to four times—for women whose mothers experienced IPV, compared to women without a family history of violence, for whom the effect is much smaller. This pattern is consistent with intergenerational transmission of violence (Hernandez-Leal et al., 2025; Bellés-Obrero et al., 2025; Collins, 2025).

Taken together, these results show that the increase in IPV following motherhood is concentrated among women who are already in more vulnerable positions prior to conception. This highlights the importance of preventive policies that target younger and less-educated women, as well as those with a family history of violence, who appear to face particularly high risk during the transition to motherhood.

4.1 Robustness checks

In this section, we conduct several robustness checks. First, we repeat our estimation using as a comparison group women who conceived their first child two years later rather than three years later. This alternative comparison involves a trade-off, as these mothers could potentially provide a more realistic counterfactual, but the estimable post-

[22] Appendix Figure A.6 plots the evolution in the raw means by type of violence for the treatment group and their matched controls.

conception window is shorter. Using this comparison group, we obtain a somewhat higher match rate than in our original sample, matching 16,631 women (88% of treated women), compared with 14,561 women (77%) in the baseline specification.[23] We present the main estimates in Figure 5. The estimated effects are somewhat smaller than those obtained in our original sample, but display the same overall pattern. Motherhood increases the cumulative probability of IPV onset by 0.7 percentage points in the year of conception and by 2.4 percentage points one year later, compared with 1.3 and 3.1 percentage points, respectively, in the baseline specification.

A related concern is that our baseline matching procedure requires treated women and their controls to be exactly the same age. This is a demanding criterion that may limit overlap between the treated and comparison groups, thereby leaving a non-trivial share of treated women (23%) unmatched. We examine whether our estimates are sensitive to relaxing this requirement. Specifically, we re-estimate the model allowing inexact matches through a priority rule: for each treated woman we first search for an exact match and, when none is available, we accept the closest available control whose age differs by at most one year.[24] As expected, loosening this criterion enlarges the pool of eligible controls and raises the share of treated women we are able to match, from 77% in the baseline to 90% when we relax exact age matching.[25] Reassuringly, the estimates are very similar to our baseline. Appendix Figure A.7 shows that two years after conception, the cumulative probability of ever experiencing IPV increases by 4 percentage points, compared with 4.3 percentage points in the main specification. The fact that the magnitude barely changes across these specifications mitigates concerns regarding the external validity of our main estimates.

We also examine the robustness of our estimates to variations in the set of matching covariates. In our baseline specification, we match on age and civil status in each of the four pre-conception years, and an indicator for whether IPV began in the first year of cohabitation. This last variable captures match quality. As an alternative, we replace the "IPV in the first year of cohabitation" variable with indicator variables for whether the woman and her partner had more than a high school education in the year prior to conception. Because these characteristics are predetermined and measured independently of the woman's cohabitation history, they provide a distinct way of capturing underlying

[23] Appendix Table A.4 compares the characteristics of the treated women who matched with women that had a child three years later (our original sample) and treated women who matched with women that had a child two years later.

[24] When several controls lie at the same distance from a treated woman, we average their outcomes and covariates.

[25] Appendix Table A.5 shows that treated and matched control women remain well balanced on observable characteristics under the relaxed specification.

IPV risk. Under this alternative matching strategy, we are able to match 63% of treated women. The estimated effects, reported in Panel (a) of Appendix Figure A.8, are similar to our baseline results. Two years after conception, the cumulative probability of ever experiencing IPV increases by 4.9 percentage points, compared to 4.3 percentage points in the main specification. The similarity in magnitude and dynamics suggests that our findings are not driven by the particular way in which we capture pre-conception match quality, but rather reflect a robust increase in IPV onset following the transition to motherhood.

We also assess the sensitivity of our results to the timing rule used to assign IPV onset within the reported yearly intervals. As described in Section 3, IPV onset is reported in 12-month intervals since the beginning of cohabitation, and in our baseline specification we assign onset to the middle of the corresponding interval (e.g., six months after cohabitation began if violence is reported to have started during the first year). As a robustness check, we instead assign onset to the beginning of the reported interval (e.g., at the start of cohabitation if violence is reported to have started during the first year). The results under this alternative timing assumption, presented in Panel (b) of Appendix Figure A.8, remain qualitatively unchanged. Two years after conception, the estimated increase in the cumulative probability of IPV is 3.5 percentage points, compared to 4.3 percentage points in the baseline specification. Although slightly smaller in magnitude, the effect remains economically meaningful and statistically significant. This confirms that our conclusions are not driven by a particular assumption about how IPV onset is located within the reported yearly interval.

An additional concern is that the coefficients for the last two periods are estimated based on a progressively smaller sample, since some women are interviewed before the end of the two-year period following conception. To assess whether changes in sample composition drive our results, we re-estimate the baseline specification restricting the sample to the 76% of treated women for whom outcomes are observed throughout the full two-year post-conception period, and report our results in Appendix Figure A.9. Reassuringly, the estimated coefficients are almost identical to the baseline estimates.

Taken together, these exercises show that our main findings are robust to using an alternative comparison group, relaxing the matching criteria, varying the set of matching covariates, changing the timing rule used to construct IPV onset, and restricting the sample to women observed throughout the full two-year post-conception period.

5 The importance of accounting for pre-conception relationship dynamics

As discussed in Section 3, estimating the causal effect of motherhood on IPV requires observing the counterfactual for each woman—namely, what her experience of violence would have been had she not had a child. To construct such a counterfactual, we propose matching on pre-conception characteristics that are more directly related to future IPV risk, such as the couple's relationship history and the quality of the match, thereby restricting the sample to women for whom these pre-conception characteristics can be observed.

In this section, we examine the importance of using our approach by conducting two exercises. First, we perform the stacked difference-in-differences estimation without any sample restrictions and matching only on age, as in previous studies. We then continue to use the pooled sample but exploit the additional information on pre-conception relationship dynamics available in survey data to improve the construction of the comparison group. Finally, we discuss why this improved pooled specification differs from our preferred baseline estimates.

5.1 An estimation without pre-conception relationship information

In this first exercise, we use the approach adopted in studies based on administrative data, where detailed pre-conception information on relationship status and quality is generally unavailable. In this approach, we depart from our baseline specification in two ways. First, rather than restricting the sample to women who cohabited before conceiving, we include all women regardless of their cohabitation status at conception. Second, rather than matching on age, civil status, and relationship quality, we match treated women to women who conceived their first child three years later using only age.

We are able to match 36,227 (99%) of the women in this pooled sample. The orange series in Figure 6 presents the results for this specification. There are two main takeaways. First, the estimates are much larger than in our baseline specification (in blue). Two years after conception, treated women are 12.2 percentage points more likely than their matched controls to have ever experienced IPV, compared with a 4.3-percentage-point difference in our baseline specification. Relative to the matched-control mean of 4.6%, this estimate corresponds to a 265% increase in the likelihood of experiencing IPV (vs. 29% in our preferred estimates).

Second, there is a slight divergence in outcomes between the treated and matched controls prior to motherhood. This is not surprising, given that prior to conception, treated

women and their matches have a sizable difference in civil status (Appendix Table A.6). In particular, while 52% of treated women are in a cohabiting relationship (married or in an informal union) the year before conception, only 12% of their matches share the same civil status in that period. Although difference-in-differences designs do not require treated and control units to be identical along every dimension, this imbalance is particularly concerning because civil status is intrinsically related to the outcome of interest. Women who are not yet cohabiting face a fundamentally different potential risk of IPV than women who already live with a partner. Consequently, the matched controls do not provide a credible counterfactual for treated women.

5.2 Using survey information to improve the comparison group

In the second exercise, we improve on the previous specification by exploiting an important advantage of the survey data: the ability to observe women's pre-conception civil status and, for those who were already cohabiting, relationship characteristics. We continue to use the pooled sample, but match treated women to controls with the same pre-conception civil status and, whenever possible, similar relationship quality. For women who cohabited before conceiving, this implies using the same matching strategy as in our baseline specification, namely matching on age, civil status, and relationship quality (proxied by whether IPV began during the first year of cohabitation). For women who had not yet begun cohabiting, constructing their counterfactual is difficult, because even in rich surveys such as ENDES, there is no information on whether they were in a stable non-cohabiting relationship at the time of conception, or how long that relationship had been in place, both of which are key correlates of future IPV. For these women, the best available approach is to match them to women of the same age and age at first sexual intercourse who conceived three years later and were also not cohabiting during the four years before the treated women conceived. Age at first sexual intercourse, which is available in some detailed surveys such as ENDES but not in administrative data, is included to capture relationship status, albeit only imperfectly.[26]

Pooling the two groups of treated women, we estimate the same stacked difference-in-differences equation. The green series in Figure 6 presents the results for this specification. There are two main takeaways. First, the estimated effect is substantially smaller than in the previous specification, but remains considerably larger than in our baseline estimation. By two years after conception, treated women are 8.9 percentage points more

[26]We are able to match 16,031 (91%) of the treated women in the subsample that did not cohabit before conception. Appendix Table A.7 compares the pre-conception characteristics of these women and their matches.

likely than their matched controls to have ever experienced IPV under this specification, compared with 12.2 percentage points when matching only on age and 4.3 percentage points in our baseline estimation. Relative to the matched-control mean of 9.2%, the 8.9-percentage-point estimate corresponds to a 97% increase in the likelihood of experiencing IPV, compared with a 265% increase in the previous specification and a 29% increase in our baseline sample. Second, unlike the previous specification, we find no evidence of differential pre-trends. Overall, this exercise shows that accounting for pre-conception civil status and relationship characteristics substantially improves the comparability of treated women and their matches, thereby reducing the upward bias found in the previous specification.

5.3 The challenge of constructing a credible counterfactual for women who conceive before cohabitation

Although matching on pre-conception relationship characteristics in the pooled sample estimation substantially reduces the estimated effects, they remain considerably larger than in our baseline specification, which is restricted to women who cohabited before conception. There are two possible explanations for this difference. First, the effects of motherhood on IPV may genuinely be larger among women who had not yet begun cohabiting at conception, in which case the larger estimates reflect treatment effect heterogeneity rather than bias. Second, despite the improved matching procedure, the estimates for women who conceived before cohabiting may still be upward biased, causing the pooled estimates to remain larger than those in our baseline specification. To better understand these two possibilities, we take a closer look at the sample of women who did not cohabit prior to conception.

