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**“Don’t Leave Me Alone”:  
Unilateral Divorce and Intimate Partner Violence**

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# **“Don’t Leave Me Alone”: Unilateral Divorce and Intimate Partner Violence**

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## **Abstract**

The paper investigates the dynamic effects of unilateral divorce legalization on intimate partner violence (IPV) in Mexico, building upon existing evidence. It adopts a heterogenous-robust event study, leveraging data from a repeated cross-sectional survey and exploiting the staggered implementation of the reform across Mexican states. The policy led to a 7.2% increase in physical IPV in the medium term, specifically between 5 to 9 years after its introduction. No significant effects were observed in shorter or longer time frames, nor on other forms of IPV. The main findings are robust across various sensitivity analyses, addressing implementation date discrepancies, potential confounders, and alternative estimation methods. The rise in physical IPV is primarily driven by women who remained married, with suggestive evidence indicating both a backlash effect and an instrumental use of violence. The analysis contributes to the literature by looking at long-term impacts, increasing the external validity of shorter-run effects, adopting a new methodology, and deeply investigating the underlying mechanisms. Overall, it underscores the importance of addressing gender norms together with enhancing women's outside options.

*Keywords:* unilateral divorce; intimate partner violence; heterogenous-robust event study; Mexico.

*JEL Classification:* J12, I18, K36.

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## 1. Introduction

Intimate partner violence (IPV) is a pervasive social, health, and human rights issue with adverse consequences on the victims' physical and mental health and economic outcomes, and negative externalities on children (WHO, 2012). There is growing literature investigating policies that could potentially mitigate IPV, such as facilitating access to divorce. However, *a priori*, the impact of such reforms is not straightforward. On the one hand, these measures may decrease IPV by raising the probability that the abused spouse divorces or by strengthening her<sup>1</sup> *bargaining power* within intact marriages (Brassiolo, 2016; Corradini & Buccione, 2023; Stevenson & Wolfers, 2006). On the other hand, these reforms may inadvertently lead to an increase in IPV due to *backlash* or the use of violence as an *instrument* to prevent divorce (Silverio Murillo, 2019; García-Ramos, 2021). Hence, it becomes an empirical inquiry to comprehend the ultimate impact and its dynamics. As unilateral divorce remains illegal in some countries or its access is limited, particularly for women in low- and middle-income countries (OECD, 2019), a better understanding of the effects and channels is crucial for designing policies that address the possible unintended drawbacks of reforms aimed at enhancing women's outside options.

The paper examines the dynamic effects - and underlying mechanisms - of unilateral divorce legalization on IPV in Mexico, extending the previous work of García-Ramos (2021). The analysis leverages the staggered implementation of unilateral divorce across Mexican states, beginning with Mexico City in 2008 and still pending official adoption in seven states. The primary data source is the *Encuesta Nacional sobre la Dinámica de las Relaciones en los Hogares* (ENDIREH): a representative cross-sectional survey on gender-based violence conducted in 2003, 2006, 2011, 2016, and 2021. The sample is restricted to include only women affected by the reform and the outcome is represented by dichotomous variables for having experienced any, physical, sexual, emotional, or economic IPV in the 12 months preceding the survey. The identification strategy employed is an event study design, including indicators for the number of survey rounds before and after the legalization of unilateral divorce, state and survey-year fixed effects, and individual and household-level controls. The study adopts the estimator proposed by Sun and Abraham (2021) to account for possible heterogeneous effects across treated cohorts as well as over time.

The analysis reveals that the legalization of unilateral divorce led to an increase in physical IPV by 7.2% in the medium run (5 to 9 years after implementation), compared to the 2006 average in treated states of 0.097. Moreover, there were no statistically significant effects observed in the shorter or

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<sup>1</sup> Despite people of all genders may be victims or perpetrators of IPV, in the analysis I refer to the most common case (WHO, 2012): women victimized by men.

longer time frames nor on the other forms of violence, while there were pre-trend issues for sexual IPV. Despite some differences, these findings remained consistent with a battery of robustness checks. In particular, alternative policy implementation dates were considered due to inconsistencies in the *de jure* dates of the reform and between *de jure* and *de facto* dates. Additionally, state-level time-varying confounders and public policies introduced during the timeframe considered were controlled for. Moreover, I discussed whether the results hold using alternative estimation and inference methods and conducting other sensitivity tests.

Given the overall robustness of the findings, the study investigates the potential underlying mechanisms. The results suggest that the increase in physical IPV is primarily driven by women who remain married, indicating changes in intra-household dynamics. If the increase is due to violence as an *instrument* to prevent divorce, it would be more prevalent among married women with greater outside options, such as higher education, urban residence, or non-indigenous background. On the other hand, if it is driven by *male backlash*, it is expected that the reform would have affected other variables related to gender norms, such as employment, attitudes, and decision-making power. The analysis provides suggestive evidence supporting both hypotheses. Indeed, the effect is observed only for married women with primary or secondary school, living in urban areas, or without an indigenous background. At the same time, the reform affected married women's employment probability and decision-making power. Interestingly, these changes in variables potentially associated with *backlash* were more pronounced among women with greater outside options, suggesting the simultaneous presence of both channels.

The paper fits into three main strands of the literature. The first investigates the effects of easier access to divorce on various dimensions, such as women's labor supply, saving decisions, and children's outcomes (Corradini & Buccione, 2023; Gruber, 2004; Heggeness, 2020; Hoehn-Velasco & Penglase, 2021b; Stevenson, 2008; Voena, 2015). The second strand studies the effect on IPV of different factors and policies, such as prosecution and enforcement policies (Chin & Cunningham, 2019; Iyengar, 2009; Sviatschiy & Trakoz, 2021), labor market conditions (Aizer, 2010; Anderberg et al., 2015; Bhalotra et al., 2020; Bhalotra et al., 2021; Clerici & Tripodi, 2021; Dhanaraj & Mahambare, 2021; Heath, 2014; Kotsadam & Villanger, 2020; Perova et al., 2021; Sanin, 2021), and cash transfers (Angelucci, 2008; Bobonis et al., 2020; Hidrobo et al., 2016; Peterman et al., 2022). Thirdly, and more specifically, the paper contributes to the literature that examines the impact of more liberal divorce laws on intimate partner violence against women in heterosexual relationships. It does so further proving the possible unintended drawbacks of these policies, as already shown by Silverio Murillo (2019) and García-Ramos (2021) in Mexico, but differently from studies conducted in the

United States, Spain, and Egypt (Brassiolo, 2016; Corradini & Buccione, 2023; Stevenson & Wolfers, 2006). Moreover, it stresses the dynamic nature of the effects.

Considering the Mexican reform, the analysis extends existing evidence by examining a longer time frame, increasing the external validity of the estimated impacts in the short and medium terms, accounting for all inconsistencies in the reform dates, employing an empirical strategy robust to potential heterogeneous effects among cohorts of states that implemented unilateral divorce at different periods, and conducting an in-depth investigation of the potential mechanisms involved. The findings of this study are consistent with García-Ramos (2021) regarding the absence of short-term effects and the subsequent increase in physical IPV in the medium run. However, this analysis specifically identifies the effect on physical IPV, with no discernible impact on other forms of IPV. Moreover, there is suggestive evidence that the observed effect does not endure in the long run. Regarding the underlying mechanisms, the study points to the utilization of *instrumental violence* against married women who completed primary and secondary school, aligning with García-Ramos (2021). Additionally, it suggests a potential protective role of higher educational attainments. The analysis extends the exploration of this channel by considering that women living in urban areas or without an indigenous background may have higher outside options in the Mexican context. Furthermore, in contrast to García-Ramos (2021), this study reveals that the reform led to increased labor market outcomes for married women and it incorporates measures of decision-making power as additional potential drivers of *male backlash*. Lastly, the analysis underscores the interplay of *instrumental violence* and *backlash* mechanisms. The medium-run increase in physical IPV appears to be driven by married women with more outside options—the same groups that experienced significant benefits from the policy in terms of improved labor market outcomes and decision-making power within the relationship. Overall, the analysis highlights that a policy facilitating relationship exit may have adverse impacts if not combined with changes at the community and society levels addressing attitudes and norms toward divorce and female empowerment.

The remainder of the paper is structured as follows. Section 2 provides an overview of the theoretical and empirical literature concerning the relationship between divorce access and intimate partner violence, while Section 3 offers insights into the Mexican context and the reform. Section 4 discusses the data employed, and Section 5 outlines the empirical strategy adopted. The main results of the analysis are presented in Section 6, followed by a battery of robustness checks in Section 7. Section 8 delves into the potential channels underlying the observed effect, and Section 9 concludes.

## 2. Divorce Access and Intimate Partner Violence

The analysis investigates the effect of unilateral divorce legalization, or generally easier access to divorce, on intimate partner violence. Theoretically, the impact may move through various channels, with an ambiguous overall direction. Firstly, the reform may facilitate the *exit* of the abused spouse from a violent marriage, thereby reducing IPV<sup>2</sup>. Secondly, changes in divorce legislation may redistribute the *bargaining power* within intact couples by making the threat of leaving the marriage more credible. Therefore, the reform may enhance women's empowerment and reduce IPV in intact marriages by increasing their outside options<sup>3</sup> (Brassiolo, 2016; Chiappori et al., 2002; McElroy & Horney, 1981). However, it is also plausible that easier access to divorce could lead to an increase in IPV. Indeed, the heightened threat of separation may escalate conflict (Anderson & Genicot, 2015), and violence may be employed as an *instrument* to prevent marriage dissolution. Additionally, the greater female independence possibly resulting from these reforms may trigger *male backlash* if it is perceived as challenging traditional gender norms (Hornung et al., 1981; Macmillan & Gartner, 1999).

Given the contrasting theoretical predictions, whether unilateral divorce implementation results in increased or decreased IPV is an empirical question. Relevant studies in this area include those by Brassiolo (2016), Corradini & Buccione (2023), García-Ramos (2021), Hoehn-Velasco & Silverio Murillo (2020), Silverio Murillo (2019), and Stevenson & Wolfers (2006). Stevenson and Wolfers (2006) observed a decline in domestic violence and female suicides and homicides following the introduction of unilateral no-fault divorce in the United States. They attribute these findings to both increased divorce rates and enhanced *bargaining power* for women within households. Similarly, Brassiolo (2016) found negative effects on IPV among married couples compared to unmarried ones in Spain. In the Egyptian context, Corradini and Buccione (2023) discovered a negative impact of divorce law changes on domestic abuse for married women with children below the custody threshold for mothers. Conversely, García-Ramos (2021), Hoehn-Velasco & Silverio Murillo (2020), and Silverio Murillo (2019) found evidence of no changes or increases in IPV following unilateral divorce legalization in Mexico, aligning with hypotheses of *male backlash* and/or *instrumental use* of violence. These papers have studied the effect of the reform on IPV and women's empowerment using different data sources, reform implementation dates, and empirical strategies<sup>4</sup>. Silverio Murillo

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<sup>2</sup> This channel implicitly assumes that the ex-partner does not continue to exert violence, or exerts it to a lower extent, after the dissolution of the relationship.

<sup>3</sup> The unilateral divorce legalization increases the outside options of the spouse more willing to exit the marriage, which in the case of violent marriages is the abused spouse, and women are the most at risk of suffering IPV (WHO, 2012). Indeed, the reform seems to have benefitted women more than men, as explained in Section 3.

<sup>4</sup> See Table A.2 in the Appendix for a scheme of the literature regarding the Mexican unilateral divorce legalization effects on women's empowerment.

(2019) compared married and cohabitating couples before and after the Mexican Supreme Court resolution in 2015 in favor of unilateral divorce. The author found an increase in emotional and economic IPV, no effects on women's decision-making power, and heterogeneous effects on women's labor supply. Hoehn-Velasco & Silverio Murillo (2020) used an event study design based on the *de facto* no-fault divorce legalization dates<sup>5</sup> across states and did not observe any significant change in female suicide and homicide rates. Finally, García-Ramos (2021) adopted a dynamic two-way fixed effects (TWFE) identification strategy and the *de jure* legalization dates<sup>6</sup>. The study found an increase in any, physical, emotional, and economic IPV, occurring five to nine years after the reform implementation and for women who remain married. Moreover, the author did not find significant effects on women's labor supply or IPV attitudes, while results held only for women who have completed at least primary education (a proxy for outside options). The findings suggested that the *instrumental use* of violence to prevent divorce may be the underlying mechanism.

Lastly, it is essential to emphasize the dynamic nature of the potential effects. Existing literature has demonstrated that the legalization of no-fault divorce might result in a temporary increase in divorce rates, which could eventually diminish or even reverse after some years (Hoehn-Velasco & Penglase, 2021a; Wolfers, 2006). Similarly, the impacts on IPV and other factors related to women's empowerment could exhibit dynamic patterns. Indeed, partners' behaviors and external conditions might react differently over time to changes in the law (García-Ramos, 2021; Hoehn-Velasco & Penglase, 2021b).

Therefore, the paper extends existing evidence, specifically the analysis by García-Ramos (2021), using more recent data, allowing to explore longer-term effects and increase the external validity of the estimated impacts in the short and medium-run, as more states have implemented the policy for a longer period. Moreover, the study enhances the empirical strategy by adopting an estimator robust to possible heterogeneous effects. Finally, it integrates the investigation of the mechanisms by considering additional factors related to outside options and *backlash* triggering, and by highlighting the channels' interplay.

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<sup>5</sup> The *de facto* legalization date is defined as the quarter-year in which there were more than 10 cases of unilateral divorce in the state. A broader discussion about the *de facto* dates and the Supreme Court resolution can be found in Section 7.1.

<sup>6</sup> The *de jure* legalization date is the date at which the reform was approved by the state's Legislative power.

### 3. Institutional Background and Reform

The analysis focuses on the context of Mexico, a federal country consisting of 31 states and Mexico City, where the divorce legislation is determined at the state level. Before the reform of interest, there were three types of divorce recognized (Mendez Sanchez, 2014; Hoehn-Velasco & Penglase, 2021a). The first type was administrative divorce, which was the quickest and required mutual consent, no children, and agreement on the division of assets. If the couple had children or could not agree on the assets' division, they could choose a judicial divorce with mutual consent. The third type was judicial divorce with cause, where one spouse could file for divorce based on specific grounds, such as adultery, abandonment, incurable illness, or domestic violence<sup>7</sup>. This kind of divorce was available when one of the spouses did not agree on the dissolution of the marriage. From 2008 onwards, several states have introduced the possibility of filing for divorce unilaterally and without the need to prove cause: the so-called “unilateral”, “no-fault”, or “express” divorce. The implementation of unilateral divorce began in Mexico City in 2008, followed by Hidalgo in 2011, and subsequently by the majority of other states in the following years. Table 1 provides information on the month and year when each state legalized no-fault divorce. These dates represent when the state officially modified the relevant articles in the Civil or Family Code, referred to as the “*de jure*” date of legalization. As of the last quarter of 2021, only seven states had not yet made the necessary changes to their Civil or Family Codes to allow for no-fault divorce.

The reform significantly increased divorce rates, with an approximate 30% rise in the initial years following its implementation, according to the literature<sup>8</sup> (Aguirre, 2019; Hoehn-Velasco & Penglase, 2021a). The policy also reduced the time and financial costs associated with divorce (García-Ramos, 2021; Hoehn-Velasco & Penglase, 2021a). These changes are particularly beneficial for individuals in violent marriages, as the abused spouse can now exit the relationship without requiring consent or evidence of domestic violence, which is often difficult to prove. Given that women are at a higher

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<sup>7</sup> Some states introduced domestic violence as a possible cause for divorce during the time frame of the analysis (see Appendix Table A.10). Therefore, in Section 7.2, I control for this reform as a possible confounder, and the results hold. Moreover, Beleche (2019) did not find any significant effect of the policy on female suicides.

<sup>8</sup> Appendix Figure B.1 and Table A.3 display the dynamic effects of the reform on the probability of being married, separated, or divorced at the time of the survey, using the Sun & Abraham (2021) event study estimator, the 2006-2021 ENDIREH data, and the *de jure* reform dates (see Section 4 and 5). The sample includes all the women interviewed in the survey. There are negative effects on the probability of being married in all periods after the policy implementation and positive effects on the probability of being separated in the short and long runs. The probability of being divorced increases in the short run by 19.2% relative to the 2006 average in treated states. Moreover, in the medium run, it points to a positive direction while in the long term to a negative one, but these effects are not significantly different from 0. Appendix Figure B.2 and Table A.3 show the results of the same analysis using the *de facto* dates of the policy (see Section 7.1), finding non-significant effects on the likelihood of being married or separated, but an increase in the probability of being divorced in the short-term by 30.8%. Therefore, the analysis confirms the relevance of the policy and indicates the importance of taking into account both the *de jure* and the *de facto* dates of its implementation.



risk of IPV, they have particularly benefited from this reform, as evidenced by the fact that wives have filed for unilateral divorce in larger numbers compared to husbands (Hoehn-Velasco & Penglase, 2021a). Additionally, women may have benefited more from the reform's economic compensation changes, which tend to favor the spouse who was primarily taking care of household chores and had fewer assets, who is usually the wife (García-Ramos, 2021).

**Table 1.** Unilateral divorce legalization dates

State	<i>De Jure</i> Date	Source	Article
Distrito Federal	Oct-08	Civil Code	266
Hidalgo	Mar-11	Family Code	102
Guerrero	Mar-12	Divorce Law	4
Yucatán	Apr-12	Family Code	191
México	May-12	Civil Code	4.89
Sinaloa	Feb-13	Family Code	181
Coahuila de Zaragoza	Apr-13	Civil Code	362
Nayarit	May-15	Civil Code	260
Aguascalientes	Jun-15	Civil Code	288
Tamaulipas	Jul-15	Civil Code	248
Michoacán de Ocampo *	Oct-15	Family Code	254
Tlaxcala	Feb-16	Civil Code	123
Colima	Mar-16	Civil Code	267
Morelos	Mar-16	Family Code	174
Puebla	Mar-16	Civil Code	442
Querétaro *	Nov-16	Civil Code	246
Baja California Sur	Dec-16	Civil Code	278
Nuevo León	Dec-16	Civil Code	273
Oaxaca	May-17	Civil Code	267
San Luis Potosí	May-17	Family Code	86
Quintana Roo *	Jul-17	Civil Code	798
Zacatecas	Sep-17	Family Code	214
Durango	Jul-18	Civil Code	261
Jalisco	Nov-18	Civil Code	404
Veracruz de Ignacio de la Llave *	Jun-20	Civil Code	141
Baja California		Civil Code	264
Campeche		Civil Code	281
Chiapas *		Civil Code	263
Chihuahua		Civil Code	255
Guanajuato		Civil Code	328
Sonora *		Family Code	141
Tabasco		Civil Code	257

*Notes:* own elaboration from Family and Civil Codes. The month and year refer to the date the Code changed. In pink there are the states that legalized unilateral divorce between November 2006 and October 2011, in green the ones between November 2011 and October 2016, in yellow the ones between November 2016 and October 2021, and in blue the states that have not changed their Codes in November 2021. \*: inconsistencies between the date that I have retrieved and the one reported in García-Ramos (2021), Hoehn-Velasco & Penglase (2021a,b), Aguirre (2019), and/or Silverio Murillo (2019).

## 4. Data and Descriptive Statistics

### 4.1 Data and Variables

The primary data source used in the analysis is the *Encuesta Nacional sobre la Dinámica de las Relaciones en los Hogares* (ENDIREH). The ENDIREH is a national and state representative cross-sectional survey on gender-based violence conducted during the fourth quarter of the years 2003, 2006, 2011, 2016, and 2021. The survey collects data through interviews with women aged 15 years or older, regardless of their relationship status (married or cohabitating; separated, divorced, or widow; or single). However, the 2003 survey round is distinct from the subsequent ones, being representative only of 11 states<sup>9</sup> and including information solely on women residing with a partner. Therefore, the main analysis is based on the rounds from 2006 to 2021, while the 2003 round is used for robustness checks. The survey was designed and conducted following the guidelines of the World Health Organization to minimize under-reporting and address privacy concerns (WHO, 2001)<sup>10</sup>.

The majority of the variables used in the analysis are constructed following García-Ramos (2021) to ensure comparability with her study. The ENDIREH provides detailed information on violence experienced in current or past relationships. In particular, 28 questions are consistent across the survey rounds and can be classified into the categories of physical, sexual, emotional, and economic intimate partner violence (see Appendix Table A.1 for the questions related to each kind of IPV). I construct four dichotomous variables for having experienced at least one act of physical or sexual violence or two acts of emotional or economic violence in the 12 months preceding the survey<sup>11</sup>. Additionally, an overall indicator variable is created, taking a value of 1 if any of these four dichotomous variables is positive. Importantly, the questions are comparable also for women who were not in a relationship at the time of the survey, asking about the experience of violence by the ex-partner in the preceding 12 months<sup>12</sup>. The ENDIREH provides relevant information also on women's socio-demographic characteristics. In particular, the covariates used in the analysis include women's age, urban or rural residence, indigenous background, educational level, socio-economic status (SES) index, and number of children. Moreover, for women who remain married, some

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<sup>9</sup> The 11 states for which the 2003 ENDIREH is representative are: Baja California, Coahuila de Zaragoza, Chiapas, Chihuahua, Hidalgo, Michoacán de Ocampo, Nuevo León, Quintana Roo, Sonora, Yucatán, and Zacatecas.

<sup>10</sup> Throughout the paper, I discuss the effects on IPV as changes in its prevalence, but it may also be that the reform affected the reporting rate. This concern is largely reduced considering that the ENDIREH survey was conducted following the WHO guidelines: respecting privacy and confidentiality, by ad-hoc trained female interviewers, and without expressively mentioning violence (WHO, 2001). Moreover, in her analysis, García-Ramos (2021) found a negative effect on reporting IPV to public authorities. This suggests that, if there is an effect on IPV reporting, it would likely be negative, and thus the estimates would represent a lower bound.

<sup>11</sup> The main results of the analysis do not change if the indicators for emotional and economic IPV are also coded as equal to 1 for having experienced at least one act of that kind of violence instead of two. Results upon request.

<sup>12</sup> The only exception is the 2006 ENDIREH round, in which women who were not currently in a relationship were asked about violence occurring since the time of separation or divorce, rather than within the previous year.

partner's characteristics are controlled for, such as age, indigenous background, educational level, and the relationship's length (see Appendix Table A.1 for the variables' definition).

To account for time-varying state-level factors, data from other surveys are used to construct relevant variables. In particular, the *Encuesta Nacional de Ocupación y Empleo* (ENOE) is used to construct the state and survey-year male unemployment rate, as a proxy for the economic cycle, and the gender gap in labor force participation, as a proxy for gender equality. Moreover, I used the National Institute of Statistics and Geography (INEGI)'s mortality records to construct the male homicide rate as a proxy for general violence in the state and year. Furthermore, information about policies introduced during the specified time frame and potentially affecting IPV is collected from other studies. In particular, I control whether and when the state has introduced domestic violence as a cause for divorce or has criminalized it (Beleche, 2019; García-Ramos, 2021), has banned marriage under the age of 18 (Bellés-Obrero & Lombardi, 2023), or has changed laws regulating abortion (Clarke & Muhlrud, 2021). The choice and construction of the possible confounders are further explained in Appendix Table A.1 and in Section 7.2.

#### **4.2 Sample and Descriptive Statistics**

The relevant sample for the analysis is constructed to include women who were married or separated at the time of the reform and have either remained so or divorced by the time of the survey (García-Ramos, 2021). Indeed, these are the women affected by no-fault divorce legalization. The sample construction process involves considering the woman's relationship status at the time of the survey, her age, her age at marriage/cohabitation, and the years since divorce/separation. For survey rounds conducted after the implementation of the reform, women are classified as affected if their relationship length exceeds the state's exposure to the policy in that survey year. Additionally, women whose divorce length is shorter than the state's exposure to the policy are also included as affected. In this way, I explicitly exclude women who married after or divorced before the unilateral divorce legalization since they may be systematically different (Alesina & Giuliano, 2007). To ensure comparability, for survey rounds conducted before the reform, the definition of the same state in the first round after the policy implementation is used. In the case of never-treated states, the average exposure to the reform per survey round among the treated states is utilized.