Panel (a) of Figure 7 compares the evolution of IPV for treated women in this sample and their matched controls. Since the outcome is, by definition, equal to 0 prior to cohabitation, neither treated women nor their matched controls experience IPV in the pre-conception period. After conception, however, IPV trends diverge substantially: by two years after conception, 17% of treated women had experienced IPV, compared with only 4% of their matched controls. To examine whether this divergence coincides with differences in partnership formation, Panel (b) compares the evolution of cohabitation for the two groups. As with IPV, the share of women who are cohabiting prior to conception is 0 in both groups by construction. At the time of conception, the two groups diverge sharply: treated women experience a large and immediate increase in the likelihood of cohabiting, while their matched controls—who conceive three years later—enter cohabitation more

gradually. After two years, 70% of treated women are married or in an informal union, compared with 54% of their matched controls.

These patterns are consistent with the two possible explanations for the larger pooled estimates. Consistent with the treatment effect heterogeneity explanation, motherhood may induce cohabitation, increasing exposure to a partner and thereby raising the risk of IPV. In this sense, cohabitation may be a *mechanism* through which motherhood increases IPV for this group, resulting in larger effects than for the group of women who were already cohabiting prior to conception. Additionally, as shown in Appendix Table A.1, women who conceive before cohabiting are younger than those who cohabit before conceiving, which may further contribute to the larger estimated effects for this subgroup.

Alternatively, the estimates for women who conceived before cohabiting may still be upward biased. In particular, some treated women may already have been in a stable relationship at conception and would have begun cohabiting soon afterward even in the absence of childbirth, whereas their matched controls may have been single at the comparable point in time, given that they only conceive three years later. This is also consistent with the cohabitation patterns observed in Panel (b) of Figure 7. In that case, the matched controls would not represent the partnership, and therefore IPV, trajectory that treated women would have followed in the absence of conception. Unfortunately, we cannot rule out this possibility using the usual pre-trends test, because for both treated women and their matched controls IPV is mechanically equal to zero prior to conception, when they were not yet cohabiting.[27] Any post-conception divergence in IPV may therefore reflect not only the transition to motherhood, but also differences in co-residence and thus exposure to a partner that would have occurred even without a child.

Even with the richness of information available in a survey such as ENDES, we cannot distinguish between these two explanations. The larger estimates in the pooled sample may reflect genuine treatment effect heterogeneity, or they may arise because constructing a credible counterfactual for women who conceive before cohabiting remains particularly difficult. For this reason, our preferred specification restricts the analysis to women who were already cohabiting prior to conception. Although this restriction comes at the cost of external validity, it substantially strengthens internal validity, as only for these women can we observe the pre-conception relationship characteristics needed to construct a credible counterfactual. We therefore view this trade-off as necessary to credibly identify the causal effect of motherhood on IPV.

[27] There are only 186 women in this group who reported that IPV started before cohabiting with their partner. As explained in Section 3.2, we exclude these women from our analysis because we do not know exactly when they began experiencing IPV.

6 Conclusions

This paper estimates the causal effect of first-time motherhood on the onset of intimate partner violence using self-reported survey data from Peru. We implement a stacked difference-in-differences design that compares women who conceive their first child with women who conceive three years later and are similar in age, pre-conception civil status, and relationship quality. Accounting for these relationship characteristics is important for constructing a comparison group that provides a credible counterfactual for the IPV trajectories treated women would have followed in the absence of motherhood. Because the information needed for this matching strategy is observed only for women who were already cohabiting before conception, our estimation focuses on this group. We find that motherhood increases the cumulative probability of experiencing physical or sexual IPV by 4.3 percentage points, or 29%, within two years of conception. This increase is driven primarily by moderate physical violence and is concentrated among younger and less-educated women and those with a family history of violence.

We then examine how the estimated effect changes under alternative approaches to constructing the comparison group. These exercises yield two main conclusions. First, accounting for pre-conception relationship characteristics materially improves the comparability of treated women and their matches and reduces the risk of overstating the effect of motherhood on IPV. Second, even detailed survey data do not necessarily provide a credible counterfactual for women who conceive before cohabiting, because their pre-conception relationship status remains only imperfectly observed. The larger estimates obtained for this group may therefore reflect genuine treatment-effect heterogeneity, but they may also capture residual upward bias. Because we cannot distinguish between these explanations, we view the specification restricted to women who were already cohabiting before conception as providing the more credible estimate, while recognizing that this choice limits external validity.

Overall, our findings identify the transition to first motherhood as a critical window for IPV prevention. Designing effective preventive policies requires a deeper understanding of the underlying mechanisms behind the effect of motherhood on IPV. Several mechanisms identified in prior work are consistent with our results. One potential explanation for why the arrival of a child heightens financial pressure, stress, sleep deprivation, and other sources of strain, all of which may increase the likelihood that anger or frustration escalates into violence (Card and Dahl, 2011; Bhalotra et al., 2025). An alternative explanation is that motherhood reduces women's outside options due to restrictions for fully engaging in the labor market, allowing men to perpetrate violence with fewer conse-

quences (Aizer, 2010; Anderberg et al., 2016). The arrival of a child could also increase IPV by resulting in more disagreements over the allocation of household resources, including women's time. A more mechanical explanation is that motherhood increases women's exposure to their partners (Bhalotra et al., 2025). Distinguishing among these mechanisms is beyond the scope of our empirical design and remains an important direction for future research.

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Tables and Figures

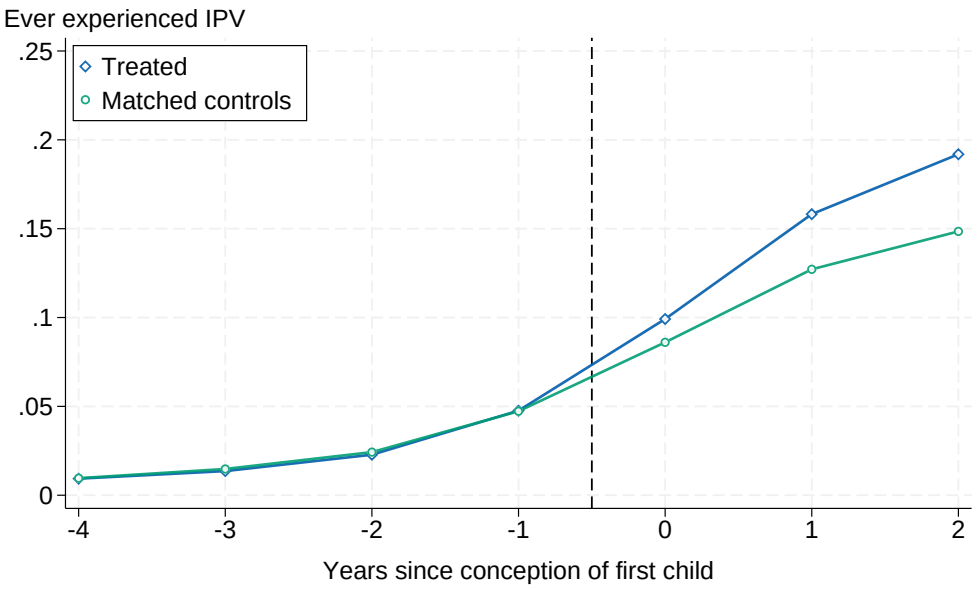
Table 1: Comparison of pre-conception characteristics of treated women and their matched controls

Variable	Matches	Treated
Region: Metropolitan Lima	0.149	0.100
Region: Pacific coast	0.273	0.282
Region: Andean highlands	0.317	0.356
Region: Amazon	0.262	0.261
Lived in small town or countryside as a child	0.522	0.561
Indigenous woman	0.056	0.094
More than a high school education (before conception)	0.427	0.345
Woman's father hit her mother	0.478	0.478
Age when first had sex	18.235	18.467
Age difference with first sexual partner (if interview age<25)	4.951	4.576
Used a condom with first sexual partner (if interview age<25)	0.283	0.242
Current/former partner had more than a high school education (before conception)	0.358	0.233
Age difference with partner (only for currently married)	4.326	4.201
Observations	14,561	14,561

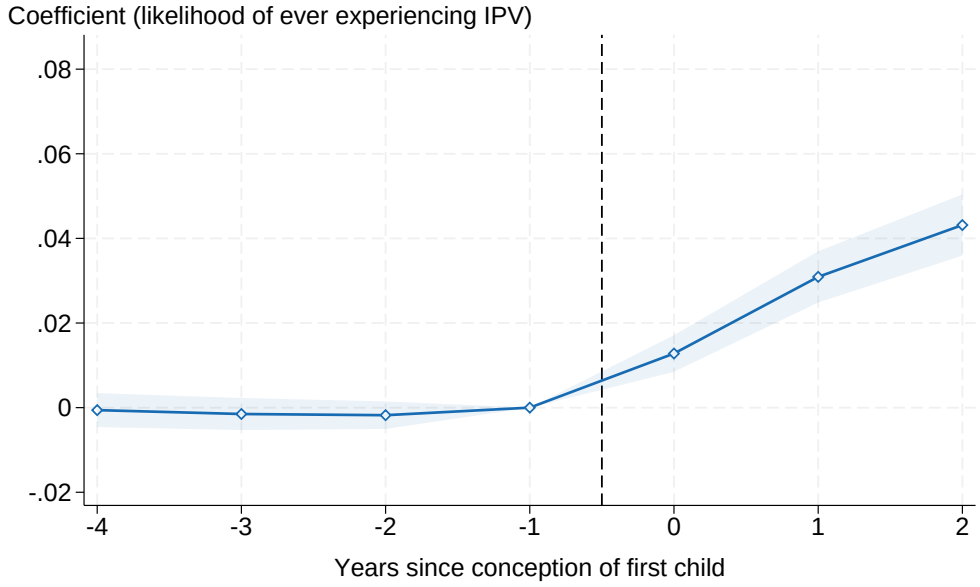
Notes: This table reports mean characteristics of the matched control women ("Matches") and the treated women ("Treated") in the year before conception (t = −1) for the sample of women who cohabited before conception. When a treated woman has more than one eligible match, the characteristics of those matches are averaged and enter the table as a single matched-control observation.