After applying the selection criteria and excluding observations with missing values in the outcome and control variables<sup>13</sup>, the main sample for the analysis consists of 240,703 women from the ENDIREH rounds conducted between 2006 and 2021. Among these 240,703 women, 85% were

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<sup>13</sup> The observations with missing values in any of the variables considered represent a small proportion (2.70%) of the sample.

married at the time of the survey, 14% were separated, and 1% had divorced (see Table 2). The IPV trends from 2006 to 2021 are depicted in Appendix Figure B.3. All types of IPV exhibit a monotonic downward trend over this period, except for physical IPV, which shows a non-monotonic pattern experiencing a slight increase in 2016 but subsequently decreasing again by 2021. In 2006, approximately 20% of the women in the sample reported experiencing at least one form of IPV in the last 12 months, while this percentage decreased to 11% in 2021.

**Table 2.** Descriptive statistics

	Summary
N	240,703
Any IPV	0.148 (0.36)
Physical IPV	0.071 (0.26)
Sexual IPV	0.031 (0.17)
Emotional IPV	0.098 (0.30)
Economic IPV	0.067 (0.25)
Any IPV - 2006 mean	0.196 (0.00)
Physical IPV - 2006 mean	0.097 (0.00)
Sexual IPV - 2006 mean	0.054 (0.00)
Emotional IPV - 2006 mean	0.121 (0.00)
Economic IPV - 2006 mean	0.093 (0.00)
Age	45.416 (13.50)
Relationship status	
Separated	33,794 (14.0%)
Divorced	1,914 (0.8%)
Married	204,995 (85.2%)
Urban residence	
Rural	50,423 (20.9%)
Urban	190,280 (79.1%)
Indigenous	
Non-indigenous	224,834 (93.4%)
Indigenous	15,869 (6.6%)
Education	
Not completed	16,885 (7.0%)
Primary	76,246 (31.7%)
Secondary	103,179 (42.9%)
Higher	44,393 (18.4%)
Number of children	3.287 (2.22)
SES index	
Low SES	89,278 (37.1%)
Middle SES	98,639 (41.0%)
High SES	52,786 (21.9%)

*Notes:* The table shows the summary statistics of the main variables used in the analysis for the sample of women affected by the reform. For continuous variables, it displays the mean and standard deviation (in parenthesis), while for factor variables the frequency and percentage (in parenthesis). *Source:* 2006-2021 ENDIREH.

## 5. Empirical Strategy

### 5.1 Event Study Specification

The identification strategy relies on the variation in reform timing across different states. Based on the dates of changes in the Civil or Family Codes, four groups of states were formed, as indicated in Table 1. Two states had legalized unilateral divorce before the 2011 ENIDREH survey round, 13 states before the 2016 round, 10 states before the 2021 round, and 7 states had not made any changes to their codes as of October 2021 (considered as "never-treated" states). Exploiting this variation, I adopt an event study specification including leads and lags: indicators for the numbers of survey rounds before and after the introduction of the reform in each state. The event study specification is similar to a dynamic two-way fixed effect model allowing for the estimation of the effect over time, and it includes leads to immediately investigating potential pre-trends. Therefore, I estimate:

$$IPV_{isy} = \sigma_s + \lambda_y + \sum_{k=-2}^{-1} \beta_k UD_{sy}^k + \sum_{k=1}^3 \beta_k UD_{sy}^k + \gamma X_{isy} + \varepsilon_{isy}$$

where  $IPV_{isy}$  is a dichotomous variable indicating whether woman  $i$ , residing in state  $s$ , in survey-year  $y$ , has experienced physical, sexual, emotional, or economic IPV in the preceding 12 months. The main focus is on the effects of unilateral divorce legalization, captured by the indicators  $UD_{sy}^k$ , which represent whether state  $s$  in survey year  $y$  has implemented the policy by  $k$  survey rounds. The excluded reference category is the survey year prior to the reform, following common practice.  $X_{isy}$  are individual, household, and – in the robustness checks – state-level time-varying controls. The basic controls include: woman's age, indigenous background, urban/rural residence, educational level, number of children, and SES index<sup>14</sup>. The state fixed effects ( $\sigma_s$ ) account for time-invariant heterogeneity across states, while the survey round fixed effects ( $\lambda_y$ ) capture year-specific shocks and differences in IPV common to all cohorts of states. The error term is denoted as  $\varepsilon_{isy}$ , and clustering is performed at the state level to account for potential correlations within states. The coefficients of interest are represented by  $\sum_{k=1}^3 \beta_k$ , which estimate the effects of unilateral divorce legalization at different time intervals (0-4, 5-9, and 10-14 years) after its implementation. These coefficients measure the change in IPV difference between women in treatment and control states  $k$  survey rounds after the reform, relative to the last period prior to treatment. Additionally, the

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<sup>14</sup> It should be noted that the reform may have affected women's educational attainment, socio-economic status, number of children, and residence. I include these variables as controls in the analysis, but excluding them would not change the main results.

coefficients  $\sum_{k=-2}^{-1} \beta_k$  are examined to assess whether there were any significant differences in trends between the treatment and control cohorts before the implementation of the reform.

## 5.2 Hypotheses

The validity of the empirical strategy relies on several assumptions, including parallel trends, no anticipation, stable unit treatment value, and homogenous treatment effects. In this Section, I briefly discuss each of them. The parallel trends assumption posits that the treatment and control groups would have followed similar trends over time in the absence of the reform. Specifically, it implies that the evolution of IPV rates would have been similar between states that implemented unilateral divorce and those that did not. While this hypothesis cannot be directly tested, it is common practice to conduct various checks to assess its plausibility. The first of these is a balance test on pre-treatment characteristics between treatment and control groups. Table 3 presents the results of t-tests on the mean differences in 2006 for individual covariates, indicators for marital status, and some states' characteristics<sup>15</sup> between each of the three cohorts of treated states and the never-treated one<sup>16</sup>. The second and third cohorts of treated states appear to be well-balanced in terms of pre-treatment characteristics when compared to the never-treated states. However, the cohort of states that legalized no-fault divorce before the 2011 round exhibits differences from the never-treated cohort on a larger number of dimensions. Prior to the implementation of the reform, women in Mexico City and Hidalgo were older, more educated, and with fewer children compared to women in never-treated states. Additionally, Mexico City and Hidalgo had higher male unemployment rates and lower gender gaps in labor force participation. While these differences may raise concerns, in the analysis I control for these variables, and it is important to remember that the balance test examines the levels of pre-treatment characteristics and does not provide information on the dynamics of these groups, which is what is crucial for assessing the validity of the identification strategy. Indeed, the most popular test consists of examining whether the treatment and control cohorts were moving along similar trends before the treatment, assessing the leads of the event study specification. While this approach does not definitively prove that the groups would have followed similar trends in the absence of the reform, it serves as the closest approximation to testing this assumption. This test is directly discussed when presenting the results (see Section 6).

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<sup>15</sup> "DV as cause for divorce" and "DV in Penal Code" are dichotomous variables equal to 1 if, in 2006, the state had domestic violence as a possible cause for divorce or had criminalized it, respectively.

<sup>16</sup> Appendix Table A.4 shows the balance test between treated and never-treated states. The treated and control groups look very similar in their pre-treatment characteristics, with only the 2006 level of male unemployment rate being slightly higher in treated states.

**Table 3.** Balance table: Treated cohorts VS never-treated

Variable	(1) Never- treated Mean/SE	(2) Treated bf 2011 Mean/SE	(3) Treated bf 2016 Mean/SE	(4) Treated bf 2021 Mean/SE	(2)-(1) Pairwise t- test Mean difference	(3)-(1) Pairwise t- test Mean difference	(4)-(1) Pairwise t- test Mean difference
<i>Individual characteristics</i>							
Age	38.162 (0.428)	39.911 (0.623)	38.106 (0.352)	38.615 (0.427)	1.749**	-0.056	0.453
Urban residence	0.750 (0.056)	0.907 (0.142)	0.814 (0.034)	0.745 (0.057)	0.157	0.064	-0.004
Indigenous	0.055 (0.035)	0.053 (0.053)	0.061 (0.020)	0.074 (0.038)	-0.003	0.005	0.019
Education	1.547 (0.109)	1.886 (0.139)	1.649 (0.042)	1.614 (0.064)	0.338*	0.102	0.067
Number of children	2.912 (0.135)	2.304 (0.260)	2.789 (0.094)	2.819 (0.084)	-0.607**	-0.123	-0.093
SES index	1.684 (0.111)	1.952 (0.199)	1.668 (0.047)	1.764 (0.128)	0.268	-0.016	0.080
Married	0.506 (0.030)	0.450 (0.018)	0.507 (0.017)	0.531 (0.023)	-0.056	0.001	0.025
Divorced	0.012 (0.003)	0.014 (0.003)	0.008 (0.001)	0.008 (0.001)	0.002	-0.004	-0.004
<i>States' characteristics</i>							
Male unemployment rate	2.692 (0.368)	4.722 (0.813)	3.613 (0.407)	3.037 (0.319)	2.030**	0.921*	0.345
Gender gap in LFP	42.288 (3.441)	31.937 (3.744)	37.787 (0.554)	39.762 (1.579)	-10.351**	-4.501	-2.526
Male homicide rate	19.126 (4.572)	13.769 (4.359)	21.562 (3.661)	11.899 (2.120)	-5.357	2.435	-7.227
DV as cause for divorce	0.614 (0.223)	0.797 (0.324)	0.512 (0.214)	0.720 (0.199)	0.183	-0.102	0.106
DV in Penal Code	0.964 (0.040)	1.000 (0.000)	0.975 (0.028)	0.945 (0.060)	0.036	0.010	-0.020
Number of observations	27068	7780	50941	38367	34848	78009	65435
Number of clusters	7	2	13	10	9	20	17

*Notes:* The table shows the difference in means for the treated cohorts VS the never-treated one in 2006, using sample weights. The sample consists of all women interviewed, dropping observations with missing values. The errors are clustered at the state level. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

Finally, it is implicit in the parallel trends assumption that the treatment is not endogenous. Although direct testing of this hypothesis is not feasible, Table 4 provides evidence supporting the exogeneity of the treatment investigating whether the level of IPV in 2006 predicts the likelihood and timing of unilateral divorce legalization.

**Table 4.** Exogeneity of reform status and timing

VARIABLES	(1) Treated 0-1	(2) Cohort 0-3	(3) Cohort 1-3
Any IPV	0.018** (0.008)	0.017 (0.016)	-0.008 (0.013)
Age	0.000 (0.001)	0.003** (0.001)	0.003** (0.001)
Urban residence	-0.031 (0.021)	-0.002 (0.040)	0.030 (0.027)
Indigenous	0.062 (0.070)	0.213 (0.147)	0.079 (0.131)
Education level	0.010 (0.015)	0.047* (0.027)	0.035* (0.018)
Number of children	0.002 (0.004)	-0.008 (0.007)	-0.009* (0.005)
SES index	-0.014 (0.023)	-0.108*** (0.039)	-0.084** (0.034)
Married	0.009 (0.021)	-0.018 (0.041)	-0.033 (0.024)
Divorced	-0.030 (0.024)	-0.054 (0.045)	0.002 (0.027)
Male unemployment rate	0.074 (0.059)	0.263** (0.118)	0.161* (0.085)
Gender gap in LFP	-0.026* (0.013)	-0.065*** (0.021)	-0.061** (0.028)
Male homicide rate	-0.006 (0.007)	0.001 (0.015)	0.010 (0.009)
DV as cause for divorce	0.179 (0.147)	0.090 (0.351)	-0.201 (0.340)
DV in Penal Code	0.131 (0.226)	0.382 (0.504)	0.238 (0.556)
Constant	1.382* (0.697)	2.588** (1.146)	3.233*** (1.135)
Observations	98,569	98,569	76,750
R-squared	0.210	0.273	0.261

*Notes:* The table investigates whether the prevalence of IPV in 2006 predicts the implementation of UD reform in (1), its timing in (2), and its timing conditional on treatment in (3). The dependent variables of (2) and (3) are coded so that higher levels represent earlier reform: 0 for never-treated states (only for (2)), 1 for the states that legalized UD after 2016, 2 for those who legalized after 2011, and 3 for those who legalized between 2006 and 2011. The specifications control for individual and state-level variables in 2006, considering all women interviewed. The errors are clustered at the state level. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.



The model controls for individual and states' characteristics and considers all women interviewed. Column (1) examines whether the level of IPV in 2006 predicts the legalization of unilateral divorce, while columns (2) and (3) examine the timing of the reform, both unconditionally and conditional on treatment, respectively. The dependent variables in columns (2) and (3) are coded such that higher levels represent an earlier implementation of the no-fault divorce reform. Table 4 provides evidence that the pre-treatment prevalence of IPV is positively correlated with the treatment status, but not with its timing<sup>17</sup>. This suggests that the timing of unilateral divorce legalization may be exogenous to the outcome. Nevertheless, it is important to control for other variables related to the timing of the reform, as done in the main analysis and robustness checks.

The second assumption is that there should not be anticipatory behaviors that cause treatment effects to occur before the actual implementation of the reform. The no anticipation assumption is readily tested with the leads of the event study specification. Additionally, possible threats to this assumption, such as the Supreme Court resolution of 2015 and *de facto* unilateral divorce legalization dates, are discussed in Section 7.1. The third assumption, the Stable Unit Treatment Values Assumption (SUTVA), implies that there should be no spillover effects. There is no apparent geographical clustering of the reform status and timing, but potential changes in inter-state migration in response to the policy could challenge this hypothesis. Although women may move to benefit from no-fault divorce, I argue that it is unlikely for couples to agree to change residence solely to take advantage of the legalization of unilateral divorce. Furthermore, García-Ramos (2021) finds no evidence of the policy's effect on inter-state immigration rates. The last assumption of the identification strategy is that the treatment effect is homogeneous. However, the homogeneity hypothesis can be relaxed by adopting the event study estimator proposed by Sun and Abraham (2021), as subsequently explained.

In recent years, extensive literature has shown that the classic two-way fixed effects (TWFE) estimator fails to identify the average treatment effect on the treated (ATT) when the treatment is staggered and the effect is heterogeneous across time and/or units (Goodman-Bacon, 2021; de Chaisemartin & D'Haultfœuille, 2022; Roth et al., 2022). With variation in treatment timing, the TWFE coefficient is a weighted average of cohorts-specific treatment effects. The issue arises because the TWFE estimator compares not only treated units with not-yet-treated or never-treated units but also later treated units with earlier treated units: the so-called "forbidden comparisons". These can introduce bias if the treatment effect is heterogeneous across time and/or units. Moreover, the weights that the TWFE gives to any two-by-two comparison are often not straightforward nor

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<sup>17</sup> Note that both the treatment status and timing appear exogenous to the pre-treatment level of physical IPV, the outcome for which I observe statistically significant effects.

relevant. To address these concerns, several heterogeneity-robust TWFE estimators have been proposed (de Chaisemartin & D’Haultfœuille, 2020; Borusyak et al., 2021; Callaway & Sant’Anna, 2021; Sun & Abraham, 2021). The event study methodology is a specific type of TWFE, namely a dynamic TWFE that includes leads of the effect, and thus suffers from similar issues. Sun and Abraham (2021) have developed an event study estimator that is robust to possible heterogeneous effects across cohorts as well as across time. In the context of this analysis, their estimator is a regression-based weighted average of treatment effects for  $k$  survey rounds relative to treatment, using as controls never-treated states<sup>18</sup>, with weights given by the shares of each cohort for that  $k$ <sup>19</sup>. Therefore, the main analysis makes use of this estimator, allowing the effect to be heterogeneous across the three cohorts of treated states and providing more reliable estimates of the treatment effects in the presence of staggered treatment timing and heterogeneous effects<sup>20</sup>.

There are a few important notes to consider before discussing the main results. Firstly, it is worth noting that there are only two states, Mexico City and Hidalgo, that have legalized unilateral divorce by 3 survey rounds at the time of the 2021 round. This means that the external validity of the coefficient  $\beta_3$  may be limited. Secondly, in the main specification, I am using the 2006-2021 ENDIREH years, which provide only one pre-reform period for Mexico City and Hidalgo. Therefore, the examination of pre-trends for these states will be further investigated in the robustness checks using the 2003 round, as discussed in Section 7.4. Lastly, it is important to highlight that the analysis still assumes a homogeneous treatment effect across states within the same cohort (implementing the policy between the same two survey rounds), which represents a limitation of the current analysis.

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<sup>18</sup> The study is conducted using the never-treated states as controls since the treatment timing unconditionally from treatment seems to be exogenous (see column (2) of Table 4). Moreover, note that the use of the last treated cohort as the control group would not allow the investigation of longer-run effects and it would return the analysis to a setting very similar to that of García-Ramos (2021). In this regard, see Appendix Table A.5 for a replication of García-Ramos (2021) analysis with the Sun and Abraham (2021) estimator.

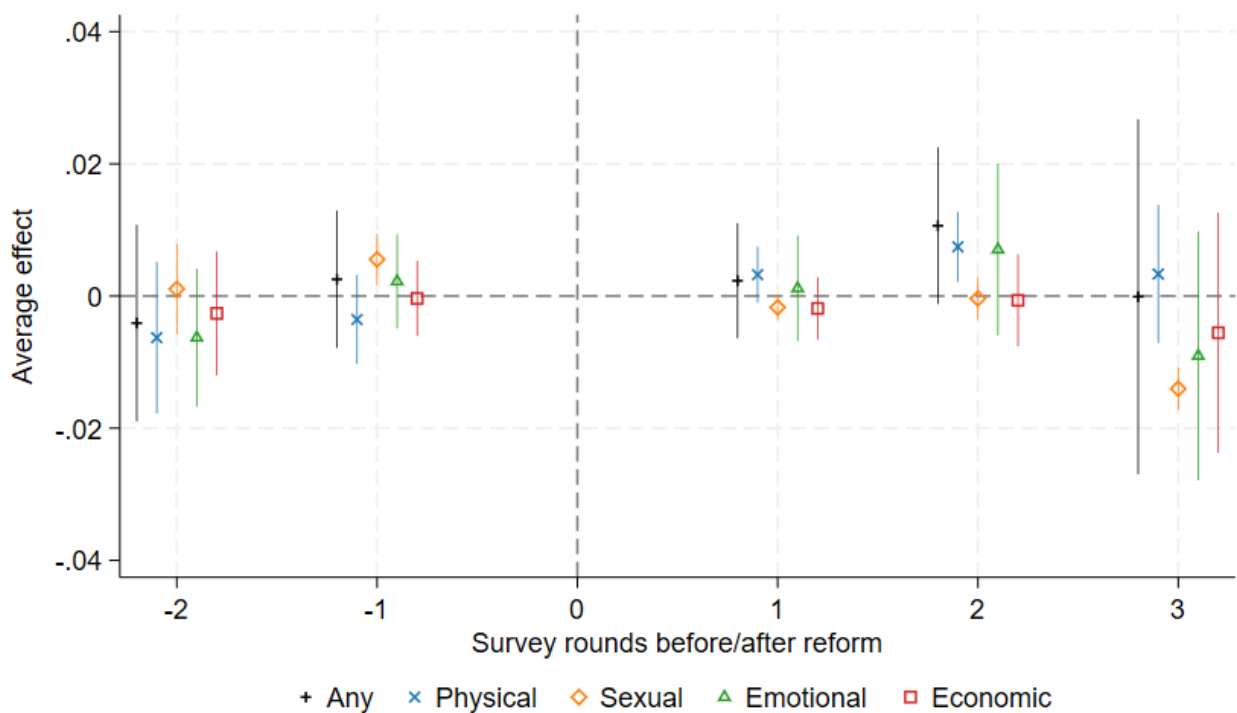
<sup>19</sup> More specifically, the estimator follows three steps. Firstly, it estimates the cohort-specific ATTs for each survey year relative to treatment through a TWFE, compared to the never-treated cohort and the pre-treatment period. Then, it computes the weights as the cohort’s share in  $k$ , namely: the probability of being in a specific cohort for each relative time indicator. Finally, it computes a weighted average of the cohort-specific ATTs from the first step, where the weights are based on the cohort shares calculated in the second step. This weighted average provides a consistent estimator of the ATTs for each  $k$  survey round relative to treatment, under the above-discussed assumptions.

<sup>20</sup> In Appendix Table A.5, I replicate the analysis of García-Ramos (2021) using the Sun and Abraham (2021) estimator. The main results are consistent, but the magnitude of the coefficients is generally smaller, the effect on economic IPV is no longer statistically different from 0, and there is a pre-trend issue for sexual IPV. This suggests that, in the present context, it may be relevant to use a heterogeneous-robust estimator.

## 6. Main Results

The analysis aims to investigate the dynamic effects of unilateral divorce legalization on self-reported IPV in Mexico. Figure 1 - and Appendix Table A.6 - display the impact on the probability of having experienced any, physical, sexual, economic, or emotional IPV in the preceding 12 months, estimated using the Sun and Abraham (2021) estimator and controlling for women's age, urban residence, indigenous background, educational attainment, SES index, and number of children<sup>21</sup>. Robust standard errors are clustered at the state level to account for possible unobservable correlations within states and a confidence level of 90% is displayed.

**Figure 1.** Unilateral divorce legalization dynamic effects on IPV



*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on women's probability of having suffered any, physical, sexual, emotional, or economic IPV in the previous 12 months. The considered reform dates are the *de jure* ones. The outcome variables are binary variables equal to 1 if the woman has suffered at least one act of the corresponding kind of IPV during the last year. All models control for women's age, indigenous background, educational level, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women affected by the reform, and the estimator used is the one by Sun & Abrahams (2021). Robust standard errors clustered by the state of residence, confidence level at 90%. Observations: 240,703. *Source:* 2006-2021 ENDIREH.

<sup>21</sup> In the Appendix Figure B.4, I show the results for the different IPV kinds, estimated both with OLS (in squares) and Sun and Abraham (2021) (in triangles), including and not including control variables. In general, there is not a large difference between the OLS and Sun and Abraham (2021) estimates. Without any control variable, the medium-run effect on physical IPV is positive but marginally not significant, with a p-value of 0.103. However, the positive effect on physical IPV in the medium run for women who remained married is significant also without any control variable.

The results shown in Figure 1 indicate that there are no statistically significant effects of the reform on any IPV at any survey rounds following its implementation. This result contrasts with the findings of García-Ramos (2021), who observed a positive effect on any IPV 5 to 9 years after the reform, while here  $\beta_2$  is positive but not significantly different from 0. This is due to the different methodology and to the fact that, in her analysis, only the states of Mexico City and Hidalgo were treated for two survey rounds, while in this study the number of states treated for 5-9 years rises to 15, increasing the estimation's external validity.

Although there seems to be no overall impact on IPV, it is still relevant to investigate the effects on each specific type of IPV. Regarding sexual IPV, Figure 1 suggests a decrease in its prevalence in the long run. However, it is important to note that there are pre-trend issues, thus these findings should not be interpreted causally. Moreover, Figure 1 does not show any significant effect of unilateral divorce legalization on emotional and economic IPV, differently from Silverio Murillo (2019) and García-Ramos (2021)<sup>22</sup>. Finally, the legalization of no-fault divorce increased the prevalence of physical IPV in the medium term (5 to 9 years after implementation). In the preferred specification, the increase amounts to 0.7 percentage points, which corresponds to a 7.2% rise relative to the average physical IPV prevalence in treated states in 2006 (equal to 0.097), and it is statistically significant at the 5% level (see Appendix Table A.6). Interestingly, unilateral divorce legalization did not significantly impact physical IPV in the shorter time frame (up to 4 years after the reform), confirming the García-Ramos (2021) result, nor in the longer time frame (10 to 14 years after the reform). The mostly small and insignificant coefficients in the short run align with the notion that partners' behaviors may change over time. This can include an increase in conflicts and threats of divorce occurring several years after the policy implementation (García-Ramos, 2021). The new evidence that the impact of no-fault divorce legalization on physical IPV does not persist in the longer term raises various possibilities. It could suggest that divorce become more socially acceptable over time or it could indicate that women who experience abuse may be deterred from seeking divorce in the long term. Overall, the findings indicate that the relationship between unilateral divorce legalization and IPV is nuanced and may vary depending on the specific form of IPV and the time frame examined.

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<sup>22</sup> In Appendix Figure B.5, the analysis focuses specifically on the effects of no-fault divorce legalization on the group of women who were still married at the time of the survey. This group represents 85% of the total sample. Additional controls, such as the partner's age, indigenous background, educational level, and the length of the relationship, are included. The results show a positive and significant effect on physical IPV in the second survey round after the reform. The estimated effect is an increase of 0.9 percentage points, which corresponds to a 9.9% increase relative to the physical IPV prevalence in 2006, significant at the 1% level. This result confirms the earlier finding discussed and suggests that the medium-term impact of the reform on physical IPV is particularly prominent among still-married women. Additionally, for this group, there is also a positive effect on any IPV in the medium run significant at the 10% level.

## 7 Robustness Checks

The baseline analysis shows that unilateral divorce legalization increases self-reported physical IPV 5 to 9 years after its implementation, while there are no effects in the shorter or longer time frames nor on the other kinds of IPV. In this Section, I investigate the robustness of the findings through a series of checks, considering inconsistencies in the reform dates, possible confounders, alternative estimation and inference methods, and other tests.

### 7.1 Reform Implementation Dates

As mentioned, there are inconsistencies between the reform's *de jure* dates that I have retrieved in the states' Civil and Family Codes and the ones indicated in the papers by García-Ramos (2021), Hoehn-Velasco & Penglase (2021a,b), Aguirre (2019), and Silverio Murillo (2019). In particular, 7 states would be part of a different cohort according to at least one of these analyses<sup>23</sup>. Therefore, I investigate whether the results remain robust excluding those states<sup>24</sup>. The findings, presented in Figures B.6 and B.7 in the Appendix, reveal that, when excluding states with inconsistent reform dates, the effect on physical IPV in  $k = 2$  is positive but no longer significant. However, when focusing only on women who remained married, the effect on physical IPV remains consistent and significant at the 5% level (see Appendix Tables A.12 and A.13).

A more worrying issue is represented by the discrepancy between the *de jure* and the *de facto* dates. In particular, there are cases of no-fault divorces in the states that have not changed their internal Codes, or before they have made this change, as shown in Appendix Table A.8. The *de facto* dates are those indicated by Hoehn-Velasco & Silverio Murillo (2020) and Hoehn-Velasco & Penglase (2021a, b), and correspond to the year in which there were more than 10 cases of unilateral divorces in the state. The reason behind this discrepancy can be imputed to the Mexican Supreme Court resolution of July 2015 (case law 1a./ J. 28 / 2015 (10a.)) that stated the unconstitutionality of laws requiring to prove fault to file for divorce (Silverio Murillo, 2019). However, there are reasons why the main analysis focuses on *de jure* dates. Firstly, the Supreme Court resolution did not invalidate state laws or regulate other aspects of unilateral divorces, such as economic compensation (Aguirre, 2019; García-Ramos, 2021). This implies that, even if it was possible to have a unilateral divorce in every state after the resolution, it would have been more costly in states that did not change their internal Codes. It is also likely that the awareness of reforms in one's own state's Code is higher compared to the knowledge of the Supreme Court resolution (García-Ramos, 2021). Finally, if the

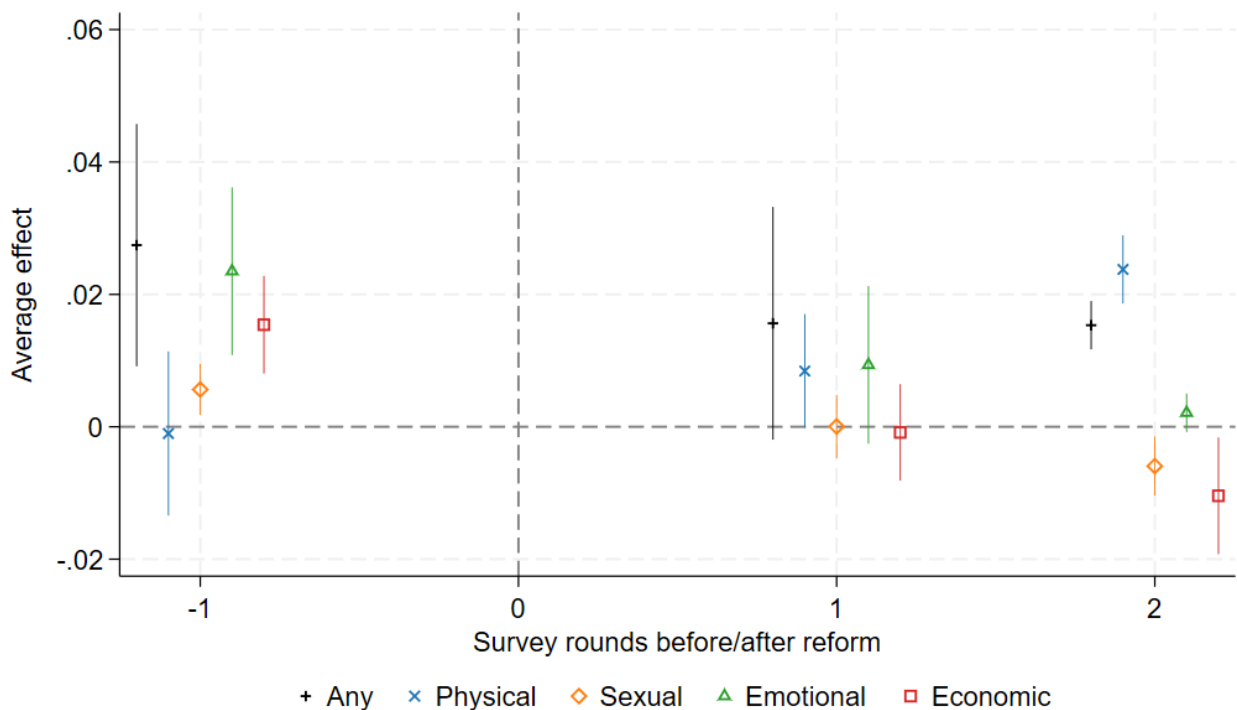
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<sup>23</sup> Table A.7 in Appendix A shows the dates recorded in each paper.

<sup>24</sup> Note that every time I exclude some states or I change the relevant reform dates, I also change the sample of women considered as affected by the unilateral divorce legalization.

control states are – partially – treated, it means that the main results may be underestimated. However, the Supreme Court resolution poses a threat to the assumption of no anticipatory behaviors<sup>25</sup>. To address this concern, a robustness check is performed using the *de facto* dates as the treatment timing. Yet, I can only investigate the effects for two survey rounds after the reform, since in this specification all the states are treated at the time of the 2021 round. Moreover, note that the variation in the treatment timing is limited (see Table A.8).

**Figure 2.** *De facto* unilateral divorce legalization dynamic effects on IPV



*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on women’s probability of having suffered any, physical, sexual, emotional, or economic IPV in the previous 12 months. The considered reform dates are the *de facto* ones. The outcome variables are binary variables equal to 1 if the woman has suffered at least one act of the corresponding kind of IPV during the last year. All models control for women’s age, indigenous background, educational level, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women affected by the reform, and the estimator used is the one by Sun & Abrahams (2021). Robust standard errors clustered by the state of residence, confidence level at 90%. Observations: 196,193. *Source:* 2006-2016 ENDIREH.

Figure 2 and Appendix Table A.9 show that, except for physical IPV, there are pre-trends for all types of IPV. Nevertheless, when using *de facto* dates, the analysis confirms the main findings regarding physical IPV: the reform increases physical IPV by 2.4 percentage points (25% increase) in the 5 to 9 years following the policy, supporting the main results and suggesting that they are

<sup>25</sup> It may also be argued that the case law poses threats to the no anticipation of the treatment (meaning the change in the internal Code), but the treatment timing may still have been difficult to anticipate.

underestimated<sup>26</sup>. Finally, in the Appendix Figure B.9, the analysis includes only the states that fall in the same cohort according to both the *de facto* and *de jure* dates. The findings confirm a positive medium-run effect on physical violence. The effect is significant also in the short-run, but it is not so if including controls for possible confounders (see next Section).

## 7.2 Possible Confounders

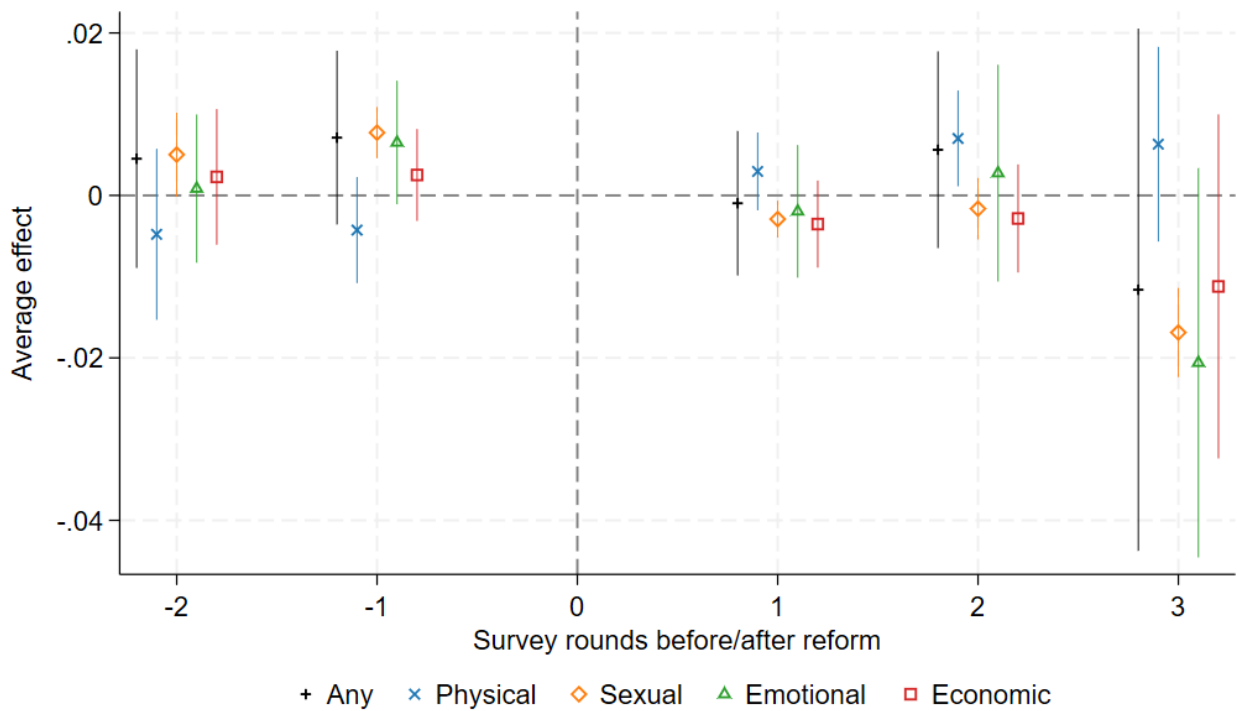
The leads in the baseline event study specification indicate that, before the implementation of unilateral divorce legalization, the IPV trends between treated and never-treated states were similar for all kinds of IPV except for the sexual one. However, there may be time-varying covariates that are correlated with IPV and could potentially confound the estimates. To address this concern, this Section follows the approach taken by García-Ramos (2021) in identifying possible confounders and includes them in the analysis.

Firstly, I control for three state-level time-varying characteristics. These variables are chosen based on their potential correlation with IPV and their relevance in the context of the reform. The first is the male unemployment rate, which serves as a proxy for the state's economic environment. Previous research has shown that gender-specific unemployment rates can impact IPV (Anderberg et al., 2016; Bhalotra et al., 2021). In this analysis, the male unemployment rate in the survey year is considered, as it is expected to be less influenced by the reform compared to the female unemployment rate. It is defined as the ratio between unemployed and economically active males by state and survey year, as an average of ratios for the year's four quarters. The second control variable is the gender gap in labor force participation, constructed as the male-to-female difference in the state and survey-year ratio for economically active people over the overall working-age population of the same sex. This variable reflects gender equality in the state and may be correlated with both IPV prevalence and the implementation of the reform. Finally, the male homicide rate (number of male homicides per 100,000 males by state and survey round) captures the states' violent environment and is particularly important considering the so-called "drug war" that has taken place in Mexico since 2006. This violent context can impact violence within households, including IPV (Almir, 2022), and affect the states' institutional setting and policies. Moreover, during the period of analysis, several other policies have been implemented in Mexican states that may have affected IPV and could be correlated with the no-fault divorce reform, potentially biasing the estimates. These policies include domestic violence-specific laws, abortion reforms, and changes in the minimum age of marriage (see Table A.10 in Appendix A for their timing).

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<sup>26</sup> See Figure B.8 in Appendix B for the same analysis on married women using the 2003-2016 survey rounds.

**Figure 3.** Unilateral divorce legalization dynamic effects on IPV: Possible confounders



*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on women’s probability of having suffered any, physical, sexual, emotional, or economic IPV in the previous 12 months. The considered reform dates are the *de jure* ones. The outcome variables are binary variables equal to 1 if the woman has suffered at least one act of the corresponding kind of IPV during the last year. All models control for women’s age, indigenous background, educational level, urban residence, SES index, number of children, and state and year fixed effects. The possible confounders included are: male homicide and unemployment rates, gender gap in labor force participation, and indicators for whether the state has introduced domestic violence as a cause of divorce in 2006-2021, has criminalized it in 2006-2021, has introduced regressive abortion laws in 2006-2021, and/or has implemented child marriage bans in 2006-2021. The sample is restricted to women affected by the reform, and the estimator used is the one by Sun & Abrahams (2021). Robust standard errors clustered by the state of residence, confidence level at 90%. Observations: 240,703. *Source:* 2006-2021 ENDIREH.

Beleche (2019) has investigated the effects of the introduction of domestic violence as grounds for divorce and the criminalization of domestic violence, finding a negative impact of the second policy on female suicides. Therefore, two dichotomous variables are constructed to account for the potential influence of these laws (see Appendix Table A.1 for the definition). Regarding abortion reforms, Mexico City decriminalized early-term elective abortion in 2007 (“ILE” reform), followed by the implementation of regressive legislation in some states defining life as beginning at conception (Clarke & Muhlrad, 2021). These reforms could have had varying effects on IPV, depending on the *bargaining power* and *backlash* channels. To control for these reforms, a dummy variable is included that takes a value of 1 for states that adopted regressive abortion laws after the 2006 survey year<sup>27</sup>.

<sup>27</sup> Additionally, the analysis investigates whether the main results remain consistent when excluding Mexico City, which implemented the more progressive “ILE” reform and is generally considered the most gender-equal state. The results are



Furthermore, starting in 2014, most Mexican states set the minimum age of marriage at 18. Bellés-Obrero and Lombardi (2023) have shown that, while this led to a decrease in formal child marriages, there was a corresponding increase in informal unions. To address the potential bias arising from this shift, a binary staggered variable is included for states adopting this policy. By conditioning on this large set of individual and state-level time-varying characteristics, as well as on state and year fixed effects, the aim is to ensure the exogeneity of the unilateral divorce legalization timing to changes in IPV. The results in Figure 3 show that the main findings of the analysis remain consistent. Indeed, although the standard errors of the estimates are larger, the positive and significant effect on physical IPV in the second survey round after the reform implementation is still observed. Furthermore, the graph confirms the no-parallel trends in sexual IPV between treated and control states before the policy.

### 7.3 Alternative Estimation and Inference Methods

As mentioned in Section 5.2, in recent years there has been a surge of new TWFE estimators robust to heterogeneous effects<sup>28</sup>. In the main analysis, I use the Sun and Abraham (2021) estimator since it is the most readily comparable to the classic Ordinary Least Squares (OLS) event study. However, in this sensitivity test, the alternative estimators proposed by Callaway & Sant'Anna (2021) and Borusyak et al. (2021) are used to assess the consistency of the results. The Callaway & Sant'Anna (2021) estimator is similar to the Sun & Abraham (2021) one in estimating group-time ATTs, then aggregated to form policy-relevant parameters. The recommended estimand is based on a “doubly-robust” (inverse probability weighting and outcome regression) method (Callaway & Sant'Anna, 2021). Additionally, this estimator explicitly allows the parallel trends assumption to be conditional on covariates. Borusyak et al. (2021) propose instead an imputation-based estimator: it predicts the counterfactual outcomes by estimating a TWFE model using non-treated observations and then aggregates the resulting individual treatment effects. It may be more precise than the others, but it also requires the parallel trends hypothesis to hold for all cohorts and periods (Roth et al., 2022). Figure B.11 in the Appendix presents the effects of unilateral divorce legalization on different types of IPV using the OLS, Callaway & Sant'Anna (2021), Borusyak et al. (2021), and Sun and Abraham (2021) estimators. The results obtained show a similar pattern across all specifications, particularly for the post-treatment periods. When focusing specifically on physical IPV, the effect in the medium

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displayed in Appendix Figure B.10 and confirm the positive medium-run impact on physical IPV. The graph shows also positive and significant effects on any and economic IPV in  $k = 3$ . However, it is important to exercise caution when interpreting these results since, excluding Mexico City, there is only one state, Hidalgo, that is treated for three survey rounds. Moreover, this confirms that Mexico City is more gender-equal compared to other states.

<sup>28</sup> See de Chaisemartin & D'Haultfœuille (2022) and Roth et al. (2022) for a detailed discussion on the properties of each estimator.

run remains positive across all estimators and is statistically significant in most cases, except for the Callaway & Sant'Anna (2021) one. However, the positive effect on physical IPV in  $k = 2$  is significant at the 10% level also with the Callaway & Sant'Anna (2021) estimator if focusing only on women who remained married (see Appendix Table A.13), which represents 85% of the sample and is identified as the group driving the results (refer to Section 8).

In the analysis, heteroskedasticity-robust standard errors clustered by the state of residence are used to account for the potential correlation of errors within states. However, since Mexico has a relatively small number of states (32), which is below the rule-of-thumb threshold of 50 clusters, the robustness of the results is further tested using wild-cluster bootstrapping as suggested by Cameron et al. (2008) to address the issue of a small number of clusters. Table A.11 in the Appendix presents the p-values computed with wild-cluster bootstrapping. While this procedure could not be implemented with the Sun and Abraham (2021) estimator, the results of this test are provided for the OLS event study specification. The table confirms the findings of the main analysis, indicating that the results are robust to the choice of inference procedure.

#### **7.4 Other Tests**

Another check that it is worthwhile conducting is to include the 2003 survey round. Indeed, it provides the second pre-reform round for Mexico City and Hidalgo, crucial to testing the parallel trends assumption. However, it should be noted that the 2003 survey was representative of only 11 states and interviewed only women residing with a partner. Therefore, when considering this round, the sample is restricted to married women (García-Ramos, 2021). Appendix Figure B.12 shows that, except for sexual IPV, there are no significant pre-trends. In the second survey round after the reform implementation ( $k = 2$ ), the effect on physical IPV is positive and significant at the 1% level, and the effect on any IPV is positive and significant at the 10% level.

Some states have implemented the policy in the year preceding the ENDIREH survey rounds (see Table 1). This may represent an issue since the questions about IPV refer to the previous 12 months. Therefore, women residing in these states may have suffered from IPV before the legalization of unilateral divorce. To address this concern, the analysis is rerun excluding these states. The results are shown in Appendix Figure B.13. The medium-run effect on physical IPV is positive but slightly not significant, although it is statistically different from zero at the 10% level when focusing on still-married women (see Appendix Table A.12 and A.13). The negative effects in the long run are entirely driven by Mexico City, as Hidalgo is among the excluded states.

Furthermore, to examine whether the medium-run impact on physical IPV is determined by any specific state, the analysis is conducted by excluding one state at a time. The results, shown in Figure B.14 in the Appendix, demonstrate that the positive and significant effect of unilateral divorce legalization on physical IPV 5 to 9 years after implementation is not due to any individual state<sup>29</sup>.

Finally, sample weights are not used in the analysis. The reasons behind this decision and a detailed comparison of the results with and without sample weights are discussed in Appendix C. The effect on physical IPV in  $k = 2$  is positive, but no longer significant, when using sample weights. This may be attributed to a loss of estimation precision with sample weights and/or to the presence of unmodeled heterogeneous effects. However, also in this last case, the use of sample weights would not solve the issue (Solon et al., 2015). Considering the questionable appropriateness of using sample weights in this context and the consistent direction of the effects, it is argued that the main results remain robust.

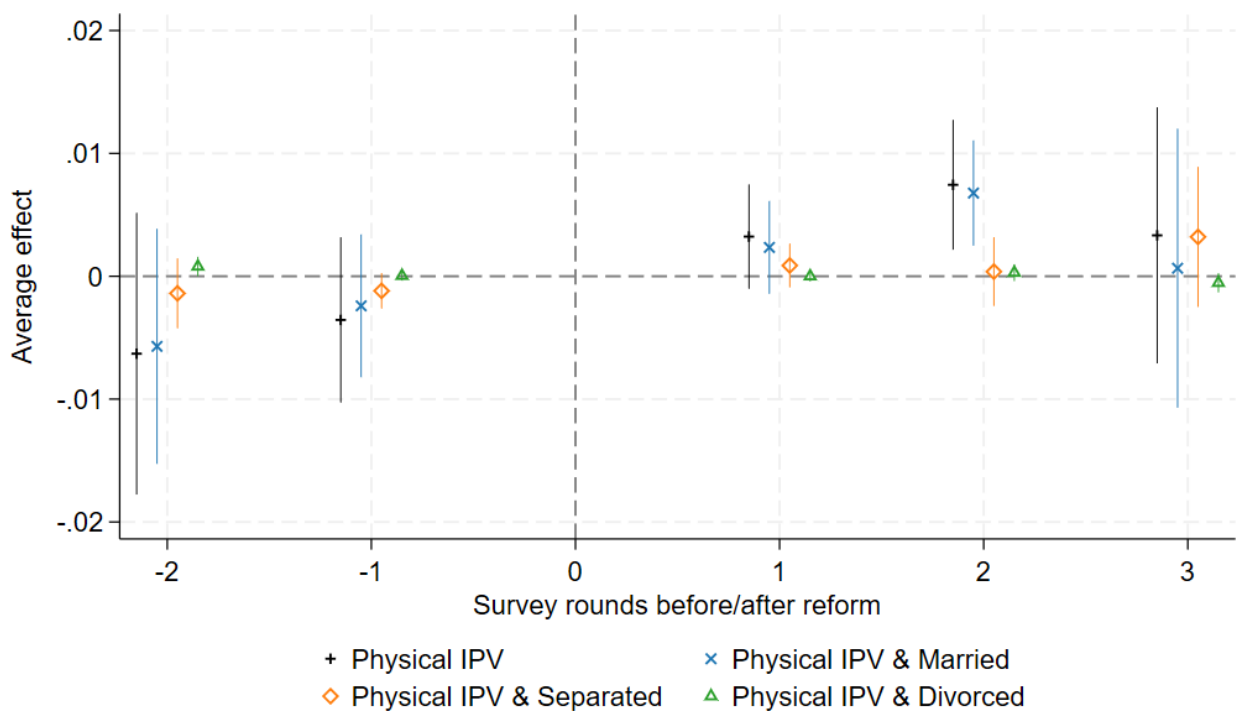
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<sup>29</sup> The positive medium-run effect on physical IPV is confirmed also excluding the first cohort of treated states - Mexico City and Hidalgo -, but it is statistically different from 0 at the 10% confidence level only if focusing on women that remained married. Results upon requests.