Figure 1: Effect of motherhood on IPV onset

(a) Raw means

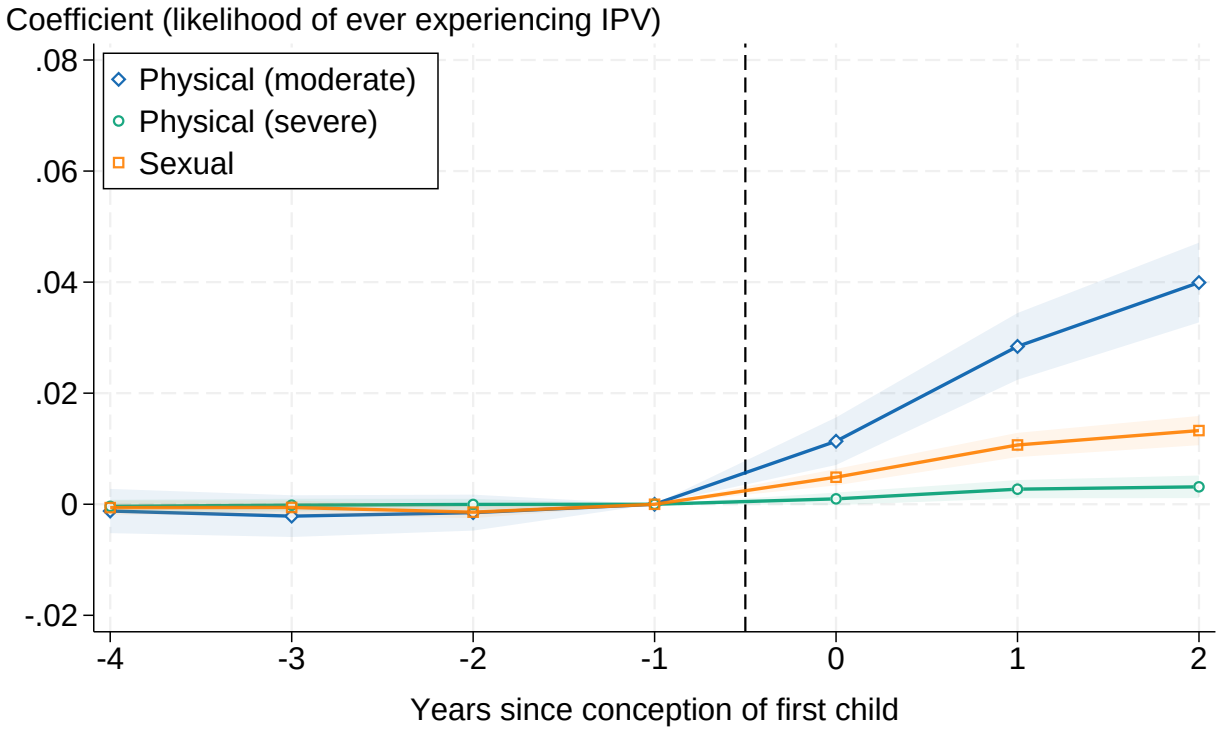


(b) Stacked difference-in-differences estimates



Notes: Panel (a) plots raw means of the cumulative probability of ever experiencing physical or sexual IPV relative to the time of conception of the first child for treated women (who conceive in year 0) and their matched comparison group (who conceive three years later). Panel (b) reports the corresponding stacked difference-in-differences estimates, with coefficients normalized to zero in the year before conception ($t = -1$). Shaded areas denote 95% confidence intervals. The vertical line indicates the year of conception of the first child. Standard errors are clustered at the individual level.

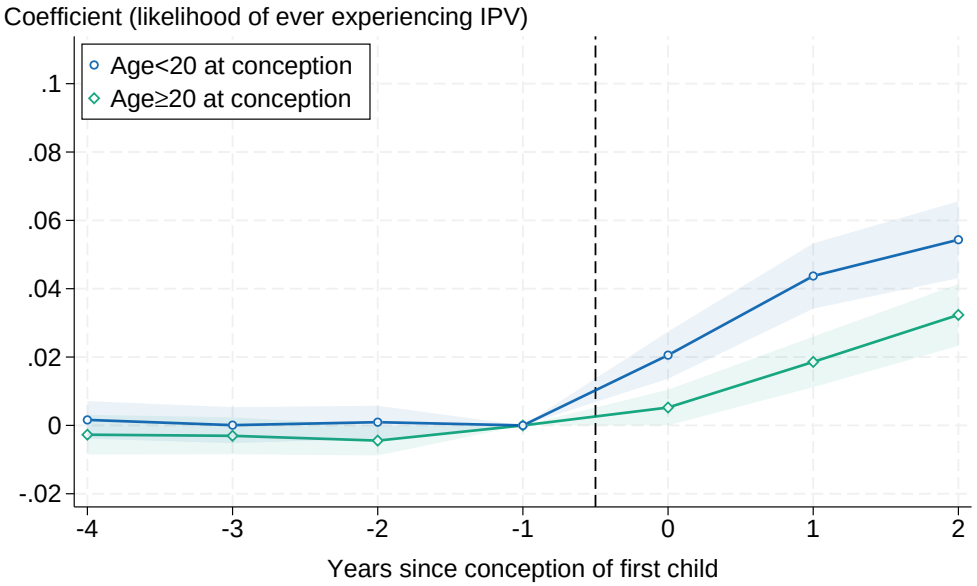
Figure 2: Effect of motherhood on IPV onset by type of violence



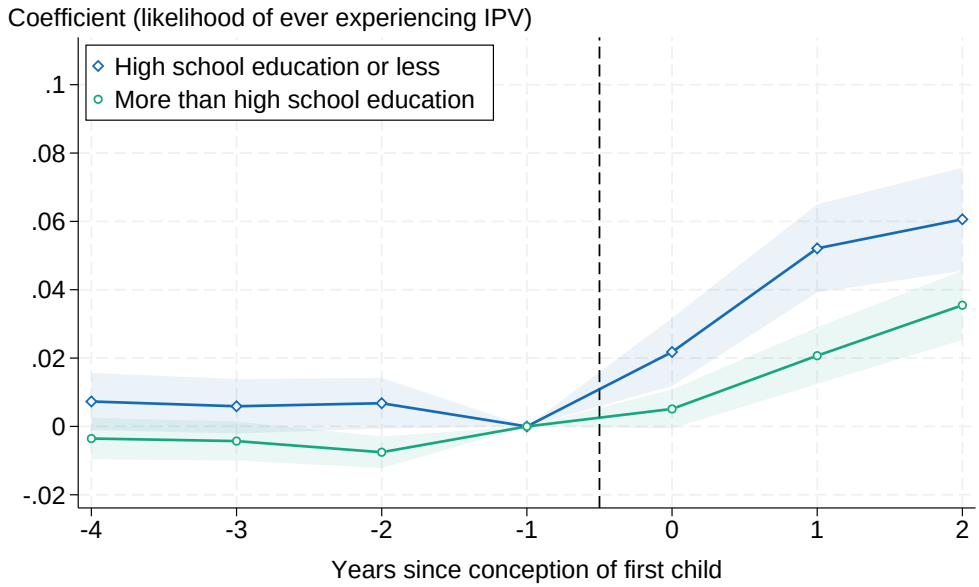
Notes: This figure reports stacked difference-in-differences estimates for the effect of motherhood on the cumulative probability of ever experiencing each type of IPV, with coefficients normalized to zero in the year before conception ($t = -1$). Shaded areas denote 95% confidence intervals. The vertical line indicates the year of conception of the first child. Standard errors are clustered at the individual level.

Figure 3: Effect of motherhood on IPV onset — Heterogeneous effects by age and women's education

(a) Heterogeneous effects by woman's age

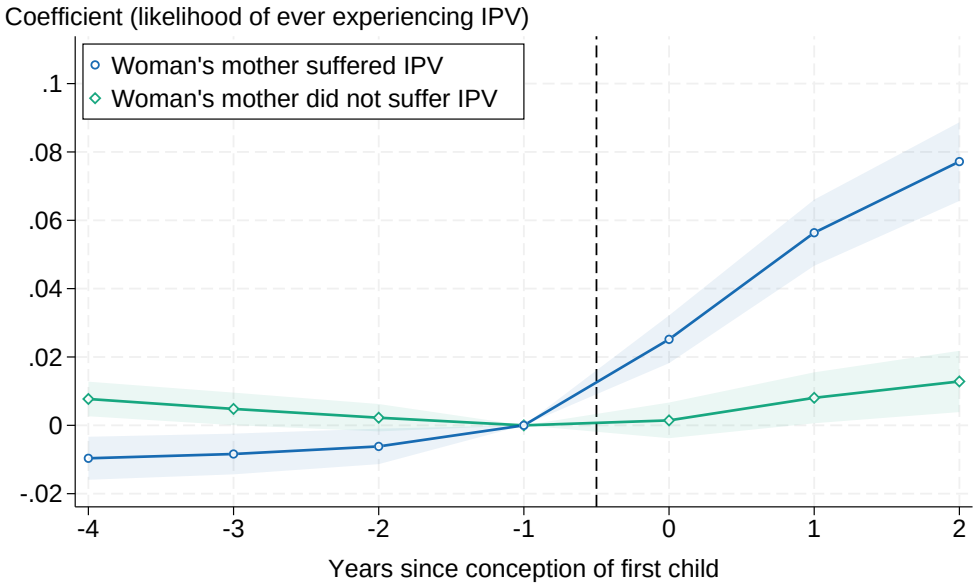


(b) Heterogeneous effects by women's education



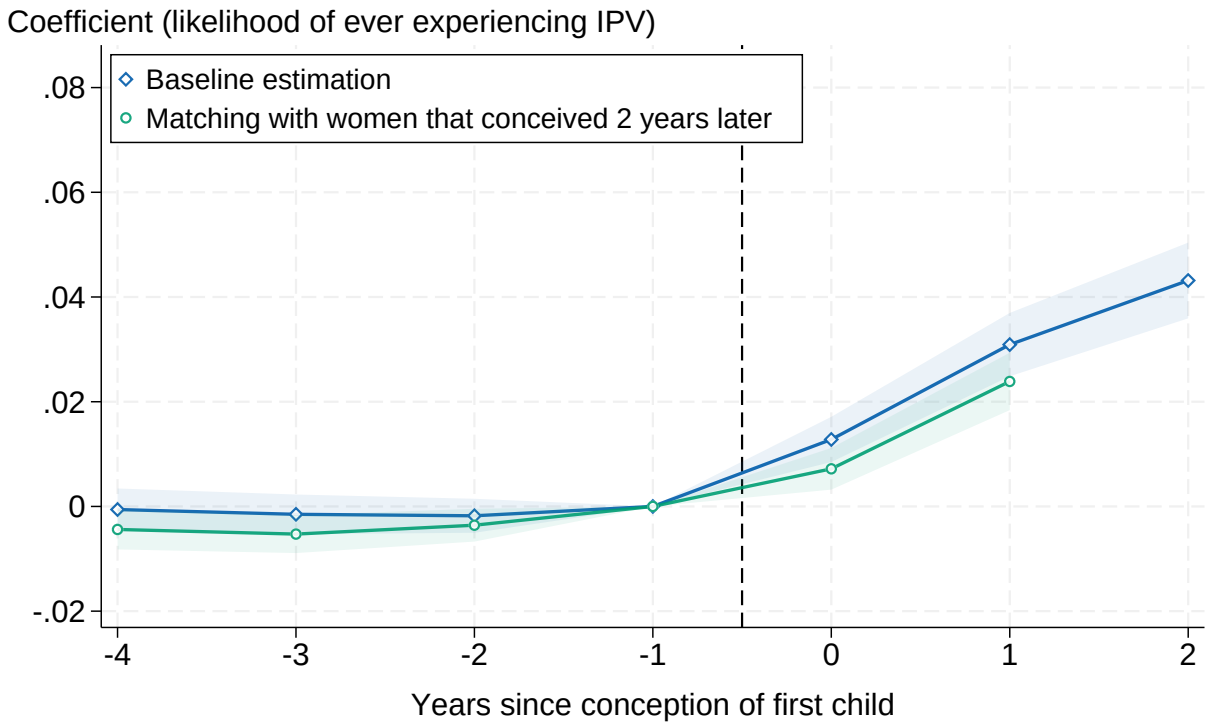
Notes: Panel (a) reports the stacked difference-in-differences estimates of the effect of motherhood on the cumulative probability of ever experiencing physical or sexual IPV, splitting the sample by whether the woman was younger than 20 at conception. Panel (b) reports the analogous estimates splitting the sample by whether the woman had more than a high school education at the time of conception. Coefficients are normalized to zero in the year before conception ($t = -1$). Shaded areas denote 95% confidence intervals. The vertical line indicates the year of conception of the first child. Standard errors are clustered at the individual level.

Figure 4: Effect of motherhood on IPV onset — Heterogeneous effects by intergenerational IPV



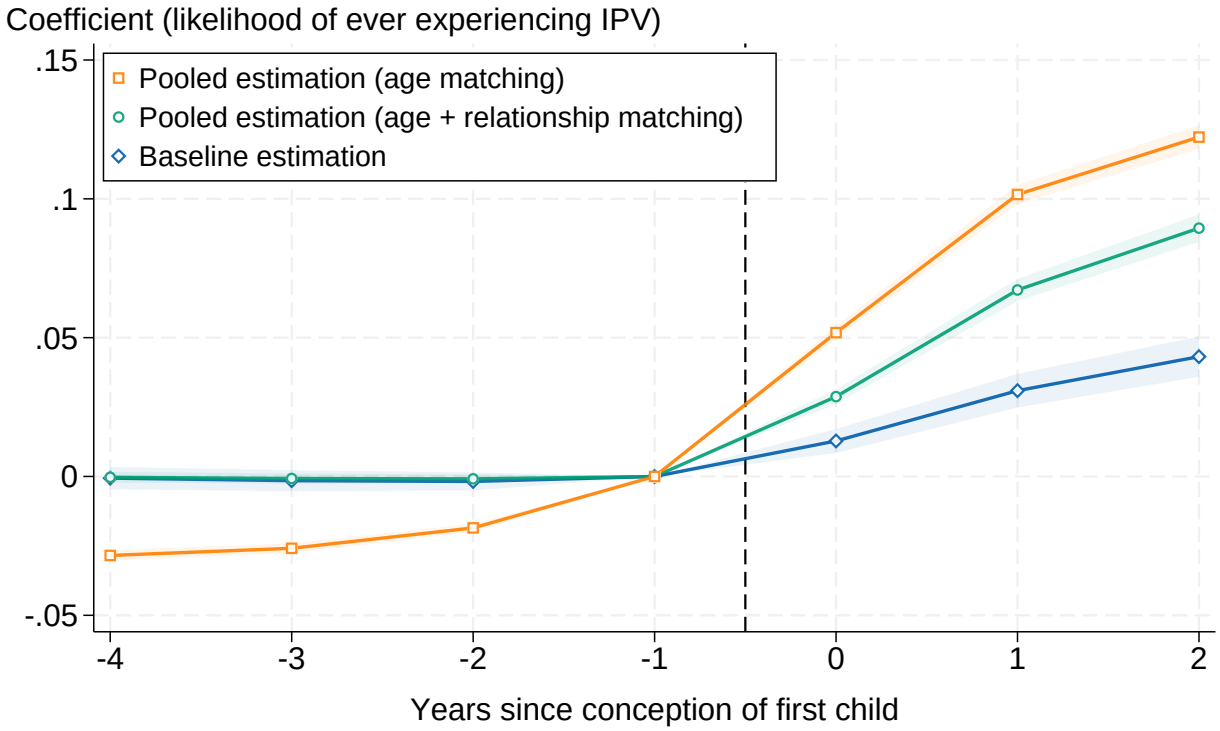
Notes: This figure reports the stacked difference-in-differences estimates of the effect of motherhood on the cumulative probability of ever experiencing physical or sexual IPV, splitting the sample by whether the woman's mother suffered IPV from the woman's father. Coefficients are normalized to zero in the year before conception ($t = -1$). Shaded areas denote 95% confidence intervals. The vertical line indicates the year of conception of the first child. Standard errors are clustered at the individual level.

Figure 5: Effect of motherhood on IPV onset — Robustness to using women who conceived two years later as controls



Notes: This figure reports stacked difference-in-differences estimates of the effect of motherhood on the cumulative probability of ever experiencing physical or sexual IPV, using matched controls who conceived two years later. Coefficients are normalized to zero in the year before conception ($t = -1$). Shaded areas denote 95% confidence intervals. The vertical line indicates the year of conception of the first child. Standard errors are clustered at the individual level.

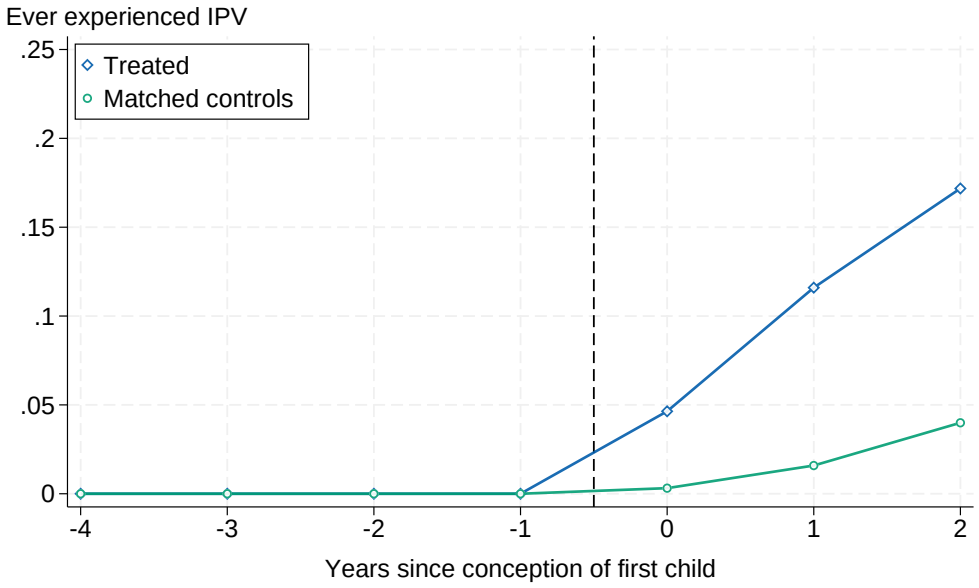
Figure 6: Effect of motherhood on IPV onset across alternative estimation strategies



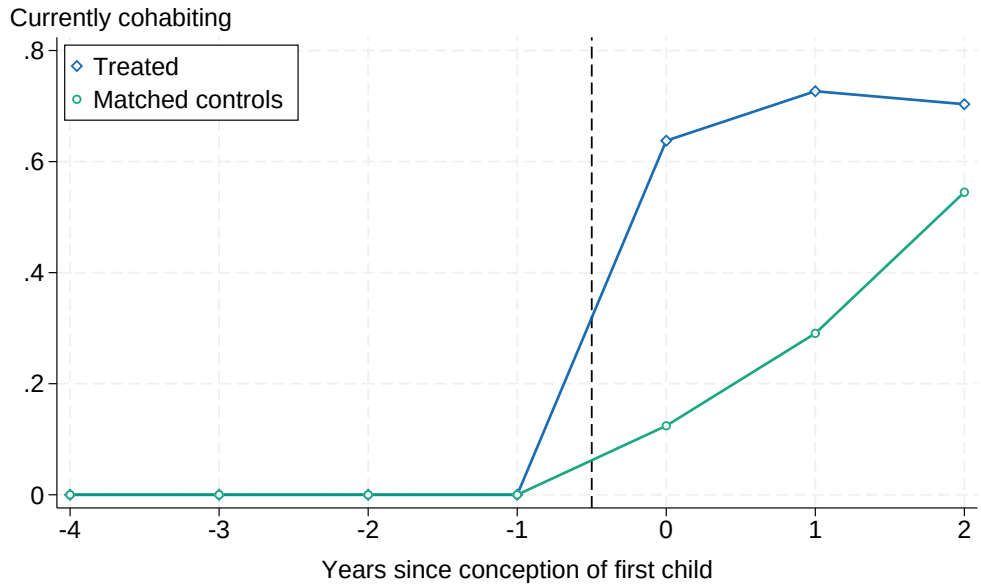
Notes: This figure compares the stacked difference-in-differences estimates of the effect of motherhood on the cumulative probability of ever experiencing physical or sexual IPV across the three estimation strategies presented in Section 5. The orange series corresponds to the pooled estimation in which treated women are matched to women who conceived three years later using only age. The green series also uses the pooled sample, but matches treated women to controls with the same pre-conception civil status and, whenever possible, similar relationship characteristics. The blue series reports our preferred baseline estimation, which restricts the sample to women who cohabited before conception and matches treated women to controls on age, pre-conception civil status, and relationship quality. The omitted category is the year prior to conception ($t = -1$). Shaded areas represent 95% confidence intervals based on standard errors clustered at the individual level.

Figure 7: Evolution of IPV onset and cohabitation status around the time of first conception – Sample that did not cohabit before conception

(a) Cumulative probability of ever experiencing IPV



(b) Likelihood of cohabiting



Notes: Panel (a) plots raw means of the cumulative probability of ever experiencing physical or sexual IPV relative to the time of conception of the first child for treated women (who conceive in year 0) and their matched comparison group (who conceive three years later), for the subsample of women who did not cohabit before conception. Panel (b) presents the same plot for the likelihood of currently cohabiting.