## 8 Mechanisms

The findings suggest that unilateral divorce legalization led to an increase in self-reported physical IPV in the medium term. However, there were no significant effects observed in the shorter or longer time frames or on other forms of violence. In this Section, I explore the underlying mechanisms. The analysis indicates that the positive effect on physical IPV is more pronounced and statistically significant when focusing on women who remain married after the implementation of unilateral divorce legalization (see Figure B.5 and Table A.13). This suggests that the mechanism behind the increase in physical IPV is related to changes within intact couples rather than an increase in divorces from abusive relationships. Figure 4 and Table A.14 further examine the impact of the reform on the joint probability of experiencing physical IPV and the marital status of women. The medium-run effect is positive and significant for the joint probability of having suffered physical IPV in the previous 12 months and being married. Moreover, the coefficient is very similar to the effect on physical IPV, proving that the channel is in changes in marriages that remain intact after the policy.

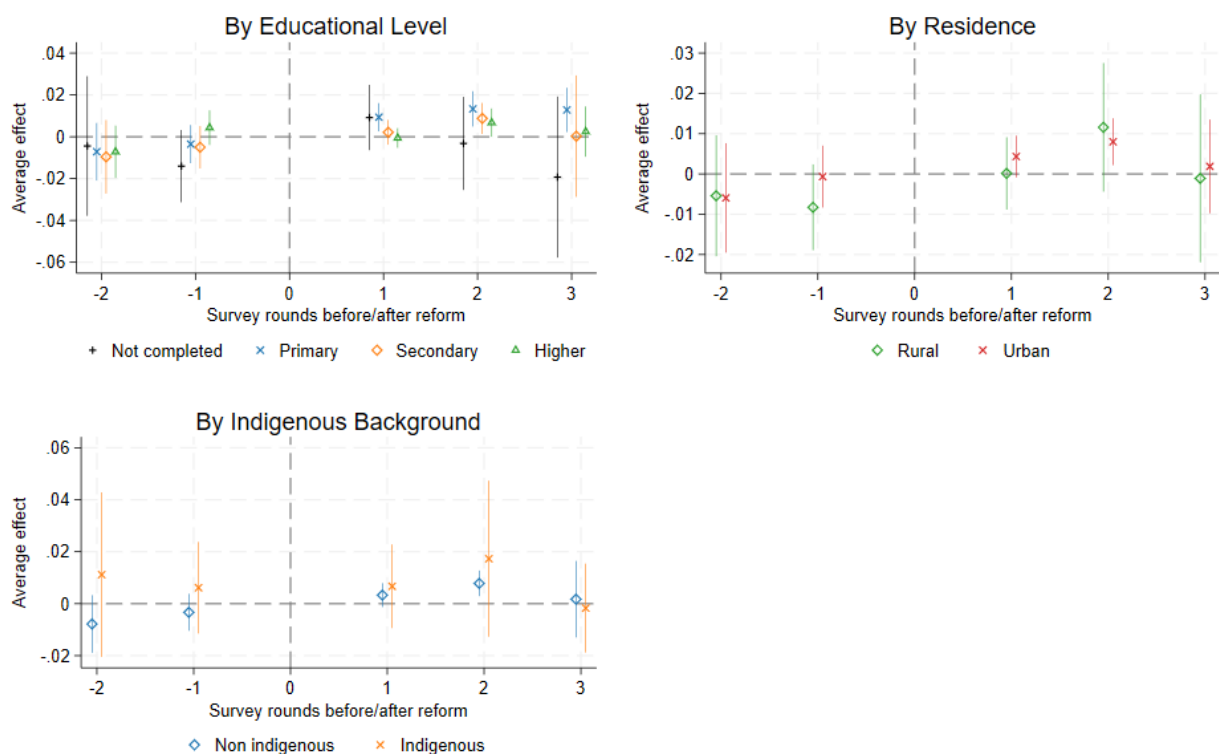
**Figure 4.** UD legalization effects on joint probability physical IPV and marital status



*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on women's joint probability of having suffered physical IPV in the previous 12 months and being married, separated, or divorced. The considered reform dates are the *de jure* ones. All models control for women's age, indigenous background, educational level, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women affected by the reform, and the estimator used is the one by Sun & Abrahams (2021). Robust standard errors clustered by the state of residence, confidence level at 90%. Observations: 240,703. *Source:* 2006-2021 ENDIREH.

Theoretically, two possible mechanisms may explain the increase in physical IPV within intact marriages after the implementation of unilateral divorce legalization (García-Ramos, 2021). The first is that violence is used as an *instrument* to prevent divorce, particularly affecting women with more outside options. To test this hypothesis, I examine the effects of the reform on physical IPV separately for different variables related to married women's outside options. Firstly, I consider the woman's educational attainment, in particular, whether she has not completed primary school, has completed it, has finished secondary school, or has higher education. Indeed, schooling attainment is commonly used as a proxy for outside options (Brassiolo, 2016; García-Ramos, 2021). Moreover, I take into account women's urban/rural residence and indigenous background.

**Figure 5.** Heterogenous effects on physical IPV, married women



*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on married women's probability of having suffered physical IPV in the previous 12 months by educational level, residence, and indigenous background. In the Figure "By Educational Level", the analysis is conducted separately for women who did not finish primary education (14,317 obs., 7% of the sample), who completed primary education (65,445 obs., 32%), who completed secondary education (87,032 obs., 42%), and who have educational attainment higher than secondary school (38,201 obs., 19%). In the Figure "By Residence", the analysis is conducted separately for women living in a rural (45,436 obs., 22%) or urban (159,559 obs., 78%) area. In the Figure "By Indigenous Background", the analysis is conducted separately for women without (190,968 obs., 93%) or with (14,027 obs., 7%) an indigenous background. Robust standard errors clustered by the state of residence. The sample is restricted to women married at the time of the survey. All specifications control for women's and partners' age, indigenous background, and educational level, relationship length, urban residence, SES index, number of children, and state and year fixed effects. Observations (total): 204,995. *Source:* 2006-2021 ENDIREH.

Indeed, urban residence may also be related to having more opportunities outside of the household and may imply greater geographical proximity to courts<sup>30</sup>. Finally, Mexicans with an indigenous background continue to face unequal economic and social conditions, potentially affecting their possibility of benefitting from the reform (World Bank, 2011)<sup>31</sup>. The results, as shown in Figure 5 and in Appendix Table A.15, support the *instrumental violence* supposition: the positive effect on physical IPV in the medium run is significant only for women with primary or secondary education, living in urban areas, or without an indigenous background<sup>32</sup>. The effect for women with educational attainment higher than secondary is also positive but slightly non-significant in the medium-run, while the effect for women that have completed primary school is significant also in the short term. This suggests that having very high outside options may be protective, while the *instrumental use* of violence may be stronger against women with a “medium” level of outside options (e.g., with primary or secondary school).

The second hypothesis is that the increase in physical IPV for still-married women is driven by *male backlash* in response to changes in variables related to gender norms or men's preferences. To explore this, the analysis investigates the effects of unilateral divorce legalization on women's employment, attitudes toward divorce due to IPV, and decision-making power. In particular, Appendix Figure B.15 and Table A.16 present the impact on the probability of being employed, the likelihood of thinking about divorce or separation due to IPV<sup>33</sup>, and the number of decisions that the woman takes alone and/or jointly with the partner (see Appendix Table A.1 for the precise definition). Differently from García-Ramos (2021), the analysis shows that the policy increased in the medium-run the probability that married women worked during the previous week by 6% relative to the 2006 mean in treated states (significant at the 10% level), while it did not affect IPV attitudes. Additionally, I explore if the reform affected another aspect of female empowerment that could potentially trigger *backlash*: women's decision-making power within the relationship. In particular, I investigate the effect on the number of decisions that the married woman takes by herself and that she takes either alone or jointly with the partner. Appendix Figure B.15 shows that the legalization of unilateral divorce did not affect the joint decision-making process of the couple, similarly to Silverio Murillo (2019), but that it significantly raised the decision-making power of the woman alone in all the time frames considered.

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<sup>30</sup> However, the reform may have affected women's educational level and residence, so these analyses are explorative.

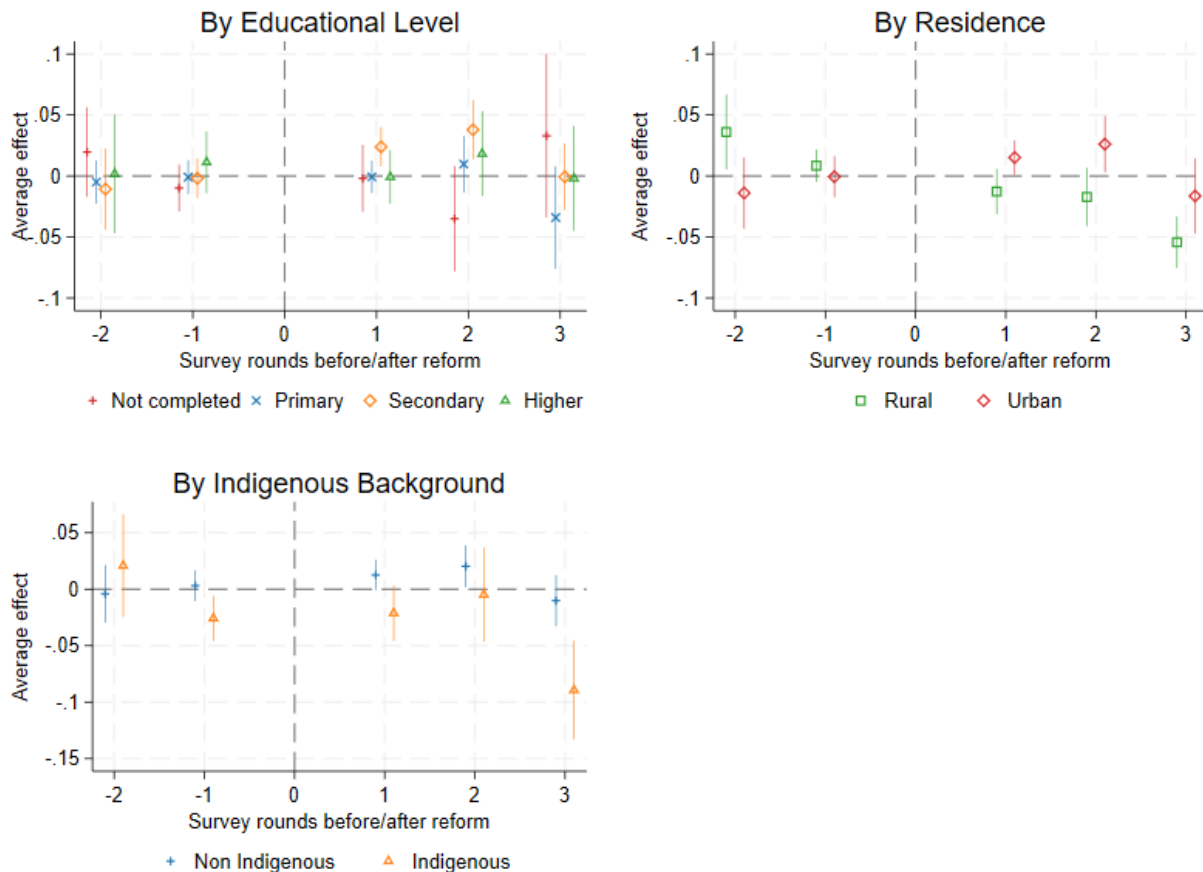
<sup>31</sup> It would have been interesting to conduct heterogeneous analyses also based on women's age and relationship length, but it is not feasible since these variables are used to define the sample of women exposed to the policy. Finally, it is not possible to investigate whether the effect changes if the woman has or not children under the age of 18 because some of the ENDIREH rounds have information about the age of the household's components but not about who are the children of the interviewed women.

<sup>32</sup> Note, however, that the results of the heterogeneity analyses are generally not statistically different from each other.

<sup>33</sup> This variable is defined only for married women that have suffered at least one act of IPV during the current relationship. Since the policy may affect this variable, the analysis of the possible effects on this outcome is only explorative.

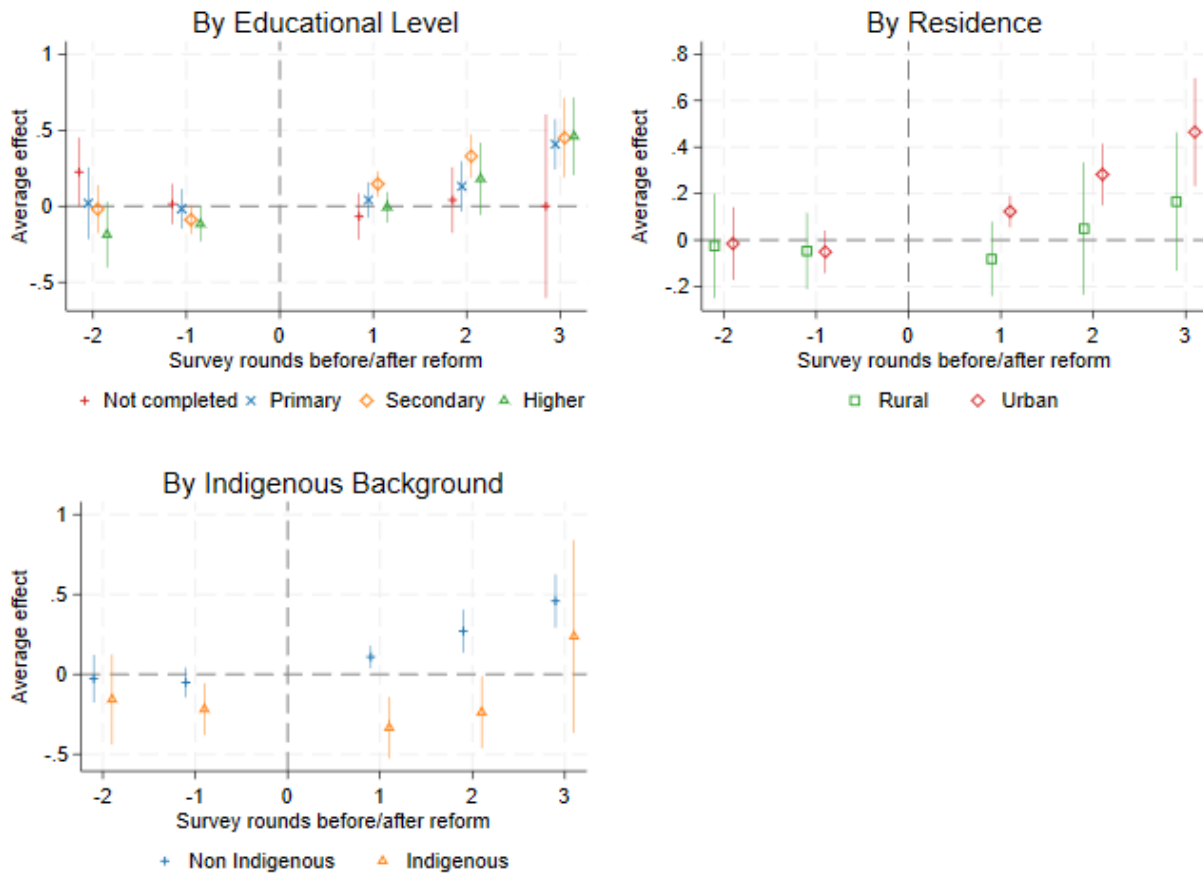
Therefore, the analysis suggests that the surge in physical IPV may also be explained by males' violent reactions to improvements in female employment and decision-making independence following the policy implementation. It is worth noting, however, that the observed increases in labor market outcomes and decision-making power may also be driven by married women with more outside options, implying that the *instrumental* and *backlash* channels may be at play simultaneously.

**Figure 6.** Heterogenous effects on employment, married women



*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on married women's probability of having worked in the previous week by educational level, residence, and indigenous background. Robust standard errors clustered by the state of residence. The outcome is a dichotomous variable equal to 1 if the woman was employed or worked in the previous week. In the Figure "By Educational Level", the analysis is conducted separately for women who did not finish primary education (14,317 obs., 7% of the sample), who completed primary education (65,445 obs., 32%), who completed secondary education (87,032 obs., 42%), and who have educational attainment higher than secondary school (38,201 obs., 19%). In the Figure "By Residence", the analysis is conducted separately for women living in a rural (45,436 obs., 22%) or urban (159,559 obs., 78%) area. In the Figure "By Indigenous Background", the analysis is conducted separately for women without (190,968 obs., 93%) or with (14,027 obs., 7%) an indigenous background. The sample is restricted to women married at the time of the survey. All specifications control for women's and partners' age, indigenous background, and educational level, relationship length, urban residence, SES index, number of children, and state and year fixed effects. Observations (total): 204,995. *Source:* 2006-2021 ENDIREH.

**Figure 7.** Heterogenous effects on decision-making power (alone), married women



*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on married women’s alone decision-making power by educational level, residence, and indigenous background. Robust standard errors clustered by the state of residence. The outcome is the number of decisions that the woman takes alone, ranging from 0 to 11. In the Figure “By Educational Level”, the analysis is conducted separately for women who did not finish primary education (14,317 obs., 7% of the sample), who completed primary education (65,445 obs., 32%), who completed secondary education (87,032 obs., 42%), and who have educational attainment higher than secondary school (38,201 obs., 19%). In the Figure “By Residence”, the analysis is conducted separately for women living in a rural (45,436 obs., 22%) or urban (159,559 obs., 78%) area. In the Figure “By Indigenous Background”, the analysis is conducted separately for women without (190,968 obs., 93%) or with (14,027 obs., 7%) an indigenous background. Robust standard errors clustered by the state of residence. The sample is restricted to women married at the time of the survey. All specifications control for women’s and partners’ age, indigenous background, and educational level, relationship length, urban residence, SES index, number of children, and state and year fixed effects. Observations (total): 204,995. *Source:* 2006-2021 ENDIREH.

Figures 6 and 7 (and Appendix Tables A.17 and A.20) demonstrate that the increases in employment and decision-making independence are indeed significant for married women with higher educational attainment (especially for those who have completed secondary school), living in urban areas, or without an indigenous background.<sup>34</sup>

<sup>34</sup> Appendix Graphs B.16 and B.17 show the heterogenous effects on IPV attitudes and on the number of decisions taken alone or jointly. The probability of thinking about divorce because of IPV slightly increases in the long run for women



## 9. Discussion and Conclusion

The paper studies the dynamic effects of unilateral divorce legalization on intimate partner violence in Mexico. It exploits the staggered implementation of the reform and uses data from a repeated cross-sectional survey, adopting a heterogeneous-robust event study estimation strategy. The analysis provides evidence that the policy increased the prevalence of self-reported physical IPV by 7.2% (0.7 ppt), 5 to 9 years after its implementation, while no significant effects were observed on the other kinds of violence nor in the short term, suggesting that the partners' behavior takes time to respond to the policy. Moreover, the effect does not persist in the longer run (10-14 years later), indicating that divorce or the other changes in women's empowerment due to the policy become more acceptable over time, or reflecting a decrease in divorce attempts among women experiencing abuse after several years. The findings are robust to a battery of checks and are driven by women who remained married.

Concerning the mechanisms, the analysis suggests that the increase in physical IPV against married women is driven by both *instrumental use* of violence and *male backlash*, implying that violence may be used at the same time as a means to prevent divorce against married women with more outside options and as a response to changes in women's empowerment dimensions considered as challenging gender norms. In particular, the positive effect on physical IPV in the medium run is significant only for married women living in urban areas, without an indigenous background, or with primary or secondary education, while having attended higher levels of education seems to be protective. At the same time, the increase in physical IPV may also be explained by males' violent reactions to improvements in female employment and decision-making independence following the unilateral divorce legalization. Interestingly, the observed increases in labor market outcomes and decision-making power are also driven by married women with more outside options, implying that the *instrumental* and *backlash* channels may be at play simultaneously. Specifically, the similarity in patterns between the dynamic heterogeneous effects on physical IPV and on the probabilities of being employed suggests that labor market changes may be a significant trigger for *backlash* against women with secondary education, living in an urban area, and without an indigenous background. The increase in IPV against these groups of women may thus be ascribed to a combination of *instrumental violence* and *backlash*. Otherwise, the predominantly non-significant effects on the labor market and decision-making outcomes for women who have completed primary education indicate that the increase in physical IPV for this group may be primarily attributed to *instrumental violence*.

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with secondary education or living in rural areas. The analysis of this outcome and in the long period should be interpreted as explorative, but it may suggest a change in attitudes for these groups. Graph B.17 indicates possible negative long-run impacts on decision-making power for women who have not completed primary school, a group with low outside options.

The analysis contributes to the literature investigating the effect of increased divorce accessibility on IPV, confirming the drawbacks of the Mexican unilateral divorce legalization (García-Ramos, 2021; Silverio Murillo, 2019), in contrast with what has been observed in other contexts (Brassiolo, 2016; Corradini & Buccione, 2023; Stevenson & Wolfers, 2006). In particular, this study builds upon García-Ramos (2021) research by extending the time frame, enhancing the external validity of short and medium-term impacts, addressing reform date inconsistencies, employing a heterogeneous-robust empirical strategy, and further investigating potential mechanisms. While consistent with García-Ramos (2021) in finding no short-term effects and subsequent medium-term increases in IPV, this analysis specifically identifies the impact on physical IPV without affecting other IPV forms, and it additionally suggests that the effect does not persist in the long run. Moreover, the study aligns with the author's results in identifying instrumental violence against women with a "medium" level of education as a mechanism, while also exploring this channel in relation to women's indigenous background and residence. Unlike García-Ramos (2021), this study reveals increased labor market outcomes for married women post-reform and incorporates measures of decision-making power as potential *male backlash* drivers. Ultimately, the analysis highlights the interaction between *instrumental violence* and *backlash* mechanisms, as the medium-term rise in physical IPV seems to be driven by married women with greater outside options—those who also benefited significantly from the policy in terms of improved labor market outcomes and decision-making power within the relationship.

The paper acknowledges certain limitations, opening avenues for future research. Firstly, despite employing an estimator robust to heterogeneous effects across different cohorts of treated states, it assumes homogeneity in the impact across states within the same cohort. Additionally, the long-term estimates are based on data from only two states, Mexico City and Hidalgo, that implemented the policy for more than two rounds by the time of the last ENDIREH survey, potentially limiting the external validity of the long-run findings. Therefore, future research could aim to refine the understanding of the dynamics and heterogeneities of the impacts. Moreover, there is a need for further exploration of the specificities of each type of IPV, as the policy affected only physical violence. Finally, as mentioned, the effect observed in Mexico contrasts with those documented in the United States (Stevenson & Wolfers, 2006), Spain (Brassiolo, 2016), and Egypt (Corradini & Buccione, 2023). Therefore, the characteristics of the Mexican context, leading to an increase in IPV after the legalization of unilateral divorce, should be further studied. The research underscores the importance of analyzing the distinct dynamics of each country when investigating the relationship between divorce access and IPV, as the results may not be generalizable.

In summary, the analysis underscores the significance of considering the dynamic effects of public policies and programs, as their impacts may evolve over time. Furthermore, it highlights the intricate interplay between *bargaining power*, *instrumental violence*, and *male backlash* for different groups of women following the legalization of unilateral divorce. Doing so, the research stresses the importance of enhancing institutional support for women seeking separation or divorce, possibly facing a heightened risk of violence, and emphasizes the need to address gender norms in combating violence against women. Specifically, it advocates for policies that not only bolster women's outside options, such as improving divorce accessibility, but also address cultural resistance to divorce and female empowerment at the community and societal levels through educational initiatives and awareness campaigns. Such a multifaceted approach is deemed essential for mitigating potential unintended consequences of these policies, including increases in *instrumental violence* and *backlash*.