ONLINE APPENDIX

Appendix A Appendix Tables and Figures

Table A.1: Comparison of characteristics of treated women who cohabited and did not cohabit before conceiving

Variable	No cohabitation before conception	Cohabitation before conception
Baseline characteristics		
Region: Metropolitan Lima	0.117	0.103
Region: Pacific coast	0.298	0.281
Region: Andean highlands	0.358	0.353
Region: Amazon	0.227	0.262
Lived in small town or countryside as a child	0.479	0.551
Indigenous woman	0.084	0.091
Had more than high school education (before conception)	0.352	0.358
Woman's father hit her mother	0.462	0.480
Age when first had sex	18.205	18.582
Age difference with first sexual partner (if interview age<25)	4.006	4.664
Used a condom with first sexual partner (if interview age<25)	0.273	0.242
Age at conception	21.120	23.040
Outcomes at time of interview		
Ever suffered physical or sexual IPV (from current/former partner)	0.173	0.233
Ever suffered physical IPV (from current/former partner)	0.168	0.229
Ever suffered severe physical IPV (from current/former partner)	0.013	0.019
Ever suffered sexual IPV (from current/former partner)	0.028	0.033
Currently cohabiting	0.683	0.911
Currently separated	0.080	0.089
Observations	17,571	18,875

Notes: This table compares the characteristics of treated women who did not cohabit before conception with treated women who cohabited before conception.

Table A.2: Comparison of characteristics of matched and unmatched treated women

Variable	Not Matched	Matched
Baseline characteristics		
Region: Metropolitan Lima	0.114	0.100
Region: Pacific coast	0.278	0.282
Region: Andean highlands	0.342	0.356
Region: Amazon	0.266	0.261
Lived in small town or countryside as a child	0.519	0.561
Indigenous woman	0.081	0.094
More than a high school education (before conception)	0.399	0.345
Woman's father hit her mother	0.487	0.478
Age when first had sex	18.969	18.467
Age difference with first sexual partner (if interview age<25)	5.001	4.576
Used a condom with first sexual partner (if interview age<25)	0.241	0.242
Age at conception	24.419	22.631
Current/former partner had more than a high school education (before conception)	0.334	0.233
Age difference with spouse (only for currently married)	3.806	4.201
IPV starts first year of cohabitation	0.262	0.024
Outcomes at time of interview		
Ever suffered physical or sexual IPV (from current/former partner)	0.369	0.193
Ever suffered physical IPV (from current/former partner)	0.362	0.189
Ever suffered severe physical IPV (from current/former partner)	0.037	0.013
Ever suffered sexual IPV (from current/former partner)	0.061	0.025
Currently cohabiting	0.879	0.921
Currently separated	0.121	0.079
Observations	4,314	14,561

Notes: This table compares the characteristics of matched and unmatched treated women for the sample of treated women who cohabited before conception.

Table A.3: Share of treated women who ever experienced different forms of IPV

	Mean
Emotional violence	
Humiliated in front of others	0.102
Threatened harm to respondent or someone close	0.042
Threatened to leave home, take children, or withdraw financial support	0.112
Physical violence (moderate)	
Pushed, shaken, or threw object	0.155
Slapped or twisted arm	0.103
Hit with fist or harmful object	0.075
Kicked or dragged	0.044
Physical violence (severe)	
Attempted strangling or burning	0.010
Attacked with knife, gun, or other weapon	0.005
Threatened with knife, gun, or other weapon	0.002
Sexual violence	
Forced sexual intercourse	0.022
Forced unwanted sexual acts	0.012
Observations	14,561

Notes: This table reports, for the treated sample that cohabited before conception, the share of women who reported ever experiencing different types of IPV from their latest partner.

Table A.4: Comparison of characteristics of treated women matched with alternative comparison groups

Variable	Original	Matched with women that conceived 2 years later	Matched both ways
Baseline characteristics			
Region: Metropolitan Lima	0.100	0.102	0.083
Region: Pacific coast	0.282	0.284	0.259
Region: Andean highlands	0.356	0.354	0.377
Region: Amazon	0.261	0.260	0.280
Lived in small town or countryside as a child	0.561	0.555	0.582
Indigenous woman	0.094	0.092	0.106
More than a high school education (before conception)	0.345	0.358	0.297
Woman's father hit her mother	0.478	0.476	0.527
Age when first had sex	18.467	18.504	18.446
Age difference with first sexual partner (if interview age<25)	4.576	4.592	4.638
Used a condom with first sexual partner (if interview age<25)	0.242	0.246	0.206
Age at conception	22.631	22.828	22.250
Current/former partner had more than a high school education (before conception)	0.233	0.251	0.162
Age difference with partner (only for currently married)	4.201	4.177	4.256
IPV starts first year of cohabitation	0.024	0.033	0.029
Outcomes at time of interview			
Ever suffered physical or sexual IPV (from current/former partner)	0.193	0.195	0.203
Ever suffered physical IPV (from current/former partner)	0.189	0.192	0.200
Ever suffered severe physical IPV (from current/former partner)	0.013	0.006	0.015
Ever suffered sexual IPV (from current/former partner)	0.025	0.026	0.027
Currently cohabiting	0.921	0.920	0.938
Currently separated	0.079	0.080	0.062
Observations	14,561	16,631	7,123

Notes: This table compares treated women who cohabited before conception and were matched under the baseline specification with those matched under the alternative specification, which uses controls who conceived two years later. The final column reports women who match under both specifications.

Table A.5: Comparison of pre-conception characteristics of treated women and their matched controls — Matching where age is allowed to differ by up to one year

Variable	Matches	Treated
Region: Metropolitan Lima	0.141	0.101
Region: Pacific coast	0.269	0.283
Region: Andean highlands	0.326	0.354
Region: Amazon	0.264	0.263
Lived in small town or countryside as a child	0.522	0.556
Indigenous woman	0.059	0.092
More than a high school education (before conception)	0.426	0.351
Woman's father hit her mother	0.482	0.480
Age when first had sex	18.288	18.488
Age difference with first sexual partner (if interview age<25)	4.985	4.633
Used a condom with first sexual partner (if interview age<25)	0.269	0.243
Current/former partner had more than a high school education (before conception)	0.358	0.245
Age difference with partner (only for currently married)	4.297	4.182
Observations	17,052	17,052

Notes: This table reports mean characteristics of the matched control women ("Matches") and the treated women ("Treated") in the year before conception (t = −1), for the sample of women who cohabited before conception. When a treated woman has more than one eligible match, the characteristics of those matches are averaged and enter the table as a single matched-control observation. The matched sample corresponds to the specification that allows the woman's age to differ by at most one year.

Table A.6: Comparison of pre-conception characteristics of treated women and their matched controls – Pooled sample matching by age

Variable	Matches	Treated
Region: Metropolitan Lima	0.146	0.109
Region: Pacific coast	0.316	0.289
Region: Andean highlands	0.332	0.356
Region: Amazon	0.205	0.246
Lived in small town or countryside as a child	0.450	0.517
Indigenous woman	0.066	0.088
Married or in an informal union (before conception)	0.121	0.515
More than a high school education (before conception)	0.454	0.353
Woman's father hit her mother	0.431	0.473
Age when first had sex	18.360	18.360
Age difference with first sexual partner (if interview age<25)	3.417	4.304
Used a condom with first sexual partner (if interview age<25)	0.343	0.259
Current/former partner had more than a high school education (before conception)	0.394	0.260
Age difference with partner (only for currently married)	3.233	3.989
Observations	36,227	36,227

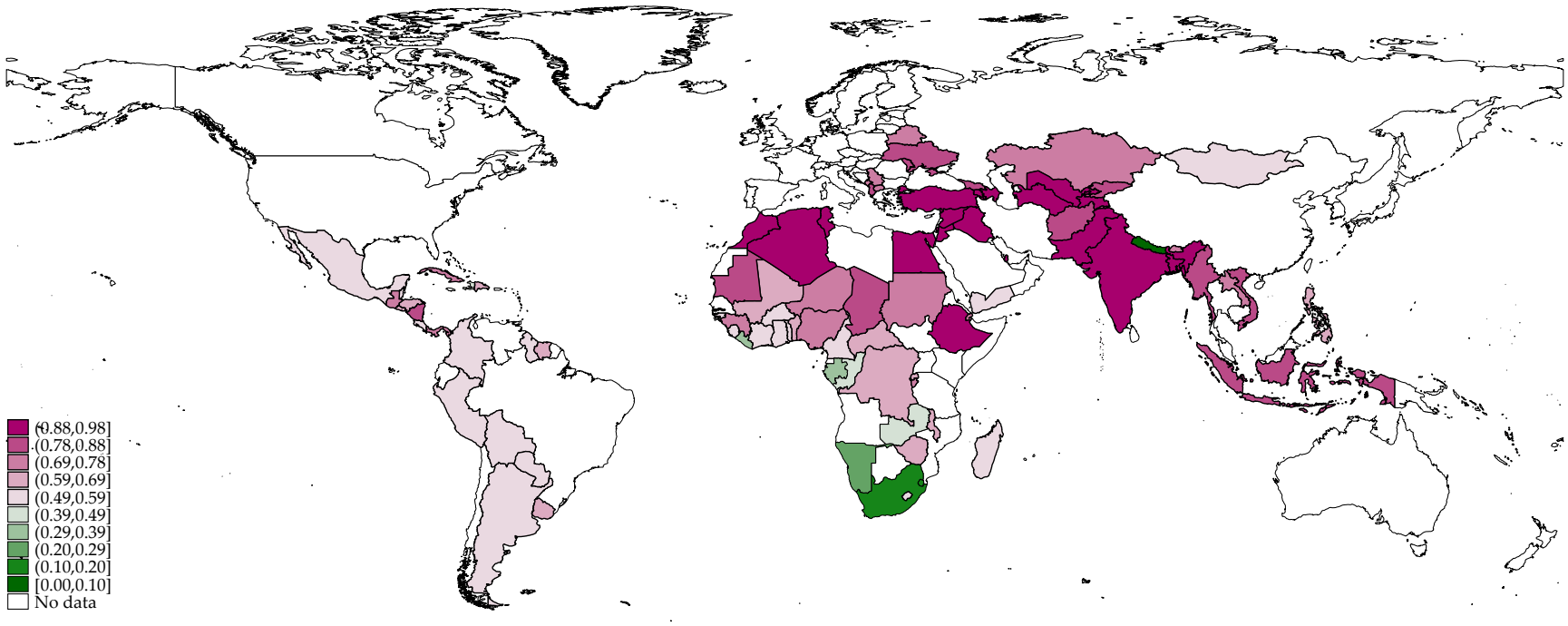
Notes: This table reports mean characteristics of the matched control women ("Matches") and the treated women ("Treated") in the year before conception (t = −1) for the pooled sample, matching women only on age. When a treated woman has more than one eligible match, the characteristics of those matches are averaged and enter the table as a single matched-control observation.

Table A.7: Comparison of pre-conception characteristics of treated women and their matched controls – Sample that did not cohabit before conception

Variable	Matches	Treated
Region: Metropolitan Lima	0.152	0.114
Region: Pacific coast	0.302	0.295
Region: Andean highlands	0.277	0.361
Region: Amazon	0.269	0.229
Lived in small town or countryside as a child	0.416	0.485
Indigenous woman	0.054	0.086
More than a high school education (before conception)	0.424	0.332
Woman's father hit her mother	0.449	0.466
Age difference with first sexual partner (if interview age<25)	3.427	3.988
Used a condom with first sexual partner (if interview age<25)	0.409	0.272
Observations	16,031	16,031

Notes: This table reports mean characteristics of the matched control women ("Matches") and the treated women ("Treated") in the year before conception (t = −1) for the sample of women who did not cohabit before conception. When a treated woman has more than one eligible match, the characteristics of those matches are averaged and enter the table as a single matched-control observation.

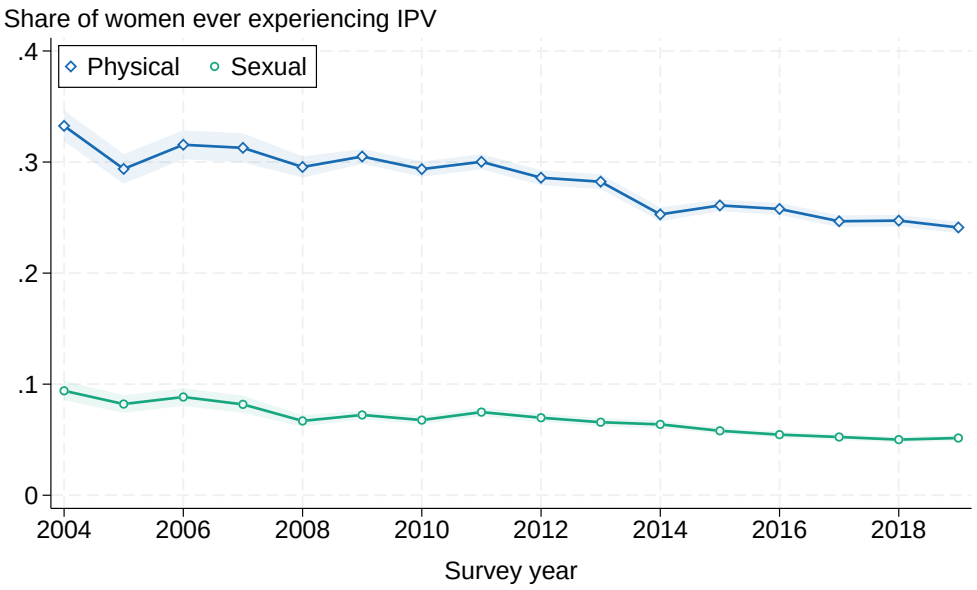
Figure A.1: Share of mothers who cohabited before conceiving their first child, by country



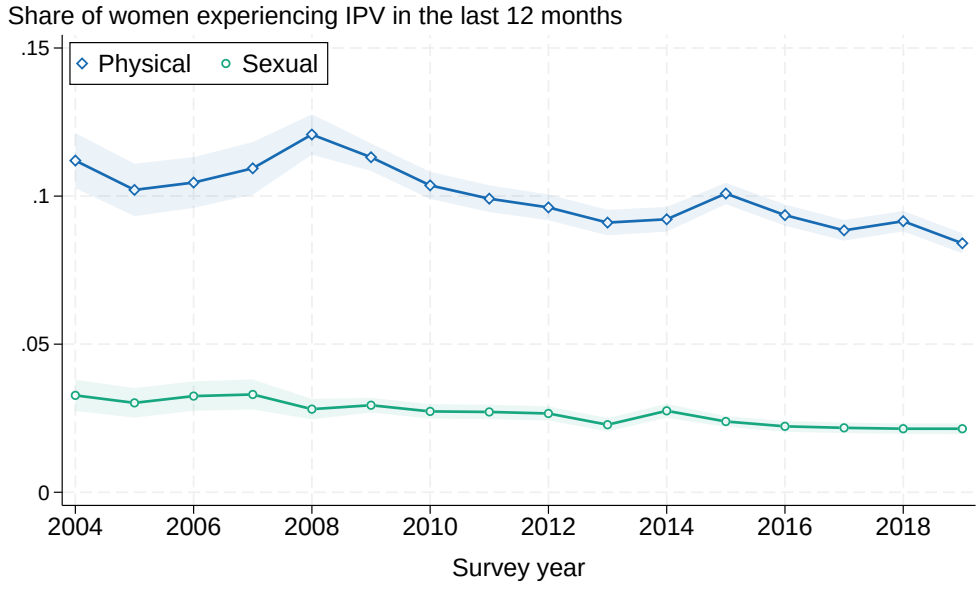
Notes: This figure shows the share of mothers ages 15-49 who cohabited before conceiving their first child. For each country, we use the latest available sample in the DHS or MICS surveys after 2000.

Figure A.2: Incidence of IPV in Peru, 2004–2019

(a) Share who ever experienced IPV

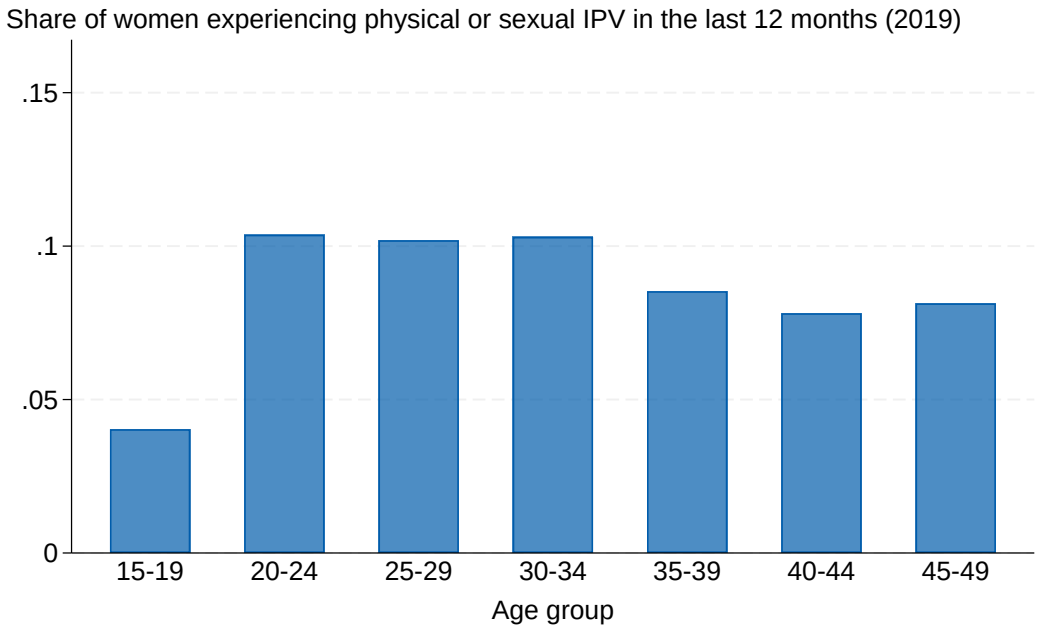


(b) Share who experienced IPV in the last 12 months



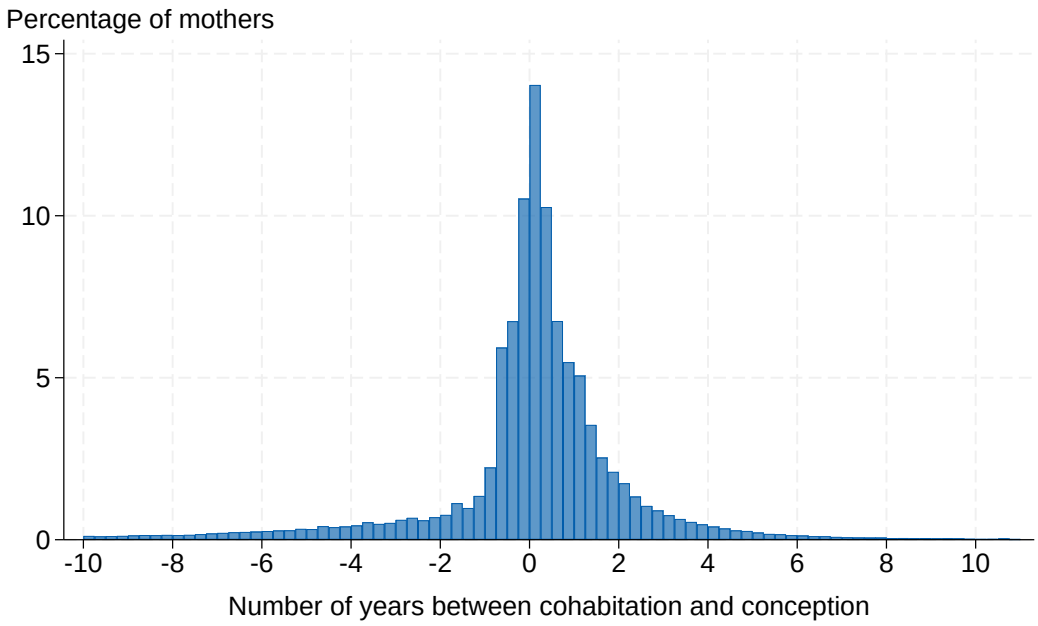
Notes: The top panel shows the share of women aged 15–49 who reported having ever experienced physical or sexual IPV from their current or most recent partner. The bottom panel shows the share reporting IPV in the twelve months preceding the interview. Data from ENDES 2004–2019. Shaded areas represent 95% confidence intervals.