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## Appendix A – Additional Tables

**Table A.1.** Variables' definitions

<b>Variable</b>	<b>Definition</b>
<i>Sample selection variables</i>	
Married	Equal to 1 if the woman is married, 0 if in union, divorced, separated, widow, or single
Divorced	Equal to 1 if the woman is divorced, 0 if married, in union, separated, widow, or single
Separated	Equal to 1 if the woman is separated, 0 if married, in union, divorced, widow, or single
Age	Woman's age
Age at marriage	Age at marriage or cohabitation
Relationship length	Age - Age at marriage
Divorce length	Years since divorce from ex husband
State exposure to policy	Survey round - Reform implementation date
Treated	Equal to 1 if the state has legalized unilateral divorce before October 2021, 0 otherwise
Never-treated	Equal to 1 if the state has not legalized unilateral divorce before October 2021, 0 if it has
Cohort 0-3	Equal to 0 if the state is never-treated, to 1 if it has legalized divorce after October 2016, to 2 if after October 2011, to 3 if after October 2006
Cohort 1-3	Equal to 1 if the state has legalized divorce after October 2016, to 2 if after October 2011, to 3 if after October 2006
<i>Outcomes</i>	
Physical IPV	Equal to 1 if, from October of the previous year to the survey date, it happened at least one time that the (ex) partner/husband has done one of the following: pushed her or pulled her hair; tied her up; kicked her; thrown any object at her; beaten her with his hands or an object; tried to hang or choke her; assaulted her with a knife or blade; red a weapon at her; threatened her with a weapon; threatened to kill her, himself or the children
Sexual IPV	Equal to 1 if, from October of the previous year to the survey date, it happened at least one time that the (ex) partner/husband has done one of the following: demanded her to have sexual intercourse; forced her to do sexual things that she does not like; used physical strength to force her to have sexual intercourse



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Emotional IPV	Equal to 1 if, from October of the previous year to the survey date, it happened at least one time that the (ex) partner/husband has done two of the following: ashamed, underestimated or humiliated her; ignored or not shown her affection; said she cheat on him; made her feel fear; threatened to leave her, hurt her, take her children away or kick her out; locked her in, forbidden her from going out or being visited; turned her children or relatives against you; destroyed, thrown away or hidden things belonging to her or the household; got angry because household chores are not done like he wants
Economic IPV	Equal to 1 if, from October of the previous year to the survey date, it happened at least one time that the (ex) partner/husband has done two of the following: complained about how she spends money; been stingy with the household expenses even if he has money; not given her the upkeep or threatened her to not giving it; spent money needed for the household; appropriated or taken money or possessions from her; forbidden her to work or study
Any IPV	Equal to 1 if at least one of the physical, sexual, emotional, or economic IPV above-defined variables is equal to 1
<i>Individual &amp; household controls</i>	
Age	Woman's age
Residence	Equal to 1 if the woman lives in an urban area, 0 if rural
Indigenous background	Equal to 1 if the woman speaks any indigenous language, 0 if not
Educational level	Equal to 0 if the woman has not completed primary school, to 1 if she has completed it, to 2 if she has completed secondary school or equivalent training, to 3 if she has undertaken post-secondary education studies
SES index	Household socio-economic status index, computed using information on the household's infrastructure, assets, and number of residents, weighted through the Principal Component Analysis (see Appendix A of García-Ramos (2021) for a detailed discussion)
Number of children	Number of children born alive
<i>Partner's controls</i>	
Partner's age	Partner's age, for married women
Partner's indigenous background	Equal to 1 if the partner speaks any indigenous language, 0 if not, for married women
Partner's educational level	Equal to 0 if the partner has not completed primary school, to 1 if he has completed it, to 2 if he has completed secondary school or equivalent training, to 3 if he has undertaken post-secondary education studies, for married women
Relationship length	Age - Age at marriage, for married women
<i>Confounders</i>	
Male unemployment rate	Average of four trimesters ratios of unemployed males on economically active males, by state and survey year

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Gender gap in LFP	Male-to-female difference of the ratios for gender-specific economically active people over the overall working-age population of the same sex, by state and survey year
Male homicide rate	Number of male homicides per 100,000 males, by state and survey year
DV as cause for divorce	Equal to 1 for states and survey years in which domestic violence was introduced as a possible cause for divorce, to 0 if the state had implemented the policy before 2006 or did not implement it and for the survey rounds before its implementation
DV in Penal Code	Equal to 1 for states and survey years in which domestic violence was criminalized, to 0 if the state had implemented the policy before 2006 or did not implement it and for the survey rounds before its implementation
Child marriage ban	Equal to 1 for states and survey years that introduced child marriage bans, to 0 if not introduced
Regressive abortion law	Equal to 1 for states and survey years that adopted regressive abortion laws after the 2006 survey year, to 0 if not adopted
 <i>Mechanisms</i>	
Employment	Equal to 1 if the previous week worked or had a job but did not work, 0 if not
IPV attitudes	Equal to 1 if the woman thinks about separation or relationship ending because of the problems and conflict with the husband or partner, 0 if do not think or wants so, cannot separate, or already separated but came back with him; among married women who suffered at least 1 act of IPV
Decision-making power (alone)	Number of decisions that the woman takes alone among: if she can work or study; if she can go out; what to do with the money she earns or she has; if she can buy a house for herself; if she can participate in the social or political life of the community; how to use or economize money; about permissions to children; moving house or city; when to have sexual intercourses; whether to use contraceptives; who has to use the contraceptives
Decision-making power (alone/jointly)	Number of decisions that the woman takes either alone or jointly with the partner among: if she can work or study; if she can go out; what to do with the money she earns or she has; if she can buy a house for herself; if she can participate in the social or political life of the community; how to use or economize money; about permissions to children; moving house or city; when to have sexual intercourses; whether to use contraceptives; who has to use the contraceptives

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*Notes:* own elaboration. *Source:* 2006-2021 ENDIREH.

**Table A.2.** Literature review of Mexican reform on women's empowerment

Author(s)	Journal	Data	Dates	Method	Findings
Hoehn-Velasco & Silverio Murillo (2020)	Econ Letters	Homicide & suicide statistics (INEGI), 2005-2017	<i>De facto</i> (until 2017)	Event study	= female suicide & homicide rates
Hoehn-Velasco & Penglase (2021b)	J Econ Behav & Organ	ENOE, 2007-2019	<i>De facto</i> (until 2017)	TWFE	= married women labor supply, ↑ hours worked for employed married women
Silverio Murillo (2019)	Unpublished Manuscript	ENDIREH, 2006-2011-2016	Supreme Court resolution 2015	Diff-in-Diff vs cohabiting women	↑IPV (any, emotional & economic) = women's decision-making ↑ labor supply for married women with young children, ↓ without young children
García-Ramos (2021)	J Dev Econ	ENDIREH, (2003)-2006-2011-2016	<i>De jure</i> (approval) (until 2016)	Dynamic TWFE	↑ any, physical, emotional, & economic IPV (5-10 years later, for still-married women)

Notes: own elaboration.

**Table A.3.** Effect on marital status: *De jure* & *De facto* dates, all women interviewed

	(1) Married <i>De Jure</i>	(2) Married <i>De Facto</i>	(3) Separated <i>De Jure</i>	(4) Separated <i>De Facto</i>	(5) Divorced <i>De Jure</i>	(6) Divorced <i>De Facto</i>
<i>Leads</i>						
UD -2	0.008 (0.014)		0.002 (0.003)		0.000 (0.002)	
UD -1	0.000 (0.006)	0.005 (0.007)	0.003 (0.002)	-0.000 (0.005)	0.001 (0.001)	-0.001 (0.002)
<i>Lags</i>						
UD 1	-0.008* (0.004)	-0.013 (0.021)	0.006** (0.002)	0.003 (0.004)	0.002* (0.001)	0.004*** (0.001)
UD 2	-0.018** (0.007)	-0.036 (0.028)	0.007 (0.004)	0.002 (0.006)	0.004 (0.003)	0.006 (0.004)
UD 3	-0.022** (0.008)		0.010*** (0.003)		-0.002 (0.003)	
Mean	0.500	0.496	0.046	0.047	0.013	0.013
Observations	507,092	396,965	507,092	396,965	507,092	396,965
Survey rounds	2006-2021	2006-2016	2006-2021	2006-2016	2006-2021	2006-2016

*Notes:* The Table shows the unilateral divorce legalization dynamic effects on women's marital status at the time of the survey, considering both the *de jure* and the *de facto* dates of the reform. The outcome variables are indicators for being married, divorced, or separated. UD k represent the leads (k=-1,2) and lags (k=1,2,3) of the event study, namely the number of survey rounds k before or after the legalization of unilateral divorce. The sample includes all women interviewed by the 2006-2021 ENDIREH rounds for the *de jure* dates, or by the 2006-2016 ENDIREH rounds for the *de facto* dates. «Mean» is the average of the outcome in treated states in 2006. The estimator used is the Sun & Abrahams (2021), and robust standard errors are clustered by the state of residence. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table A.4.** Balance table: Treated VS never-treated

Variable	(1) Treated Mean/(SE)	(2) Never-treated Mean/(SE)	(1)-(2) Pairwise t-test Mean difference
<i>Individual characteristics</i>			
Age	38.548 (0.349)	38.162 (0.428)	0.385
Urban residence	0.803 (0.036)	0.750 (0.056)	0.053
Indigenous	0.064 (0.018)	0.055 (0.035)	0.009
Education	1.670 (0.048)	1.547 (0.109)	0.123
Number of children	2.730 (0.086)	2.912 (0.135)	-0.181
SES index	1.743 (0.065)	1.684 (0.111)	0.059
Married	0.507 (0.014)	0.506 (0.030)	0.001
Divorced	0.009 (0.001)	0.012 (0.003)	-0.003
<i>States' characteristics</i>			
Male unemployment rate	3.565 (0.314)	2.692 (0.368)	0.873*
Gender gap in LFP	37.659 (1.188)	42.288 (3.441)	-4.629
Male homicide rate	16.975 (2.338)	19.126 (4.572)	-2.151
DV as cause for divorce	0.627 (0.147)	0.614 (0.223)	0.013
DV in Penal Code	0.968 (0.025)	0.964 (0.040)	0.003
Number of observations	97088	27068	124156
Number of clusters	25	7	32

*Notes:* The Table shows the difference in means for the treated VS never-treated states in 2006 (pre-treatment period), using sample weights. The sample consists of all women interviewed, dropping observations with missing values in any variable considered. The errors are clustered at the state level. Significance levels: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

**Table A.5.** Replication of García-Ramos (2021) with Sun and Abraham (2021) estimator

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Any		Physical		Sexual		Emotional		Economic	
	García-Ramos	Event study - SA	García-Ramos	Event Study - SA	García-Ramos	Event Study - SA	García-Ramos	Event Study - SA	García-Ramos	Event Study - SA
UD -2		0.009 (0.008)		-0.000 (0.004)		0.010*** (0.003)		0.009 (0.006)		0.005 (0.005)
UD 1	0.001 (0.007)	0.006 (0.007)	0.003 (0.003)	0.005 (0.003)	-0.006* (0.002)	-0.002 (0.002)	-0.002 (0.006)	0.003 (0.006)	-0.003 (0.004)	-0.001 (0.004)
UD 2	0.037*** (0.006)	0.022*** (0.004)	0.018*** (0.004)	0.017*** (0.004)	0.001 (0.002)	-0.006** (0.002)	0.025*** (0.006)	0.011*** (0.003)	0.012** (0.004)	0.001 (0.003)
Obs	191,639	195,678	191,639	195,678	191,639	195,678	191,639	195,678	191,639	195,678

*Notes:* The Table shows the replication of García-Ramos (2021) results using the Sun and Abraham (2021) estimator. In the “García-Ramos” columns, I report the results of Panel B of Table 4 in García-Ramos (2021). In the “Event study -SA” columns, I report the findings using the Sun and Abraham (2021) event study estimator. The specifications control for women's age, indigenous background and educational attainment, urban residence, SES index, number of children, (and presence of children in García-Ramos), and state and year fixed effects. Robust standard errors clustered by the state of residence in parentheses. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. *Source:* 2006-2016 ENDIREH.

**Table A.6.** Unilateral divorce legalization dynamic effects on IPV: *De jure* dates

	(1)	(2)	(3)	(4)	(5)
	Any	Physical	Sexual	Emotional	Economic
<i>Leads</i>					
UD -2	-0.004 (0.009)	-0.006 (0.007)	0.001 (0.004)	-0.006 (0.006)	-0.003 (0.006)
UD -1	0.002 (0.006)	-0.003 (0.004)	0.005** (0.002)	0.002 (0.004)	-0.000 (0.003)
<i>Lags</i>					
UD 1	0.002 (0.005)	0.003 (0.003)	-0.002 (0.001)	0.001 (0.005)	-0.002 (0.003)
UD 2	0.011 (0.007)	0.007** (0.003)	-0.000 (0.002)	0.007 (0.008)	-0.001 (0.004)
UD 3	-0.000 (0.016)	0.003 (0.006)	-0.014*** (0.002)	-0.009 (0.011)	0.005 (0.011)
Mean	0.196	0.097	0.054	0.121	0.093
Observations	240,703	240,703	240,703	240,703	240,703

*Notes:* The Table shows the unilateral divorce legalization dynamic effects on women's probability of having suffered any, physical, sexual, emotional, or economic IPV in the previous 12 months. The considered reform dates are the *de jure* ones. The outcome variables are binary variables equal to 1 if the woman has suffered at least one act of the corresponding kind of IPV during the last year. UD k represent the leads (k=-1,2) and lags (k=1,2,3) of the event study, namely the number of survey rounds k before or after the legalization of unilateral divorce. All models control for women's age, indigenous background, educational level, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women affected by the reform, and the estimator used is the one by Sun & Abrahams (2021). «Mean» is the average of the outcome in treated states in 2006. Robust standard errors clustered by the state of residence in parentheses. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. *Source:* 2006-2021 ENDIREH.

**Table A.7. De jure dates inconsistencies**

State	De jure Date	GR (2021)	HV-P (2021a)	A (2019)	SM (2019)
Districto Federal	Oct-08	Oct-08	2008	2008	Oct-08
Hidalgo	Mar-11	Mar-11	2011	2011	Mar-11
Guerrero	Mar-12	Mar-12	2012	2012	Mar-12
Yucatán	Apr-12	Apr-12	2013	2012	Feb-13
México	May-12	May-12	2012	2012	May-12
Sinaloa	Feb-13	Mar-13	2013	2013	Feb-13
Coahuila de Zaragoza	Apr-13	Apr-13	2013	2013	Apr-13
Nayarit	May-15	May-15	2015	2015	Jun-15
Aguascalientes	Jun-15	Jun-15	2015	2015	Jun-15
Tamaulipas	Jul-15	Jul-15	2015	2015	Jul-15
<b>Michoacán de Ocampo</b>	Oct-15		2015	2015	Sep-15
Tlaxcala	Feb-16	Feb-16	2016	2016	Feb-16
Colima	Mar-16	Mar-16	2016	2016	Feb-16
Morelos	Mar-16	Mar-16	2016	2016	Mar-16
Puebla	Mar-16	Mar-16	2016	2016	Mar-16
<b>Querétaro</b>	Nov-16	Nov-16	2016		Dec-16
Baja California Sur	Dec-16		2017	2016	Jan-17
Nuevo León	Dec-16		2016	2016	Dec-16
Oaxaca	May-17		2017		Apr-17
San Luis Potosí	May-17		2017		May-17
<b>Quintana Roo</b>	Jul-17		2013	2013	
Zacatecas	Sep-17		2017		Jun-17
<b>Durango</b>	Jul-18				
Jalisco	Nov-18		2018		Oct-18
<b>Veracruz de Ignacio de la Llave</b>	Jun-20		2015		Feb-15
Baja California					
Campeche					
<b>Chiapas</b>					Jan-19
Chihuahua					
Guanajuato					
<b>Sonora</b>			2015		
Tabasco					

*Notes:* The Table shows the dates I have used in the analysis and the ones used in other papers studying the same reform. The “De jure Date” refers to the month and year in which the Family or Civil Code of the state changed, as I have retrieved. These are the dates used in the main analysis. “GR (2021)” reports the dates of column 2, Table B.1 of García-Ramos (2021), last updated in November 2016. “HV-P (2021a)” reports the dates of column 2, Table 1 of Hoehn-Velasco & Penglase (2021a), last updated in 2017. “A (2019)” reports the dates of column 1, Table 1 of Aguirre (2019), last updated in January 2017. “SM (2019)” reports the dates of column 4, Table 1 of Silverio Murillo (2019), last updated in January 2019. In pink are the states that legalized unilateral divorce between November 2006 and October 2011, in green are the ones that legalized it between November 2011 and October 2016, in yellow are the ones that legalized it between November 2016 and October 2021, and in blue are the states that have not legalized unilateral divorce at the time of the respective last updates. The states in red are the ones that would be in a different cohort according to the date reported by at least one of the other papers.



**Table A.8.** *De facto* and *de jure* reform dates

State	<i>De Facto</i> Date *	<i>De Jure</i> Date
Distrito Federal	2008	Oct-08
Hidalgo	2011	Mar-11
Guerrero	2012	Mar-12
Yucatán	2012	Apr-12
México	2013	May-12
Sinaloa	2013	Feb-13
Coahuila de Zaragoza	2013	Apr-13
Tamaulipas	2014	Jul-15
Nuevo León	2014	Dec-16
Quintana Roo	2014	Jul-17
Campeche	2014	
Chiapas	2014	
Nayarit	2015	May-15
Aguascalientes	2015	Jun-15
Querétaro	2015	Nov-16
Veracruz de Ignacio de la Llave	2015	Jun-20
Guanajuato	2015	
Sonora	2015	
Tabasco	2015	
Michoacán de Ocampo	2016	Oct-15
Tlaxcala	2016	Feb-16
Colima	2016	Mar-16
Morelos	2016	Mar-16
Puebla	2016	Mar-16
San Luis Potosí	2016	May-17
Durango	2016	Jul-18
Jalisco	2016	Nov-18
Baja California	2016	
Chihuahua	2016	
Baja California Sur	2017	Dec-16
Oaxaca	2017	May-17
Zacatecas	2017	Sep-17

*Notes:* The Table shows the *de jure* and *de facto* dates of unilateral divorce legalization. The “*De jure* Date” refers to the month and year in which the Family or Civil Code of the state changed, as I have retrieved. The year “*De facto* Date” refers to the year in which there were more than 10 unilateral divorces in the state, as defined by Hoehn-Velasco & Silverio Murillo (2020) and Hoehn-Velasco & Penglase, (2021a-b). In pink are the states that legalized unilateral divorce between November 2006 and October 2011, in green are the ones that legalized it between November 2011 and October 2016, in yellow are the ones that legalized it between November 2016 and October 2021, and in blue are the states that have not legalized unilateral divorce at the time of the 2021 ENDIREH round.

**Table A.9.** Unilateral divorce legalization dynamic effects on IPV: *De facto* dates

	(1) Any	(2) Physical	(3) Sexual	(4) Emotional	(5) Economic
<i>Leads</i>					
UD -1	0.027** (0.011)	-0.001 (0.007)	0.006** (0.002)	0.023*** (0.008)	0.015*** (0.004)
<i>Lags</i>					
UD 1	0.015 (0.011)	0.008 (0.005)	0.000 (0.003)	0.009 (0.007)	-0.001 (0.004)
UD 2	0.015*** (0.002)	0.024*** (0.003)	-0.006** (0.003)	0.002 (0.002)	-0.010* (0.005)
Mean	0.193	0.096	0.052	0.120	0.091
Observations	196,193	196,193	196,193	196,193	196,193

*Notes:* The Table shows the unilateral divorce legalization dynamic effects on women's probability of having suffered any, physical, sexual, emotional, or economic IPV in the previous 12 months. The considered reform dates are the *de facto* ones. The outcome variables are binary variables equal to 1 if the woman has suffered at least one act of the corresponding kind of IPV during the last year. UD k represent the leads (k=-1) and lags (k=1,2) of the event study, namely the number of survey rounds k before or after the legalization of unilateral divorce. All models control for women's age, indigenous background, educational level, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women affected by the reform, and the estimator used is the one by Sun & Abrahams (2021). «Mean» is the average of the outcome in treated states in 2006. Robust standard errors clustered by the state of residence in parentheses. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. *Source:* 2006-2016 ENDIREH.

**Table A.10.** Dates of other policies in 2003-2021

<b>State</b>	<b><i>De jure</i> date</b>	<b>DV cause divorce</b>	<b>DV penal</b>	<b>Abortion</b>	<b>Child marriage ban</b>
Aguascalientes	Jun-15	yes	Feb-04		Feb-16
Baja California		Sep-04	yes	Dec-08	
Baja California Sur	Dec-16	yes	Mar-05		May-14
Campeche		Jun-07	Dec-14		May-16
Chiapas		Nov-04	yes	Jan-09	May-16
Chihuahua		yes	yes	Jun-08	Dec-17
Coahuila de Zaragoza	Apr-13	yes	yes		Sep-15
Colima	Mar-16	yes	Nov-05	Nov-19	Sep-16
Distrito Federal	Oct-08	yes	yes		Jul-16
Durango	Jul-18	yes	Apr-04	May-09	Feb-17
Guanajuato		Jun-08	yes	May-09	Jul-18
Guerrero	Mar-12	yes	yes		May-17
Hidalgo	Mar-11		yes		Dec-16
Jalisco	Nov-18	Nov-07	yes	Jul-09	May-15
México	May-12	yes	yes		Mar-16
Michoacán de Ocampo	Oct-15	yes	yes		Jun-16
Morelos	Mar-16	Sep-06	Jun-06	Dec-08	Aug-16
Nayarit	May-15	May-07	Dec-04	Jun-09	Mar-16
Nuevo León	Dec-16	yes	yes		Jan-18
Oaxaca	May-17	yes	yes	Sep-09	Dec-15
Puebla	Mar-16	Nov-07	yes	Jun-09	Mar-16
Querétaro	Nov-16	Feb-08	Feb-08	Sep-09	Oct-18
Quintana Roo	Jul-17	Jul-04	Jun-06	May-09	Dec-14
San Luis Potosí	May-17	yes	yes	Sep-09	Sep-15
Sinaloa	Feb-13	yes	yes		Aug-16
Sonora		yes	yes	Apr-09	Dec-18
Tabasco		Dec-08	yes		Jul-17
Tamaulipas	Jul-15	yes	yes	Dec-09	Jun-16
Tlaxcala	Feb-16	Jan-06	Dec-13		Dec-16
Veracruz de Ignacio de la Llave	Jun-20	yes	Nov-03	Nov-09	Feb-14
Yucatán	Apr-12		yes	Aug-09	Jun-15
Zacatecas	Sep-17	yes	yes		Mar-17

*Notes:* The Table shows the timing of the various reforms implemented in Mexico during the time-frame of the analysis. “*De jure* date” refers to the month and year in which the Family or Civil Code of the state changed to allow for unilateral divorce, as I have retrieved. “DV cause divorce” and “DV penal” report the date on which the state has introduced domestic violence as grounds for divorce or has criminalized it if this was during the time frame of the analysis, while “yes” means that the policy was introduced before the last trimester of 2003, as in columns (7) and (8) of Table B.1 in García-Ramos (2021). “Abortion” refers to the implementation dates of regressive abortion laws, as in Table A2 of Clarke & Muhlrud (2021). “Child marriage ban” reports the dates of Table A.1 of Bellés-Obrero and Lombardi (2023), referring to the introduction of the minimum age at marriage of 18 without exception.

**Table A.11.** OLS event study: Wild-cluster bootstrap p-values

	(1)	(2)	(3)	(4)	(5)
	Any	Physical	Sexual	Emotional	Economic
<b>UD -2</b>	-0.001	-0.001	-0.001	-0.003	-0.002
	(0.009)	(0.006)	(0.003)	(0.006)	(0.005)
WCB p-value	0.934	0.860	0.833	0.658	0.716
<b>UD -1</b>	0.004	-0.001	0.004**	0.003	-0.001
	(0.006)	(0.003)	(0.002)	(0.004)	(0.003)
WCB p-value	0.558	0.724	0.046	0.442	0.822
<b>UD 1</b>	0.001	0.003	-0.003*	0.000	-0.003
	(0.006)	(0.003)	(0.002)	(0.005)	(0.003)
WCB p-value	0.838	0.252	0.116	0.946	0.369
<b>UD 2</b>	0.010	0.009**	-0.003	0.007	-0.002
	(0.007)	(0.004)	(0.002)	(0.006)	(0.003)
WCB p-value	0.172	0.043	0.170	0.284	0.516
<b>UD 3</b>	0.008	0.006	-0.007***	0.002	0.004
	(0.013)	(0.005)	(0.002)	(0.012)	(0.009)
WCB p-value	0.675	0.346	0.188	0.905	0.684
Mean	0.196	0.097	0.054	0.121	0.093
Observations	240,703	240,703	240,703	240,703	240,703

*Notes:* The Table shows the Ordinary Least Squares (OLS) event study estimation of the dynamic effects of unilateral divorce legalization on women’s probability of having suffered any, physical, sexual, emotional, or economic IPV in the preceding 12 months. UD k represent the leads (k=-1,2) and lags (k=1,2,3) of the event study, namely the number of survey rounds k before or after the legalization of unilateral divorce. “WCB p-value” refers to wild cluster bootstrap p-values, computed with the *boottest* command in STATA. “Mean” refers to the average IPV in treatment states in 2006. Robust standard errors clustered by the state of residence in parentheses. The specifications control for women's age, indigenous background and educational attainment, urban residence, SES index, number of children, and state and year fixed effects. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. *Source:* 2006-2021 ENDIREH.