Figure A.3: Physical or sexual IPV in the last 12 months by age group, 2019



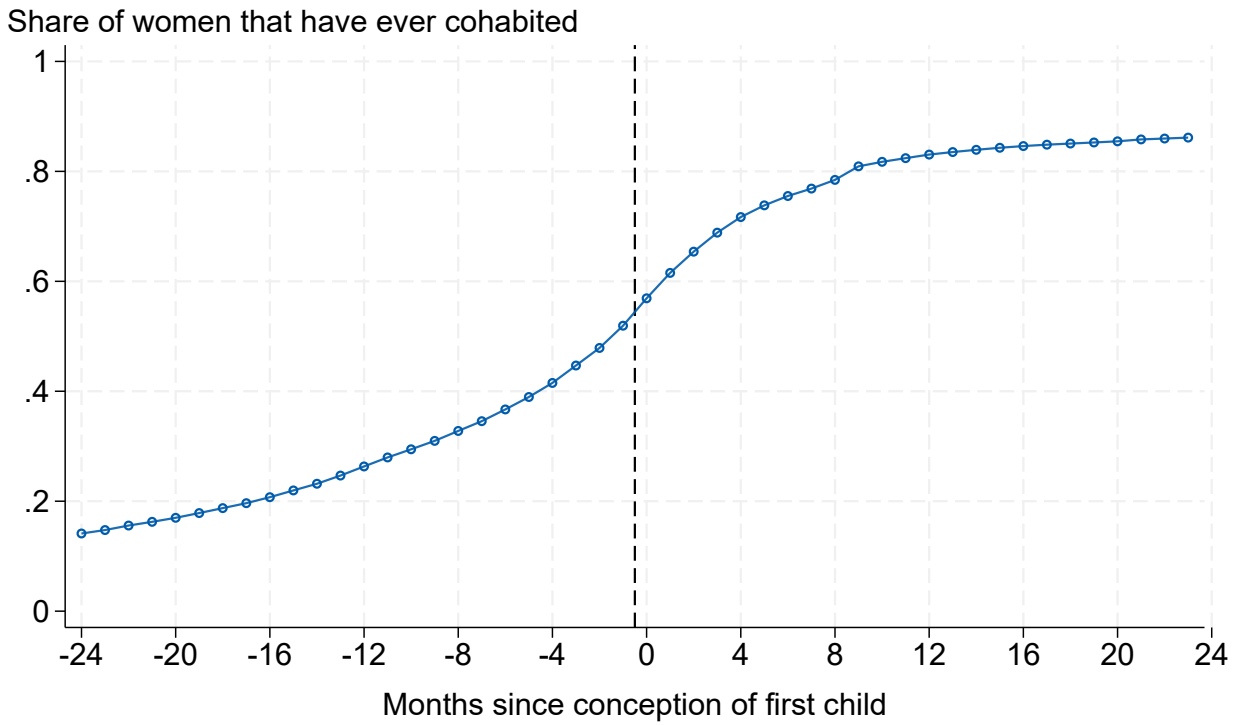
Notes: This figure shows the share of women reporting physical or sexual IPV from their current or most recent partner in the twelve months preceding the interview, by age group. Data from ENDES 2019.

Figure A.4: Distribution of years between cohabitation and first conception



Notes: This figure shows the distribution of the number of years between the start of cohabitation and the conception of the first child for women in the 2004–2019 ENDES. Negative values indicate that cohabitation began after conception.

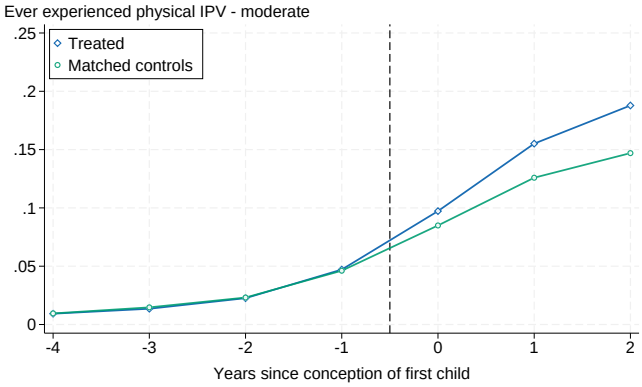
Figure A.5: Cohabitation around the time of first conception



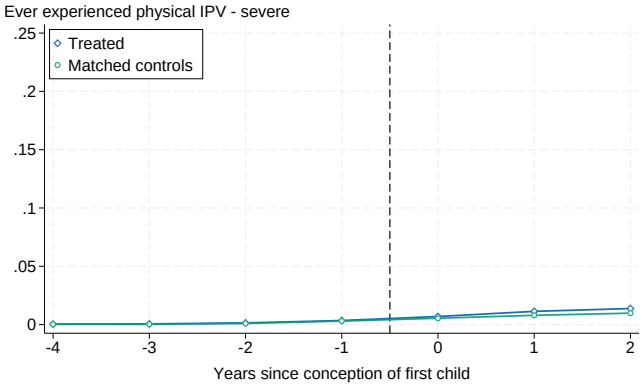
Notes: This figure plots the share of women from the 2004–2019 ENDES who had ever cohabited with a partner by each month relative to the conception of their first child. The vertical line indicates the month of conception.

Figure A.6: Effect of motherhood on IPV onset by type of violence

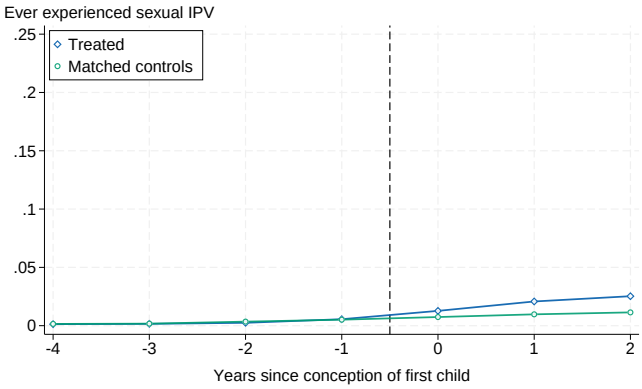
(a) Physical violence (moderate)



(b) Physical violence (severe)

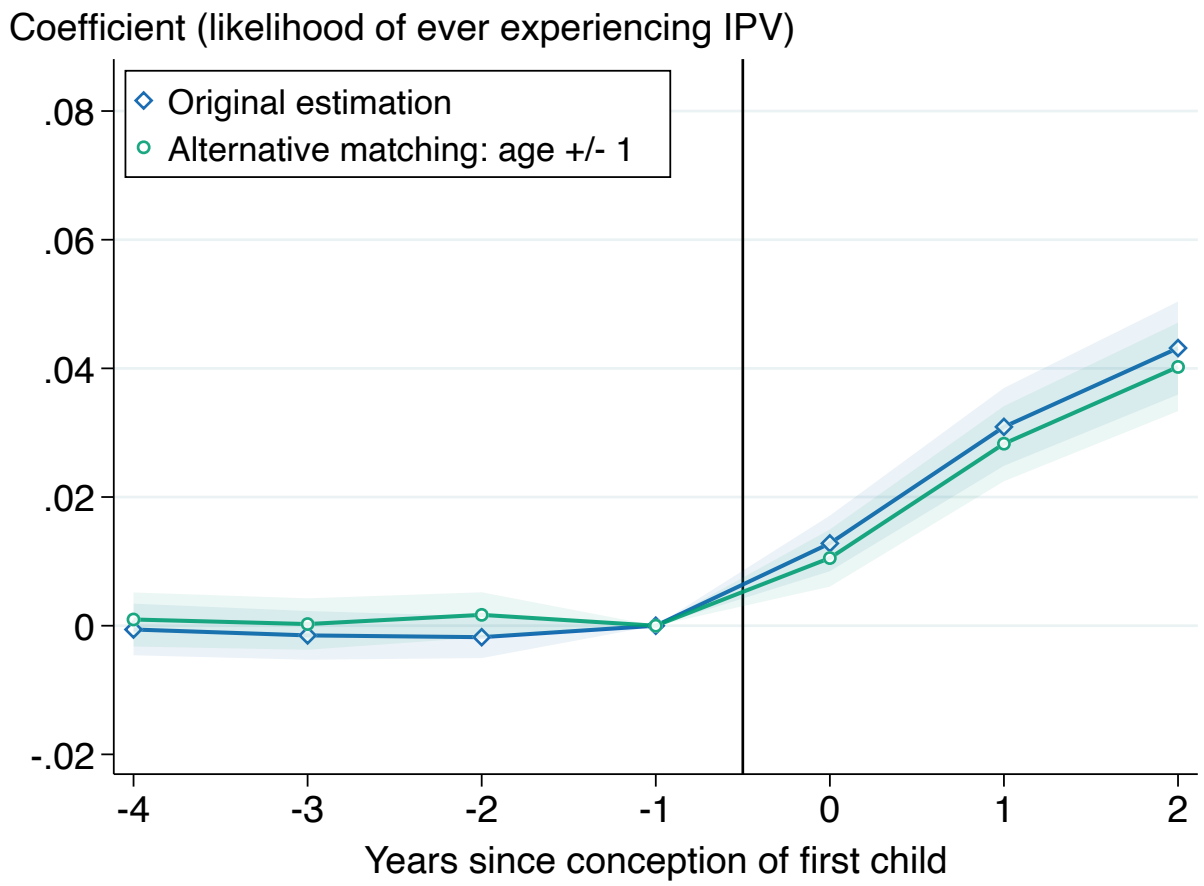


(c) Sexual violence



Notes: Each panel plots raw means of the cumulative probability of ever experiencing each type of IPV relative to the time of conception of the first child, for treated women (who conceive in year 0) and their matched comparison group (who conceive three years later), for the sample of women who cohabited before conception. The vertical line indicates the year of conception of the first child. Moderate physical violence includes pushing, slapping, hitting, kicking, dragging, and throwing objects. Severe physical violence includes attempted strangling/burning, threats or attacks with a deadly weapon. Sexual violence includes forced sexual intercourse or unwanted sexual acts.

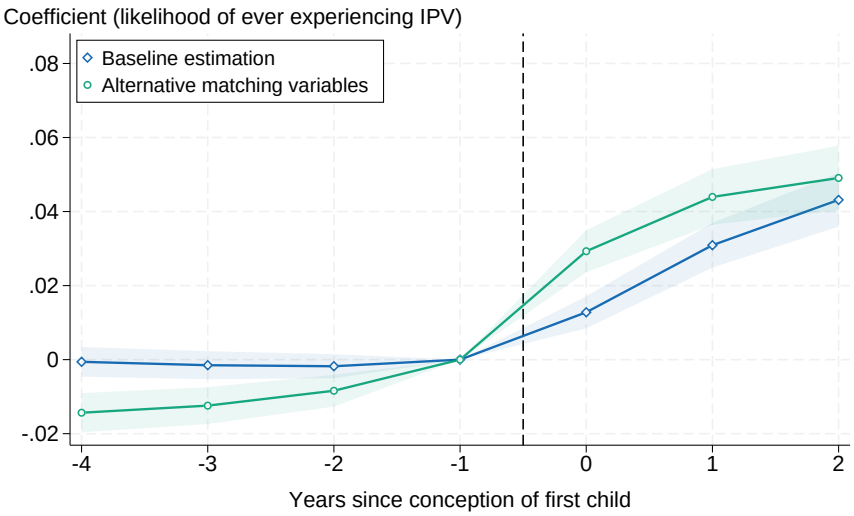
Figure A.7: Effect of motherhood on IPV onset – Allowing the woman's age to differ by up to one year