**Table A.12.** Robustness checks: Physical IPV, all women in sample

Physical IPV	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Main	No controls	No <i>de jure</i> inconsistent	Only <i>de jure</i> = <i>de facto</i>	Confounders	No CDMX	No 12m before	CS (2021)	BJS (2021)
<i>Leads</i>									
UD -2	-0.006 (0.007)	-0.006 (0.007)	-0.002 (0.009)		-0.005 (0.006)	-0.009 (0.008)	-0.006 (0.007)	-0.000 (0.005)	0.002 (0.004)
UD -1	-0.003 (0.004)	-0.004 (0.004)	-0.002 (0.006)	-0.002 (0.007)	-0.004 (0.004)	-0.005 (0.004)	-0.004 (0.004)	0.003 (0.004)	0.006 (0.007)
<i>Lags</i>									
UD 1	0.003 (0.003)	0.003 (0.002)	0.002 (0.003)	0.011* (0.005)	0.003 (0.003)	0.004 (0.003)	0.002 (0.003)	0.004 (0.003)	0.003 (0.003)
UD 2	0.007** (0.003)	0.005 (0.003)	0.005 (0.003)	0.023*** (0.005)	0.007* (0.004)	0.009** (0.003)	0.006 (0.004)	0.005 (0.004)	0.007** (0.003)
UD 3	0.003 (0.006)	-0.001 (0.007)	0.002 (0.008)		0.006 (0.007)	0.006 (0.007)	-0.001 (0.006)	0.002 (0.006)	0.003 (0.004)
Mean	0.097	0.097	0.096	0.096	0.097	0.097	0.096	0.097	0.097
Num. states	32	32	25	19	32	31	26	32	32
Observations	240,703	240,703	189,694	117,741	240,703	231,663	194,260	240,703	240,703

*Notes:* The Table shows the robustness checks for the unilateral divorce legalization dynamic effects on women's probability of having suffered at least one act of physical IPV in the previous 12 months. UD k represent the leads (k=-1,2) and lags (k=1,2,3) of the event study. Column (1) shows the analysis results with the *de jure* dates, the Sun & Abrahams (2021) estimator, and controlling for women's age, indigenous background, educational level, urban residence, SES index, and number of children. Column (2) does not include control variables, Column (3) excludes the states with inconsistencies in the *de jure* dates, Column (4) includes only the states with consistent *de jure* and *de facto* dates, Column (5) controls for state-level confounders, Column (6) excludes Mexico City, Column (7) does not consider states implementing the policy within the 12 months preceding the survey rounds, Column (8) uses the Callaway and Sant'Anna (2021) estimator, and Column (9) the Borusjak et al. (2021) one. The sample is restricted to women affected by the reform, adjusted when omitting some states. Robust standard errors clustered by the state of residence in parentheses. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. *Source:* 2006-2021 ENDIREH.

**Table A.13.** Robustness checks: Physical IPV, married women

Physical IPV (Married)	(1) Main	(2) No controls	(3) No <i>de jure</i> inconsistent	(4) Only <i>de jure</i> = <i>de facto</i>	(5) Confounders	(6) No CDMX	(7) No 12m before	(8) 2003 round	(9) CS (2021)	(10) BJS (2021)
<i>Leads</i>										
UD -3								-0.010 (0.007)		
UD -2	-0.007 (0.007)	-0.007 (0.007)	-0.004 (0.009)		-0.007 (0.007)	-0.009 (0.008)	-0.005 (0.007)	-0.004 (0.006)	-0.001 (0.007)	0.003 (0.005)
UD -1	-0.003 (0.004)	-0.003 (0.004)	-0.000 (0.006)	-0.001 (0.007)	-0.003 (0.004)	-0.004 (0.005)	-0.003 (0.004)	-0.003 (0.004)	0.002 (0.004)	0.006 (0.008)
<i>Lags</i>										
UD 1	0.004 (0.003)	0.003 (0.003)	0.003 (0.003)	0.011* (0.005)	0.004 (0.003)	0.004 (0.003)	0.002 (0.003)	0.004 (0.003)	0.005 (0.003)	0.003 (0.003)
UD 2	0.009*** (0.003)	0.008** (0.003)	0.008** (0.003)	0.029*** (0.002)	0.009*** (0.003)	0.009** (0.004)	0.007* (0.004)	0.009*** (0.003)	0.008* (0.004)	0.008*** (0.003)
UD 3	0.002 (0.009)	0.001 (0.010)	0.000 (0.010)		0.002 (0.009)	0.004 (0.009)	-0.009* (0.005)	0.003 (0.009)	0.004 (0.009)	0.004 (0.007)
Mean	0.091	0.091	0.091	0.091	0.091	0.091	0.090	0.088	0.091	0.091
Basic controls	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State controls	No	No	No	No	Yes	No	No	No	No	No
Num. states	32	32	25	19	32	31	26	32	32	32
Survey years	2006-21	2006-21	2006-21	2006-16	2006-21	2006-21	2006-21	2003-21	2006-21	2006-21
Observations	204,995	204,995	162,004	101,539	204,995	196,811	165,946	228,381	204,995	204,995

*Notes:* The Table shows the robustness checks for the unilateral divorce legalization dynamic effects on women's probability of having suffered at least one act of physical IPV in the previous 12 months, considering women who remained married. UD k represent the leads ( $k=-1,2$ ) and lags ( $k=1,2,3$ ) of the event study, namely the number of survey rounds k before or after the legalization of unilateral divorce. Column (1) shows the results of the main analysis with the *de jure* dates, the Sun & Abrahams (2021) estimator, and controlling for women's and partners' age, indigenous background, and educational level, relationship length, urban residence, SES index, and number of children. Column (2) does not include control variables, Column (3) excludes the states with inconsistencies in the *de jure* dates, Column (4) includes only the states with consistent *de jure* and *de facto* dates (omitting the 2021 survey round), Column (5) controls for state-level confounders, Column (6) excludes Mexico City, Column (7) does not consider states implementing the policy within the 12 months preceding the survey rounds, Column (8) includes the 2003 survey round, Column (9) uses the Callaway and Sant'Anna (2021) estimator, and Column (10) the Borusjak et al. (2021) one. The sample is restricted to married women affected by the reform, adjusted when omitting some states. Robust standard errors clustered by the state of residence in parentheses. Significance levels: \*\*\*  $p<0.01$ , \*\*  $p<0.05$ , \*  $p<0.1$ . *Source:* 2006-2021 ENDIREH.

**Table A.14.** Mechanisms: Effects on joint probability physical IPV and marital status

	(1) Physical IPV	(2) Physical IPV & Married	(3) Physical IPV & Separated	(4) Physical IPV & Divorced
<i>Leads</i>				
UD -2	-0.006 (0.007)	-0.006 (0.006)	-0.002 (0.002)	0.001 (0.001)
UD -1	-0.003 (0.004)	-0.002 (0.003)	-0.001 (0.001)	0.000 (0.000)
<i>Lags</i>				
UD 1	0.003 (0.003)	0.002 (0.002)	0.001 (0.001)	0.000 (0.000)
UD 2	0.007** (0.003)	0.007** (0.003)	0.000 (0.002)	0.000 (0.000)
UD 3	0.003 (0.006)	0.001 (0.007)	0.003 (0.003)	-0.001 (0.000)
Observations	240,703	240,703	240,703	240,703

*Notes:* The Table shows the unilateral divorce legalization dynamic effects on women's joint probability of having suffered physical IPV in the previous 12 months and being married (Column 2), separated (Column 3), or divorced (Column 4). Column 1 reports the results of the main analysis. The considered reform dates are the *de jure* ones. The outcome variables are binary variables equal to 1 if the woman has suffered at least one act of physical IPV during the last year and is married/separated/divorced at the time of the survey. UD k represent the leads (k=-1,2) and lags (k=1,2,3) of the event study, namely the number of survey rounds k before or after the legalization of unilateral divorce. All models control for women's age, indigenous background, educational level, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women affected by the reform, and the estimator used is the one by Sun & Abrahams (2021). Robust standard errors clustered by the state of residence in parentheses. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. *Source:* 2006-2021 ENDIREH.



**Table A.15.** Mechanisms: Heterogenous effects on physical IPV, married women

Physical IPV	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Not completed	Education			Residence		Background	
		Primary	Secondary	Higher	Rural	Urban	Indigenous	Non-indigenous
<i>Leads</i>								
UD -2	-0.004 (0.020)	-0.007 (0.008)	-0.009 (0.010)	-0.007 (0.007)	-0.005 (0.009)	-0.006 (0.008)	0.011 (0.019)	-0.008 (0.007)
UD -1	-0.014 (0.011)	-0.003 (0.005)	-0.005 (0.006)	0.004 (0.005)	-0.008 (0.006)	-0.001 (0.004)	-0.006 (0.011)	-0.003 (0.004)
<i>Lags</i>								
UD 1	0.009 (0.009)	0.009** (0.004)	0.002 (0.003)	-0.000 (0.003)	0.000 (0.005)	0.004 (0.003)	0.007 (0.010)	0.003 (0.003)
UD 2	-0.003 (0.013)	0.013** (0.005)	0.009* (0.004)	0.007 (0.004)	0.011 (0.009)	0.008** (0.003)	0.017 (0.018)	0.008** (0.003)
UD 3	-0.019 (0.023)	0.012* (0.006)	0.000 (0.017)	0.002 (0.007)	-0.001 (0.013)	0.002 (0.007)	-0.002 (0.010)	0.002 (0.009)
Mean	0.086	0.103	0.107	0.069	0.088	0.099	0.107	0.096
Observations	14,317	65,445	87,032	38,201	45,436	159,559	14,027	190,968

*Notes:* The Table shows the unilateral divorce legalization dynamic effects on married women's probability of having physical IPV in the previous 12 months by educational level (Column 1-4), urban/rural residence (Column 5-6), and indigenous background (Column 7-8). The considered reform dates are the *de jure* ones. The outcome variables are binary variables equal to 1 if the woman has suffered at least one act of physical IPV during the last year. The educational level has four categories: did not finish primary education, completed primary education, completed secondary education, and educational attainment higher than secondary school. The analysis is conducted separately for women in the respective groups. UD k represent the leads (k=-1,2) and lags (k=1,2,3) of the event study, namely the number of survey rounds k before or after the legalization of unilateral divorce. All specifications control for women's and partners' age, indigenous background, and educational level, relationship length, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women married at the time of the survey, and the estimator used is the one by Sun & Abrahams (2021). «Mean» is the average of the outcome in treated states in 2006. Robust standard errors clustered by the state of residence in parentheses. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. *Source:* 2006-2021 ENDIREH.

**Table A.16.** Mechanisms: Effects on empowerment, married women

	(1) Employed	(2) Think of divorce because of IPV	(3) Num. decision – Alone/Jointly	(4) Num. decision – Alone
<i>Leads</i>				
UD -2	-0.004 (0.014)	-0.007 (0.012)	0.214 (0.133)	-0.034 (0.087)
UD -1	0.001 (0.009)	0.001 (0.008)	0.026 (0.078)	-0.063 (0.053)
<i>Lags</i>				
UD 1	0.010 (0.008)	-0.011 (0.007)	0.026 (0.073)	0.076* (0.043)
UD 2	0.019* (0.011)	-0.008 (0.011)	0.074 (0.122)	0.236*** (0.077)
UD 3	-0.019 (0.011)	0.022 (0.019)	-0.052 (0.183)	0.434*** (0.113)
Mean	0.346	0.159	8.434	3.533
Observations	204,963	81,751	204,995	204,995

*Notes:* The Table shows the unilateral divorce legalization dynamic effects on four measures of women's empowerment. In particular, the outcome in Column 1 is the probability that the woman worked during the previous week, in Column 2 is the probability that she thinks about ending the relationship because the partner was violent (asked only to women who suffered IPV during the current relationship), and in Columns 3-4 are the number of decisions, ranging from 0 to 11, that the woman takes alone or jointly with the partner (Column 3) or alone (Column 4). The considered reform dates are the *de jure* ones. UD k represent the leads (k=-1,2) and lags (k=1,2,3) of the event study, namely the number of survey rounds k before or after the legalization of unilateral divorce. All specifications control for women's and partners' age, indigenous background, and educational level, relationship length, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women married at the time of the survey, and the estimator used is the one by Sun & Abrahams (2021). «Mean» is the average of the outcome in treated states in 2006. Robust standard errors clustered by the state of residence in parentheses. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. *Source:* 2006-2021 ENDIREH.

**Table A.17.** Mechanisms: Heterogenous effects on employment, married women

Employment	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Not completed	Education			Residence		Background	
		Primary	Secondary	Higher	Rural	Urban	Indigenous	Non-indigenous
<i>Leads</i>								
UD -2	0.020 (0.022)	-0.005 (0.011)	-0.011 (0.020)	0.002 (0.030)	0.036* (0.019)	-0.014 (0.018)	0.021 (0.028)	-0.004 (0.015)
UD -1	-0.010 (0.012)	-0.001 (0.008)	-0.002 (0.010)	0.011 (0.015)	0.008 (0.008)	-0.001 (0.010)	-0.026** (0.012)	0.003 (0.008)
<i>Lags</i>								
UD 1	-0.002 (0.017)	-0.001 (0.008)	0.024** (0.010)	-0.001 (0.013)	-0.013 (0.011)	0.015* (0.009)	-0.021 (0.015)	0.013 (0.008)
UD 2	-0.035 (0.026)	0.010 (0.014)	0.038** (0.015)	0.018 (0.021)	-0.017 (0.014)	0.026* (0.014)	-0.005 (0.025)	0.020* (0.011)
UD 3	0.033 (0.041)	-0.034 (0.026)	-0.001 (0.017)	-0.002 (0.026)	-0.054*** (0.013)	-0.016 (0.019)	-0.089*** (0.027)	-0.010 (0.014)
Mean	0.185	0.232	0.367	0.625	0.172	0.381	0.247	0.353
Observations	14,311	65,432	87,022	38,198	45,426	159,537	14,025	190,938

*Notes:* The Table shows the unilateral divorce legalization dynamic effects on married women's employment by educational level (Column 1-4), urban/rural residence (Column 5-6), and indigenous background (Column 7-8). The considered reform dates are the *de jure* ones. The outcome is a binary variable equal to 1 if the woman was employed or worked in the previous week. Education has four categories: did not finish primary education, completed primary education, completed secondary education, and educational attainment higher than secondary. The analysis is conducted separately for women in the respective groups. UD k represent the leads (k=-1,2) and lags (k=1,2,3) of the event study, namely the number of survey rounds k before or after the policy. All specifications control for women's and partners' age, background, education, relationship length, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women married at the time of the survey, and the Sun & Abrahams (2021) estimator is used. «Mean» is the average of the outcome in treated states in 2006. Robust standard errors clustered by the state in parentheses. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. *Source:* 2006-2021 ENDIREH.

**Table A.18.** Mechanisms: Heterogenous effects on IPV attitudes, married women

Think of divorce because of IPV	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Not completed	Education			Residence		Background	
		Primary	Secondary	Higher	Rural	Urban	Indigenous	Non-indigenous
<i>Leads</i>								
UD -2	0.027 (0.025)	-0.005 (0.015)	-0.023 (0.021)	0.010 (0.032)	-0.001 (0.020)	-0.008 (0.014)	0.073 (0.043)	-0.012 (0.013)
UD -1	0.018 (0.018)	0.002 (0.010)	-0.013 (0.010)	0.024 (0.018)	-0.001 (0.014)	0.001 (0.009)	0.048** (0.022)	-0.002 (0.008)
<i>Lags</i>								
UD 1	-0.017 (0.012)	-0.011 (0.007)	-0.008 (0.010)	-0.015 (0.012)	-0.015 (0.010)	-0.010 (0.009)	0.001 (0.012)	-0.011 (0.008)
UD 2	-0.009 (0.025)	0.001 (0.012)	-0.011 (0.017)	-0.015 (0.012)	-0.014 (0.016)	-0.007 (0.012)	0.006 (0.024)	-0.008 (0.011)
UD 3	-0.007 (0.038)	0.010 (0.016)	0.052* (0.030)	-0.024 (0.021)	0.046** (0.019)	0.020 (0.018)	-0.075* (0.039)	0.034 (0.022)
Mean	0.119	0.157	0.172	0.150	0.141	0.163	0.150	0.160
Observations	5,461	26,853	35,849	13,588	16,201	65,550	5,317	76,434

*Notes:* The Table shows the unilateral divorce legalization dynamic effects on married women's IPV attitudes by educational level (Column 1-4), urban/rural residence (Column 5-6), and indigenous background (Column 7-8). The reform dates are the *de jure* ones. The outcome is a binary variable equal to 1 if the woman has thought about divorce or separation because of IPV (asked only to women who suffered IPV during the current relationship). Education has four categories: did not finish primary education, completed primary education, completed secondary education, and educational attainment higher than secondary. The analysis is conducted separately for women in the respective groups. UD k represent the leads (k=-1,2) and lags (k=1,2,3) of the event study. All specifications control for women's and partners' age, background, education, relationship length, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women married at the time of the survey, and the Sun & Abrahams (2021) estimator is used. «Mean» is the average of the outcome in treated states in 2006. Robust standard errors clustered by the state in parentheses. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. *Source:* 2006-2021 ENDIREH.

**Table A.19.** Mechanisms: Heterogenous effects on decision-making power (alone/jointly), married women

Decision-making (alone/jointly)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Not completed	Education			Residence		Background	
		Primary	Secondary	Higher	Rural	Urban	Indigenous	Non-indigenous
<i>Leads</i>								
UD -2	0.346 (0.326)	0.257 (0.213)	0.258 (0.109)	0.109 (0.092)	0.428 (0.259)	0.176 (0.121)	0.101 (0.337)	0.219 (0.134)
UD -1	0.111 (0.169)	0.005 (0.117)	0.049 (0.062)	-0.005 (0.061)	0.203 (0.146)	-0.005 (0.077)	-0.211** (0.145)	0.043 (0.080)
<i>Lags</i>								
UD 1	-0.056 (0.154)	0.020 (0.124)	0.048 (0.063)	0.008 (0.069)	0.008 (0.098)	0.032 (0.069)	-0.077 (0.179)	0.029 (0.069)
UD 2	-0.242 (0.263)	-0.008 (0.211)	0.150 (0.112)	0.107 (0.105)	-0.062 (0.157)	0.122 (0.110)	-0.163 (0.324)	0.090 (0.119)
UD 3	-1.349*** (0.218)	-0.062 (0.224)	0.002 (0.209)	0.124 (0.150)	-0.275 (0.213)	0.069 (0.175)	0.113 (0.285)	-0.056 (0.187)
Mean	6.525	7.752	8.988	9.442	7.569	8.622	7.215	8.519
Observations	14,317	65,445	87,032	38,201	45,436	159,559	14,027	190,968

*Notes:* The Table shows the unilateral divorce legalization dynamic effects on married women's alone/jointly decision-making power by educational level (Column 1-4), urban/rural residence (Column 5-6), and indigenous background (Column 7-8). The considered reform dates are the *de jure* ones. The outcome is the number of decisions that the woman takes alone or jointly with the partner, ranging from 0 to 11. Education has four categories: did not finish primary education, completed primary education, completed secondary education, and educational attainment higher than secondary. The analysis is conducted separately for women in the respective groups. UD k represent the leads (k=-1,2) and lags (k=1,2,3) of the event study. All specifications control for women's and partners' age, background, education, relationship length, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women married at the time of the survey, and the Sun & Abrahams (2021) estimator is used. «Mean» is the average of the outcome in treated states in 2006. Robust standard errors clustered by the state in parentheses. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. *Source:* 2006-2021 ENDIREH.

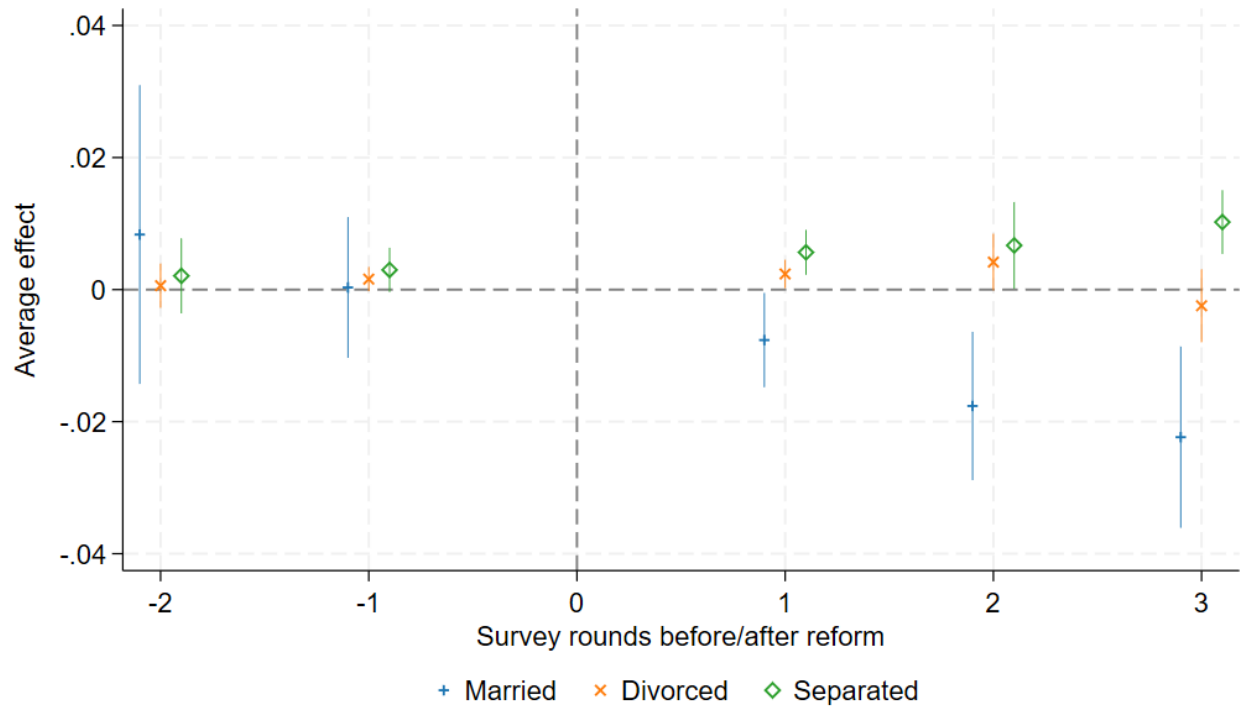
**Table A.20.** Mechanisms: Heterogenous effects on decision-making power (alone), married women

Decision-making (alone)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Not completed	Education			Residence		Background	
		Primary	Secondary	Higher	Rural	Urban	Indigenous	Non-indigenous
<i>Leads</i>								
UD -2	0.022 (0.137)	0.019 (0.143)	-0.020 (0.096)	-0.187 (0.132)	-0.025 (0.136)	-0.015 (0.095)	-0.156 (0.172)	-0.027 (0.090)
UD -1	0.014 (0.081)	-0.017 (0.079)	-0.089 (0.057)	-0.117 (0.069)	-0.046 (0.100)	-0.051 (0.056)	-0.218** (0.099)	-0.052 (0.056)
<i>Lags</i>								
UD 1	-0.068 (0.093)	0.041 (0.071)	0.146*** (0.051)	-0.007 (0.061)	-0.081 (0.097)	0.123*** (0.041)	-0.334*** (0.117)	0.108** (0.043)
UD 2	0.040 (0.131)	0.013 (0.101)	0.329*** (0.087)	0.180 (0.144)	0.049 (0.173)	0.282*** (0.080)	-0.238* (0.137)	0.270*** (0.082)
UD 3	0.000 (0.367)	0.407*** (0.100)	0.449*** (0.159)	0.459*** (0.155)	0.165 (0.181)	0.465*** (0.141)	0.237 (0.367)	0.458*** (0.101)
Mean	2.705	3.203	3.692	4.217	2.669	3.720	2.825	3.583
Observations	14,317	65,445	87,032	38,201	45,436	159,559	14,027	190,968

*Notes:* The Table shows the unilateral divorce legalization dynamic effects on married women's alone decision-making power by educational level (Column 1-4), urban/rural residence (Column 5-6), and indigenous background (Column 7-8). The considered reform dates are the *de jure* ones. The outcome is the number of decisions that the woman takes alone, ranging from 0 to 11. Education has four categories: did not finish primary education, completed primary education, completed secondary education, and educational attainment higher than secondary. The analysis is conducted separately for women in the respective groups. UD k represent the leads (k=-1,2) and lags (k=1,2,3) of the event study, namely the number of survey rounds k before or after the policy. All specifications control for women's and partners' age, background, education, relationship length, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women married at the time of the survey, and the Sun & Abrahams (2021) estimator is used. «Mean» is the average of the outcome in treated states in 2006. Robust standard errors clustered by the state in parentheses. Significance levels: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. *Source:* 2006-2021 ENDIREH.

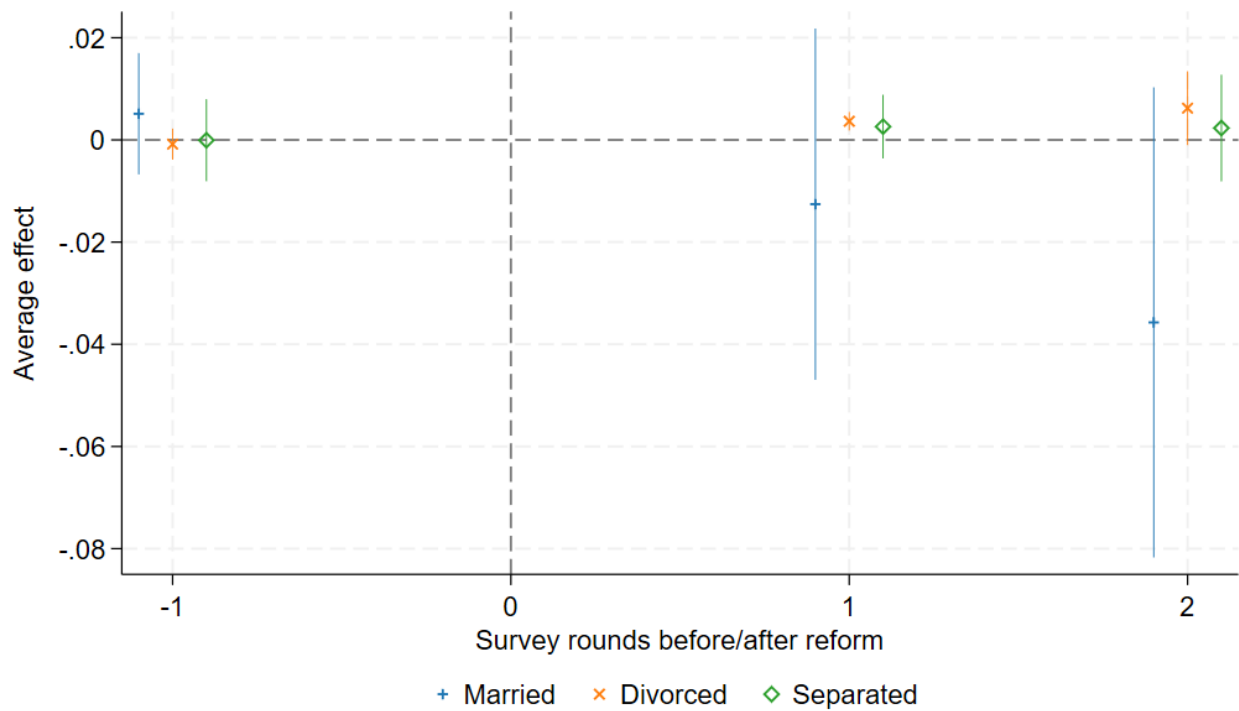
## Appendix B – Additional Figures

**Figure B.1.** Effect on marital status: *De jure* dates, all women interviewed



*Notes:* The Figure shows the dynamic effects of unilateral divorce organization on women's marital status. The outcome variables are indicators for being married (in blue), divorced (in orange), or separated (in green). The considered dates of the reform are the *de jure* ones. Robust standard errors clustered by the state of residence, confidence level at 90%. The sample includes all women interviewed. Observations: 504,361. *Source:* 2006-2021 ENDIREH.

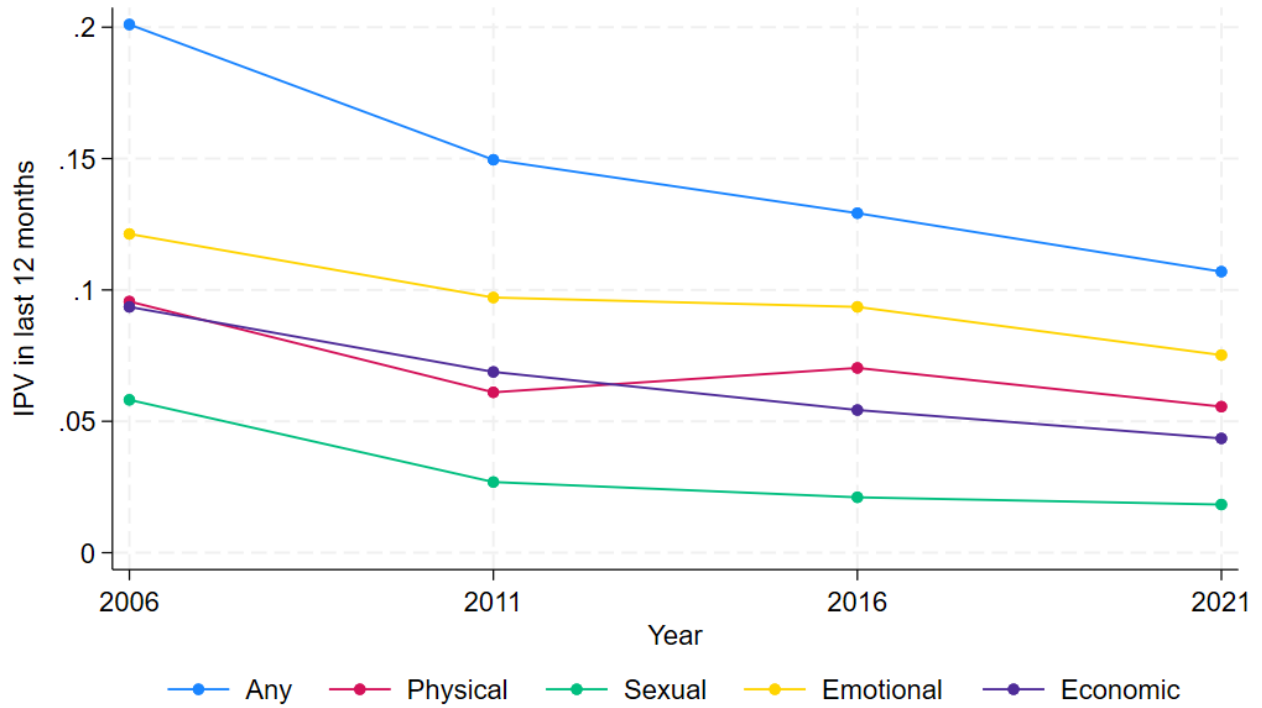
**Figure B.2.** Effect on marital status: *De facto* dates, all women interviewed



*Notes:* The Figure shows the dynamic effects of unilateral divorce organization on women’s marital status. The outcome variables are indicators for being married (in blue), divorced (in orange), or separated (in green). The considered dates of the reform are the *de facto* ones. Robust standard errors clustered by the state of residence, confidence level at 90%. The sample includes all women interviewed. Observations: 396,965. *Source:* 2006-2016 ENDIREH.

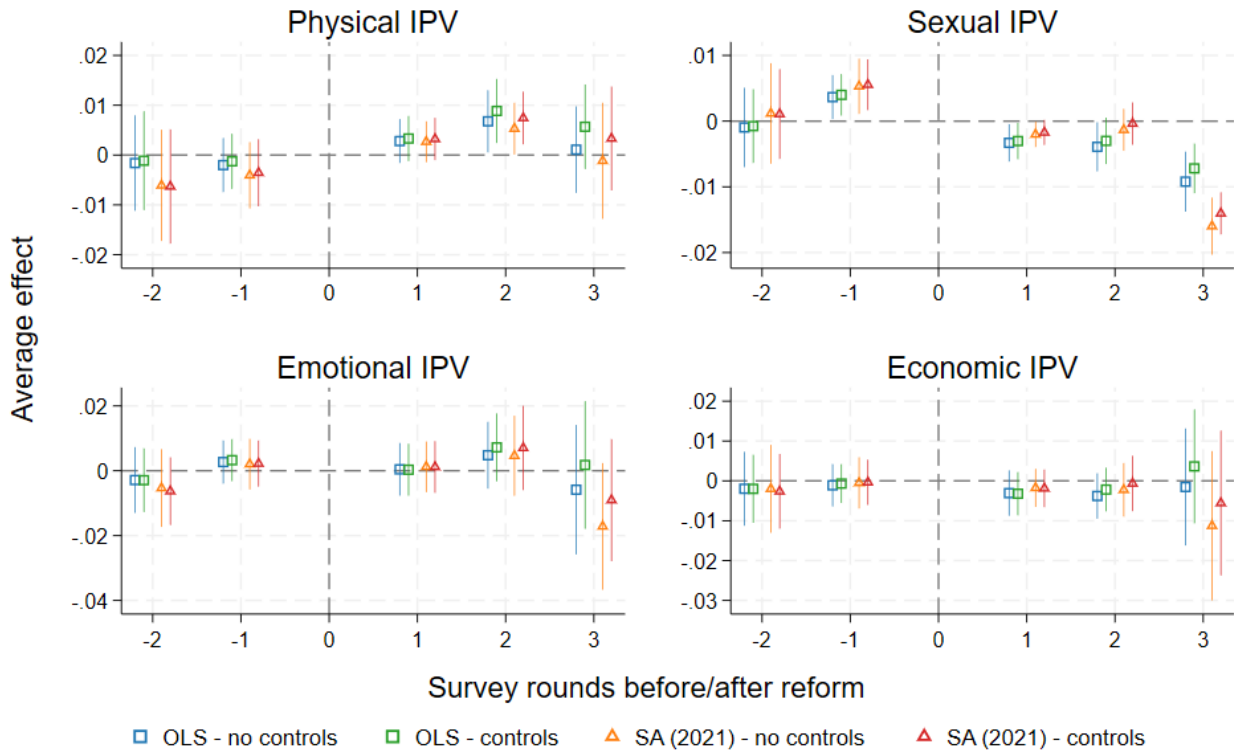


**Figure B.3.** IPV prevalence by survey year



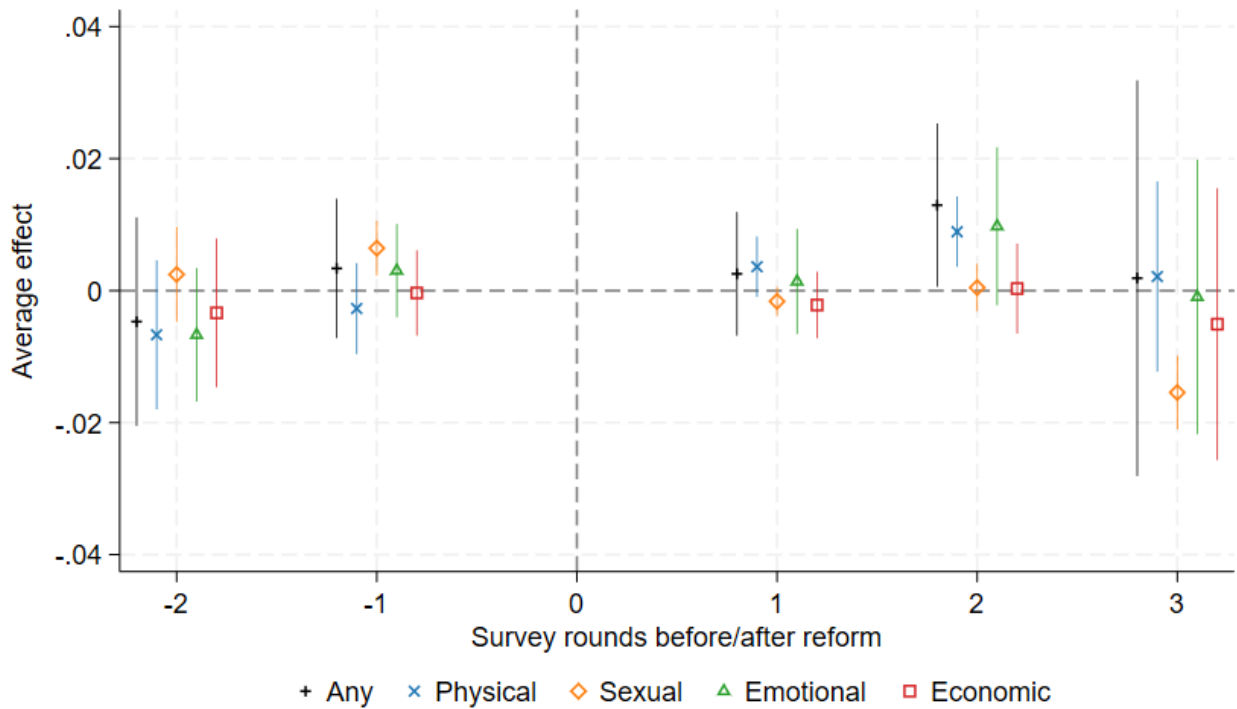
*Notes:* The Figure shows the proportion of women having suffered at least one act of any, physical, sexual, emotional, or economic IPV in the previous 12 months for every ENDIREH survey round, using sample weights and considering the sample of women affected by the reform. *Source:* 2006-2021 ENDIREH.

**Figure B.4.** Effects on IPV: OLS and SA (2021) with and without controls



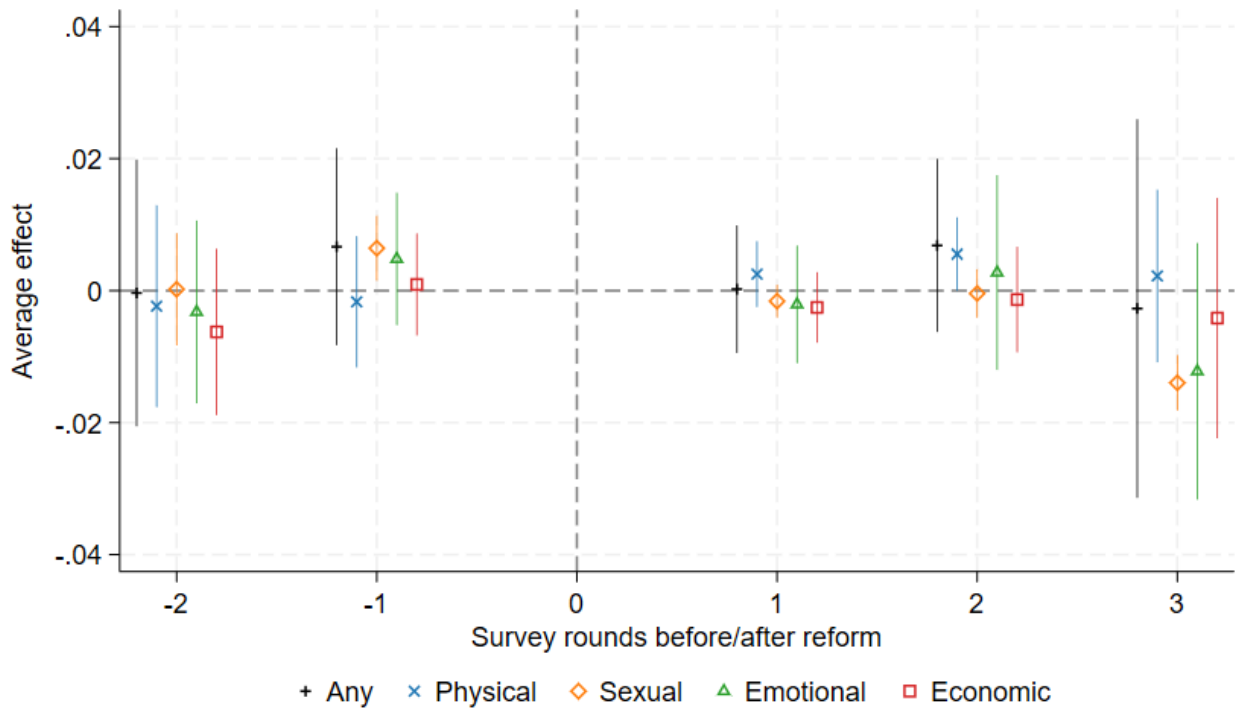
*Notes:* The Figure shows the dynamic effects of unilateral divorce legalization on women’s probability of having suffered physical, sexual, emotional, or economic IPV in the previous 12 months, with or without control variables and using different estimators. The considered reform dates are the *de jure* ones. The outcome variables are binary variables equal to 1 if the woman has suffered at least one act of the corresponding kind of IPV during the last year. “OLS” stands for Ordinary Least Squares estimator; “SA (2021)” stands for Sun and Abraham (2021) estimator. Controls include women’s age, indigenous background, educational level, urban residence, SES index, number of children, and state and year fixed effects. Robust standard errors clustered by the state of residence, confidence level at 90%. Observations: 240,703. *Source:* 2006-2021 ENDIREH.

**Figure B.5.** Unilateral divorce legalization dynamic effects on IPV, married women



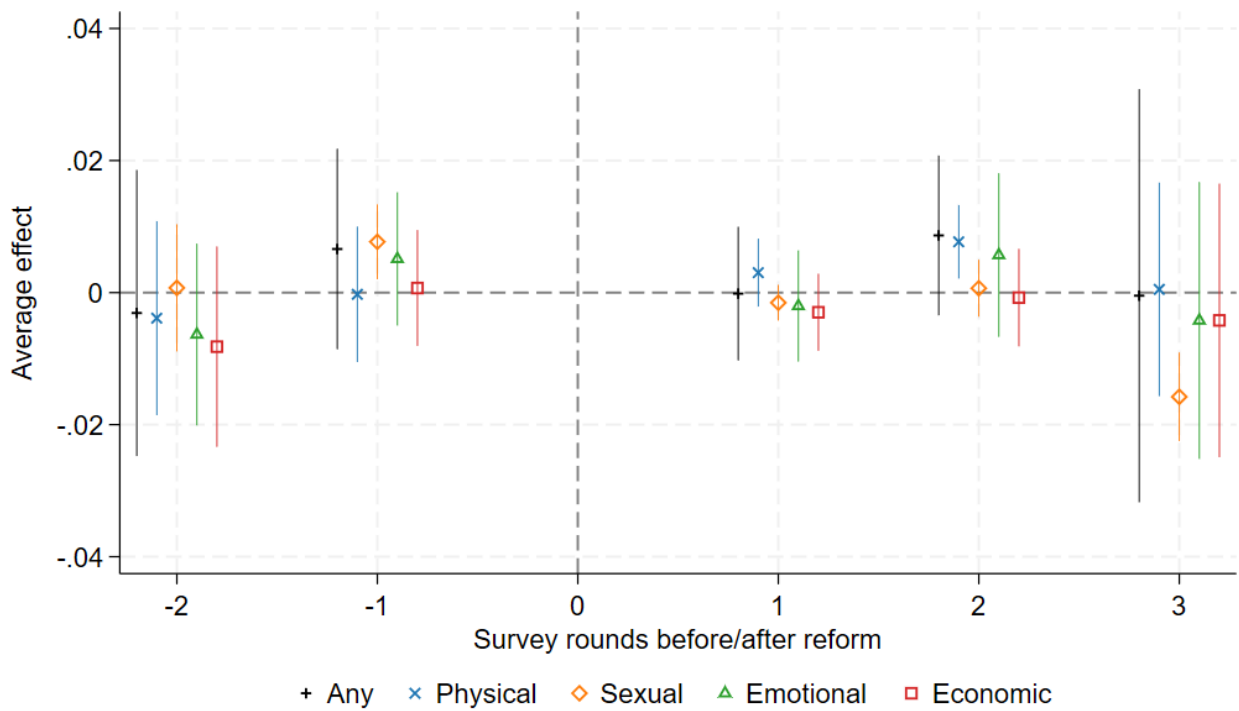
*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on women’s probability of having suffered any, physical, sexual, emotional, or economic IPV in the previous 12 months. The considered reform dates are the *de jure* ones. The outcome variables are binary variables equal to 1 if the woman has suffered at least one act of the corresponding kind of IPV during the last year. The sample is restricted to women married at the time of the survey. All specifications control for women’s and partners’ age, indigenous background, and educational level, relationship length, urban residence, SES index, number of children, and state and year fixed effects. The estimator used is the one by Sun & Abrahams (2021). Robust standard errors clustered by the state of residence, confidence level at 90%. Observations: 204,995. *Source:* 2006-2021 ENDIREH.

**Figure B.6.** Effect on IPV excluding states with inconsistent *de jure* reform dates



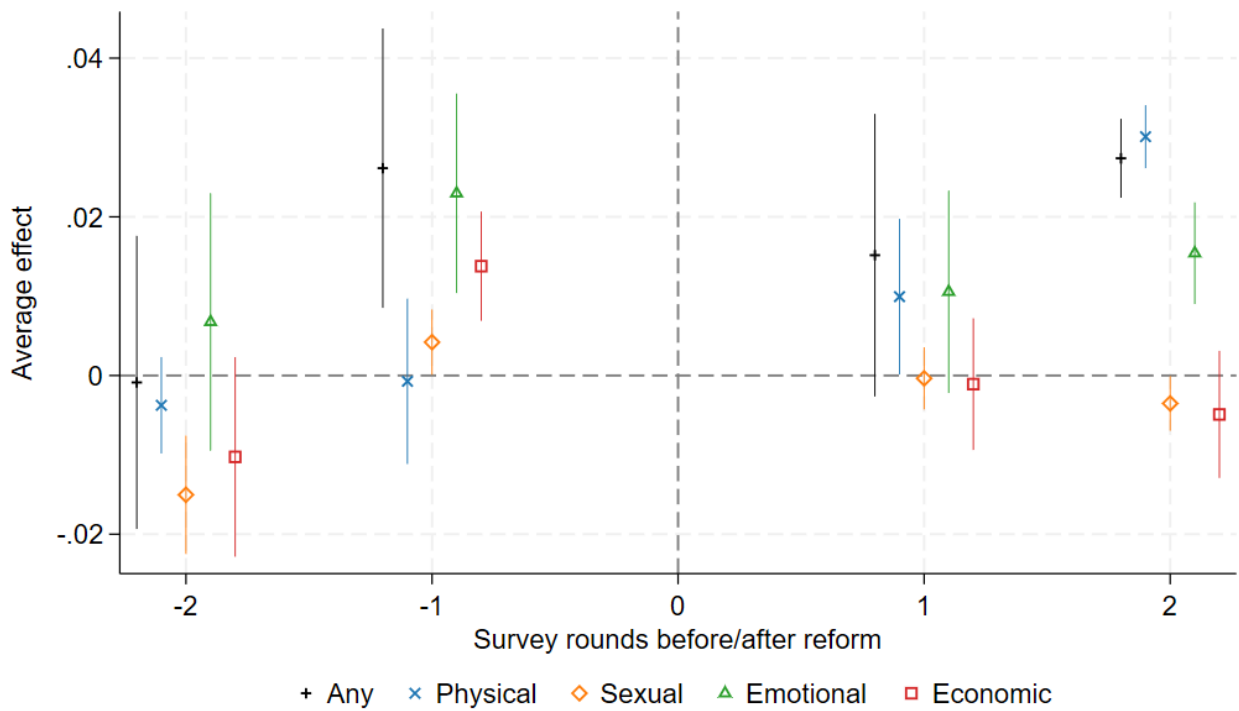
*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on women’s probability of having suffered any, physical, sexual, emotional, or economic IPV in the previous 12 months, excluding the states with inconsistencies between the reform’s *de jure* dates that I have retrieved in the states’ Civil and Family Codes and the ones indicated in the papers by García-Ramos (2021), Hoehn-Velasco & Penglase (2021a,b), Aguirre (2019), and Silverio Murillo (2019). In particular, the states of Michoacán de Ocampo, Querétaro, Quintana Roo, Durango, Veracruz de Ignacio de la Llave, Chiapas, and Sonora are excluded. The considered reform dates are the *de jure* ones. The outcome variables are binary variables equal to 1 if the woman has suffered at least one act of the corresponding kind of IPV during the last year. All models control for women’s age, indigenous background, educational level, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women affected by the reform, and the estimator used is the one by Sun & Abrahams (2021). Robust standard errors clustered by the state of residence, confidence level at 90%. Observations: 189,694. *Source:* 2006-2021 ENDIREH.