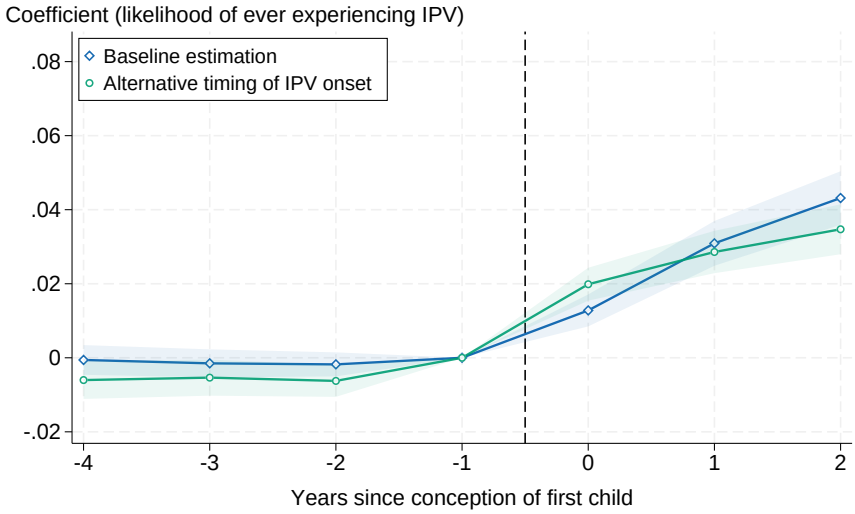
Notes: This figure plots the results of stacked difference-in-differences estimates of the effect of motherhood on the cumulative probability of ever experiencing physical or sexual IPV for the sample of women who cohabited before conception. It compares the baseline specification, which requires each control to coincide exactly with the treated woman on age (in addition to civil status, the calendar year of observation, and whether IPV began in the first year of cohabitation), to an alternative matching strategy that relaxes the exact-age requirement through a priority rule: for each treated woman we first seek an exact match and, when none is available, we accept the closest control whose age differs by at most one year. Coefficients are normalized to zero in the year before conception (t = −1). Shaded areas denote 95% confidence intervals. The vertical line indicates the year of conception of the first child. Standard errors are clustered at the individual level.

Figure A.8: Effect of motherhood on IPV onset – Robustness to alternative matching variables and an alternative construction of the outcome variable

(a) Robustness to alternative matching variables

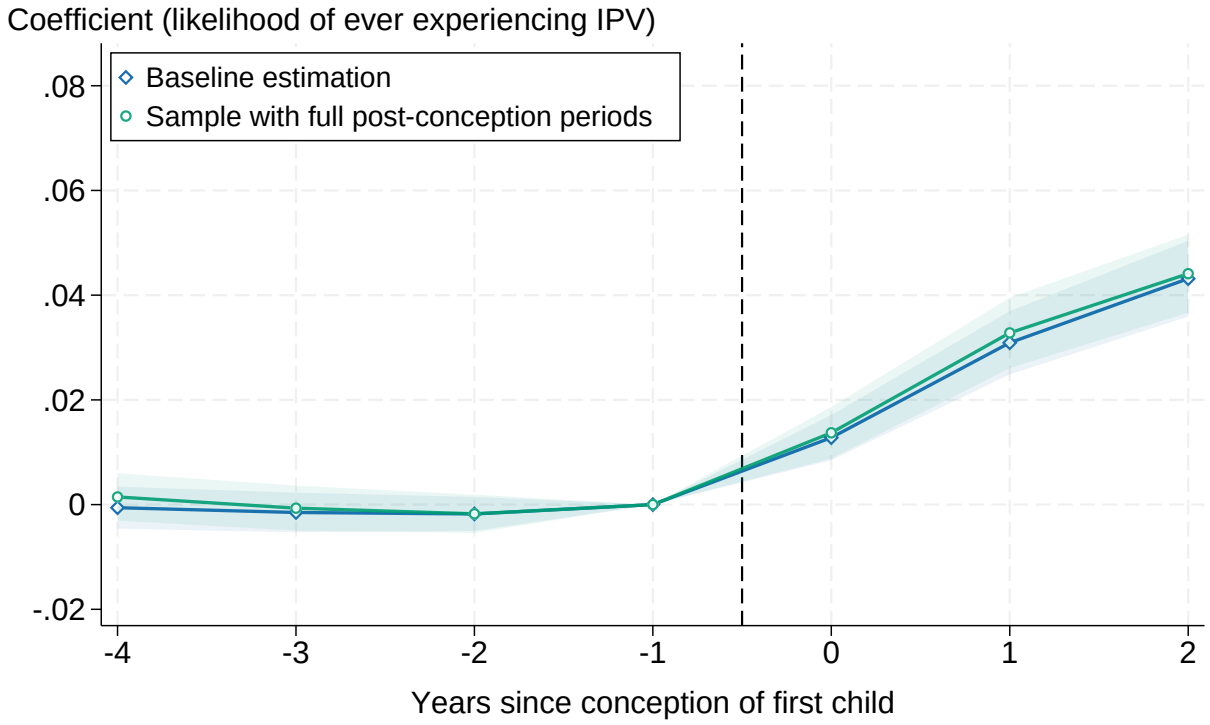


(b) Robustness to alternative construction of the outcome variable



Notes: This figure plots the results of stacked difference-in-differences estimates of the effect of motherhood on the cumulative probability of ever experiencing physical or sexual IPV for the sample of women who cohabited before conception. Panel (a) compares the baseline specification (matching on age, civil status, and whether IPV began in the first year of cohabitation) to an alternative matching strategy that replaces the early-cohabitation IPV variable with indicator variables for whether the woman and her partner had more than a high school education prior to conception. Panel (b) compares the baseline timing assumption (assigning IPV onset to the midpoint of the reported yearly interval since cohabitation began) to an alternative definition that assigns IPV onset to the start of the reported interval. Coefficients are normalized to zero in the year before conception (t = -1). Shaded areas denote 95% confidence intervals. The vertical line indicates the year of conception of the first child. Standard errors are clustered at the individual level.

Figure A.9: Effect of motherhood on IPV onset — Robustness to restricting the sample to women observed throughout the full two-year post-conception period



Notes: This figure reports stacked difference-in-differences estimates of the effect of motherhood on the cumulative probability of ever experiencing physical or sexual IPV for the sample of women who cohabited before conception, restricting the sample to treated women observed throughout the full two-year post-conception period. Coefficients are normalized to zero in the year before conception ($t = -1$). Shaded areas denote 95% confidence intervals. The vertical line indicates the year of conception of the first child. Standard errors are clustered at the individual level.

Appendix B Questions on IPV in ENDES

Table B.1: ENDES domestic violence module questions used to measure IPV

Type of violence	**Question**: Has your (last) husband (partner) ever...
Emotional	• said or done things to humiliate you in front of others? • threatened to harm you or someone close to you? • threatened to leave the house, take away your children, or withdraw financial support?
Physical (moderate)	• pushed you, shaken you, or thrown something at you? • slapped you or twisted your arm? • hit you with his fist or with something that could hurt you? • kicked or dragged you?
Physical (severe)	• tried to strangle or burn you? • attacked/assaulted you with a knife, gun, or other type of weapon? • threatened you with a knife, gun, or other type of weapon?
Sexual	• used physical force to make you have sexual relations even if you did not want to? • forced you to perform sexual acts that you do not approve of?
For all "Yes" answers, the respondent was then asked: "How often in the last 12 months?" with the options "Very often / Sometimes / Never". In cases where the respondent provided at least one "Yes" in any of the physical or sexual questions, she was then asked: "How long after you married/began living with your (last) husband (partner) did those things start to happen?"	

Notes: This table reproduces the questions from the ENDES domestic violence module used to measure emotional, physical, and sexual IPV.

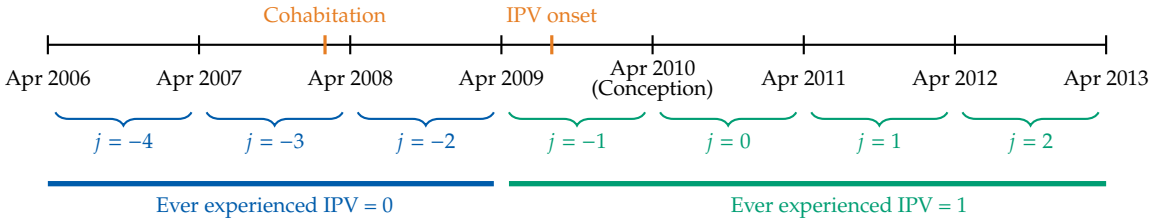
Appendix C Additional details on dataset construction

This appendix describes in detail how we construct the panel used in the stacked difference-in-differences estimation. We begin by constructing a monthly timeline centered around the month–year of conception. For women who are pregnant with their first child at the time of the survey, we compute the month–year of conception using the interview date and reported gestational duration. For women who already had their first child, we use the month–year of birth and assume a gestational length of nine months.

As explained in Section 3, the outcome variable is defined as an indicator equal to one if, by a given period, the woman had already experienced physical or sexual IPV by her most recent partner. For women who report never having experienced violence, the outcome variable is set to zero in all periods. For women who report having experienced violence, the outcome is coded as zero prior to the start of cohabitation and switches to one from the assigned month of IPV onset onward. IPV onset is reported by ENDES in yearly intervals since cohabitation began (coded as 0 if it occurred before the first anniversary, 1 if it occurred between the first and second anniversary, and so on). Because the survey does not record the exact month in which violence began, onset is interval-censored within a 12-month window. In our baseline specification, when a woman reports that violence began during a given year of cohabitation, we assign onset to the middle of that 12-month interval (e.g., six months after cohabitation began if it was reported to occur before the first anniversary). We also examine robustness to alternatively assigning onset to the beginning of the reported interval.

After assigning IPV onset at the monthly level, we aggregate the data to annual periods defined as 12-month windows relative to the month–year of conception. For example, if a woman conceived in April 2010, the year immediately preceding conception spans April 2009 through March 2010, the first post-conception year spans April 2010 through March 2011, and so on. Within each 12-month window, the outcome variable equals one if IPV onset has occurred at any point up to or within that window (i.e., we take the maximum value within the 12-month period).

To illustrate, consider a woman who conceived in April 2010, began cohabiting in February 2008, and reported that violence started after one year of cohabitation (i.e., between the first and second anniversary). Under the midpoint rule, we assign IPV onset to August 2009 (18 months after cohabitation began). In this case, the outcome variable equals zero up to two periods before conception, equals one starting in the year immediately preceding conception (April 2009–March 2010), and remains one thereafter, as depicted below:


Cohabitation
IPV onset
Apr 2006
Apr 2007
Apr 2008
Apr 2009
Apr 2010
(Conception)
Apr 2011
Apr 2012
Apr 2013
j = −4
j = −3
j = −2
j = −1
j = 0
j = 1
j = 2
Ever experienced IPV = 0
Ever experienced IPV = 1