**Figure B.7.** Effect on IPV excluding states with inconsistent *de jure* reform dates, married women



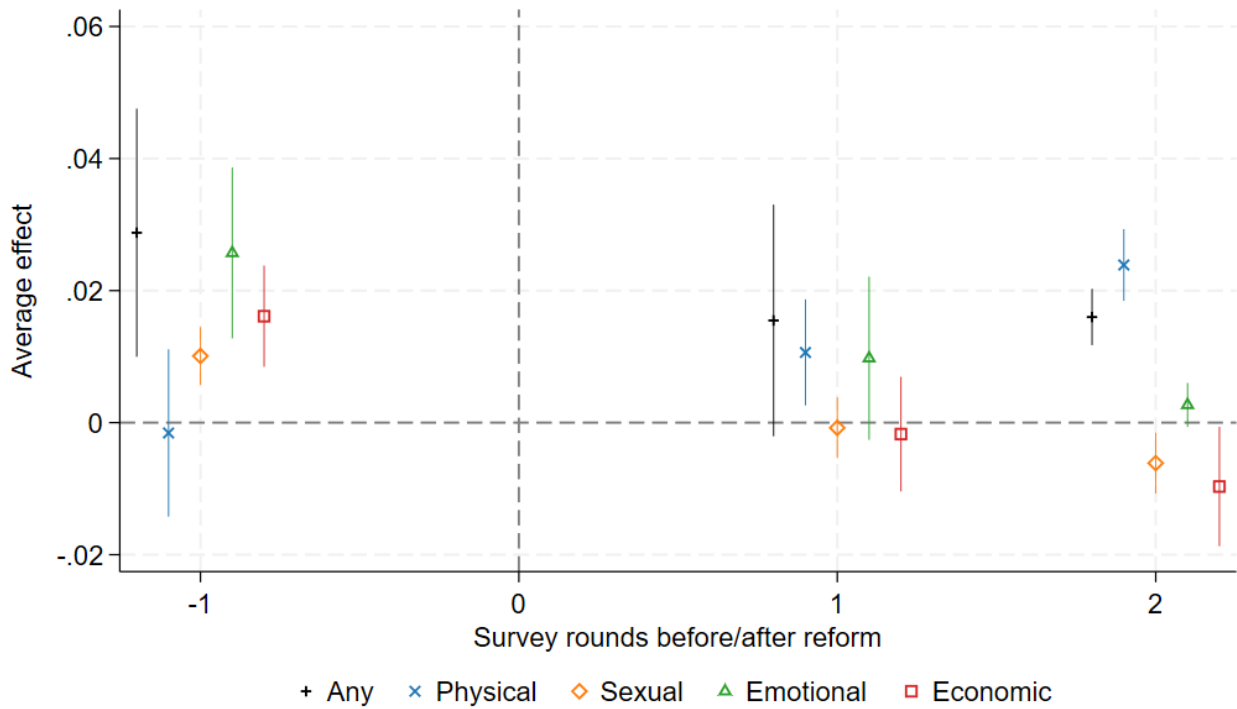
*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on married women’s probability of having suffered any, physical, sexual, emotional, or economic IPV in the previous 12 months, excluding the states with inconsistencies between the reform’s *de jure* dates that I have retrieved in the states’ Civil and Family Codes and the ones indicated in the papers by García-Ramos (2021), Hoehn-Velasco & Penglase (2021a,b), Aguirre (2019), and Silverio Murillo (2019). In particular, the states of Michoacán de Ocampo, Querétaro, Quintana Roo, Durango, Veracruz de Ignacio de la Llave, Chiapas, and Sonora are excluded. The considered reform dates are the *de jure* ones. The outcome variables are binary variables equal to 1 if the woman has suffered at least one act of the corresponding kind of IPV during the last year. All models control for women’s and partners’ age, indigenous background, and educational level, relationship length, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women affected by the reform and married at the time of the survey, and the estimator used is the one by Sun & Abrahams (2021). Robust standard errors clustered by the state of residence, confidence level at 90%. Observations: 162,004. *Source:* 2006-2021 ENDIREH.

**Figure B.8.** *De facto* unilateral divorce legalization dynamic effects on IPV, married women



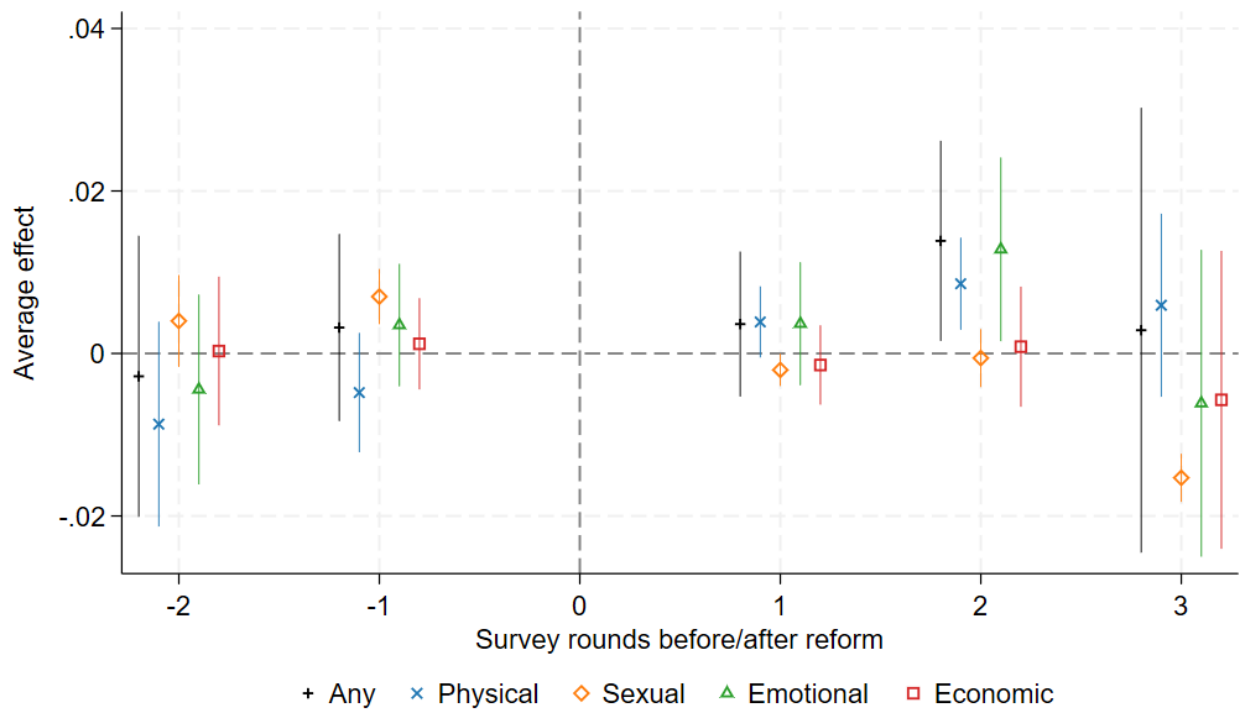
*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on married women’s probability of having suffered any, physical, sexual, emotional, or economic IPV in the previous 12 months. The considered reform dates are the *de facto* ones. The outcome variables are binary variables equal to 1 if the woman has suffered at least one act of the corresponding kind of IPV during the last year. All models control for women’s and partners’ age, indigenous background, and educational level, relationship length, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women affected by the reform and married at the time of the survey, and the estimator used is the one by Sun & Abrahams (2021). Robust standard errors clustered by the state of residence, confidence level at 90%. Observations: 193,053. *Source:* 2003-2016 ENDIREH.

**Figure B.9.** Unilateral divorce legalization dynamic effects on IPV: *De jure* = *de facto* cohorts



*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on women’s probability of having suffered any, physical, sexual, emotional, or economic IPV in the previous 12 months, including only the states that fall in the same cohort according to both the *de jure* and the *de facto* dates of the reform. In particular, the states of Nuevo León, San Luis Potosí, Quintana Roo, Durango, Jalisco, Veracruz de Ignacio de la Llave, Baja California, Campeche, Chiapas, Chihuahua, Guanajuato, Tabasco, and Sonora are excluded. The outcome variables are binary variables equal to 1 if the woman has suffered at least one act of the corresponding kind of IPV during the last year. All models control for women’s age, indigenous background, educational level, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women affected by the reform, and the estimator used is the one by Sun & Abrahams (2021). Robust standard errors clustered by the state of residence, confidence level at 90%. Observations: 117741. *Source:* 2006-2016 ENDIREH.

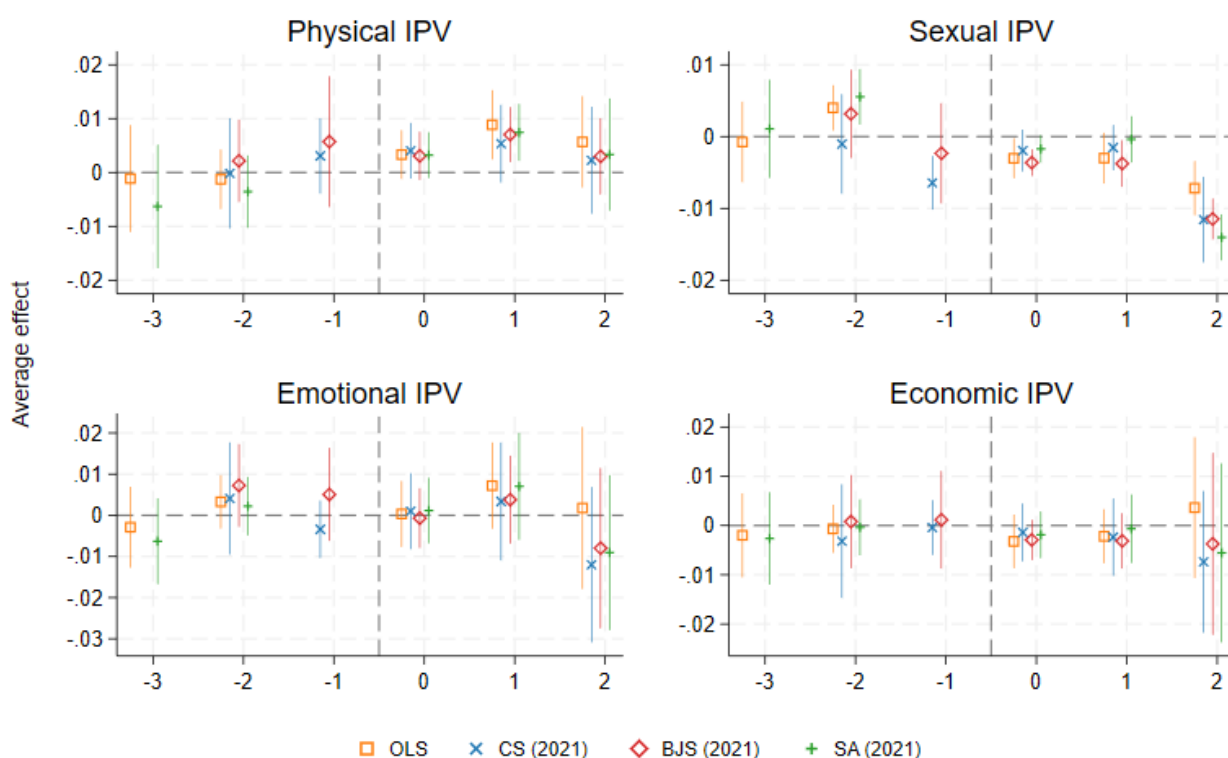
**Figure B.10.** Unilateral divorce legalization dynamic effects on IPV: No Mexico City



*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on women’s probability of having suffered any, physical, sexual, emotional, or economic IPV in the previous 12 months, excluding Mexico City. The outcome variables are binary variables equal to 1 if the woman has suffered at least one act of the corresponding kind of IPV during the last year. All models control for women’s age, indigenous background, educational level, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women affected by the reform, and the estimator used is the one by Sun & Abrahams (2021). Robust standard errors clustered by the state of residence, confidence level at 90%. Observations: 233,936. *Source:* 2006-2021 ENDIREH.

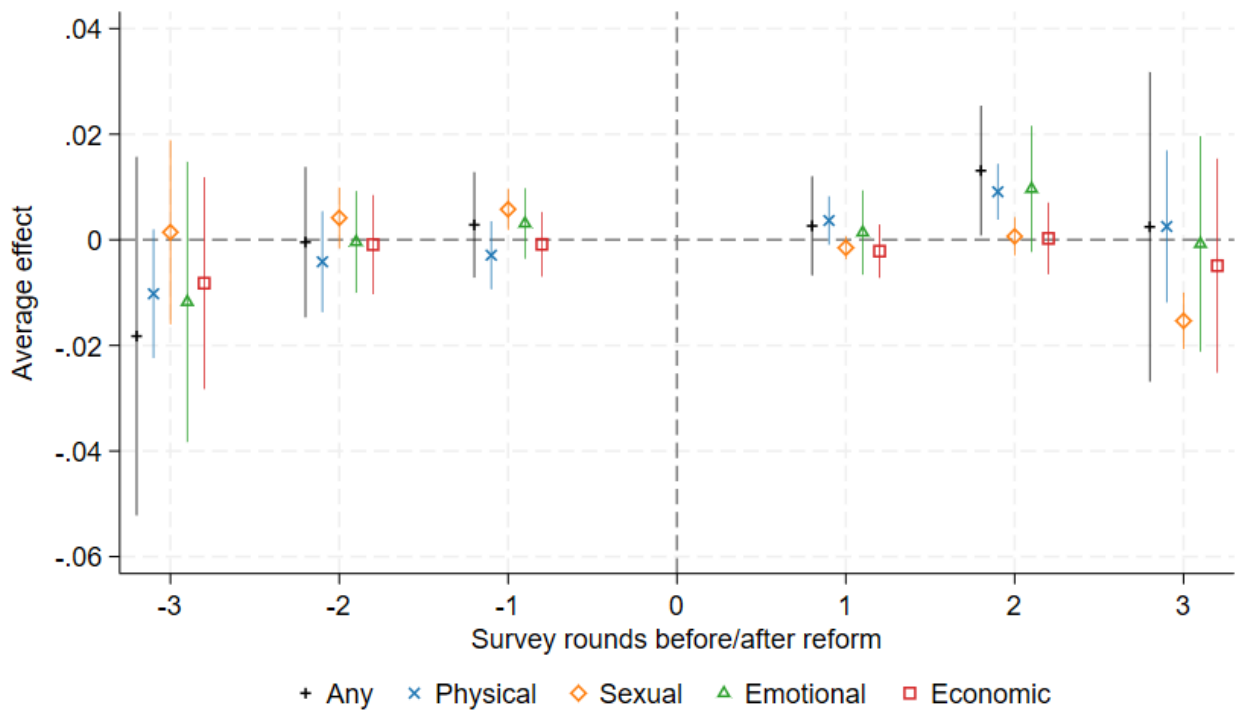


**Figure B.11.** Unilateral divorce legalization dynamic effects on IPV: All estimators



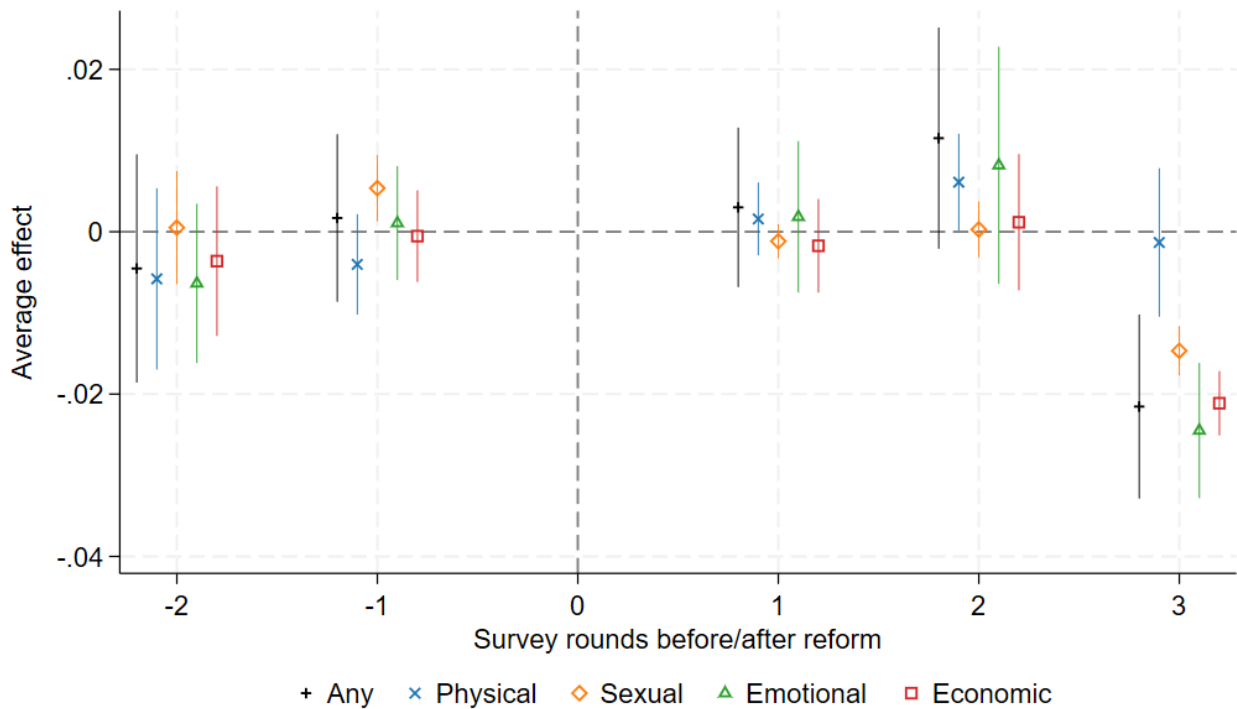
*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on women’s probability of having suffered physical, sexual, emotional, or economic IPV in the previous 12 months, using different estimators. The outcome variables are binary variables equal to 1 if the woman has suffered at least one act of the corresponding kind of IPV during the last year. “OLS” stands for Ordinary Least Squares estimator; “CS (2021)” stands for Callaway & Sant’Anna (2021) estimator, with never-treated states as the comparison group; “BJS (2021)” stands for Borusyak et al. (2021) estimator; “SA (2021)” stands for Sun & Abrahm (2021) estimator, with never-treated states as the comparison group. All models control for women’s age, indigenous background, educational level, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women affected by the reform. Robust standard errors clustered by the state of residence, confidence level at 90%. Observations: 240,703. *Source:* 2006-2021 ENDIREH.

**Figure B.12.** Unilateral divorce legalization dynamic effects on IPV, married women



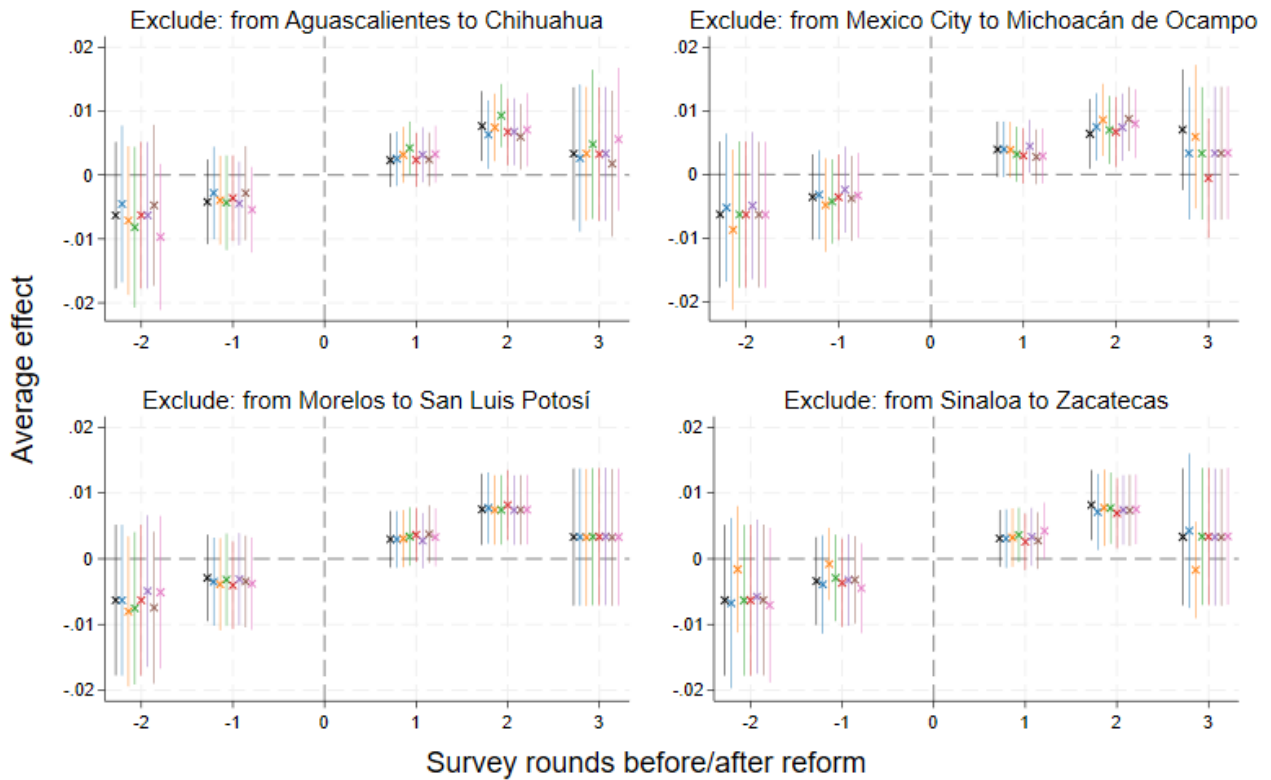
*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on married women’s probability of having suffered any, physical, sexual, emotional, or economic IPV in the previous 12 months, including the 2003 ENDIREH survey round. The considered reform dates are the *de jure* ones. The outcome variables are binary variables equal to 1 if the woman has suffered at least one act of the corresponding kind of IPV during the last year. The sample is restricted to women affected by the reform and married at the time of the survey. All specifications control for women’s and partners’ age, indigenous background, and educational level, relationship length, urban residence, SES index, number of children, and state and year fixed effects. The estimator used is the one by Sun & Abrahams (2021). Robust standard errors clustered by the state of residence, confidence level at 90%. Observations: 228,349. *Source:* 2003-2021 ENDIREH.

**Figure B.13.** Unilateral divorce legalization effects on IPV: Exclude states with policy in 12 months preceding ENDIREH rounds



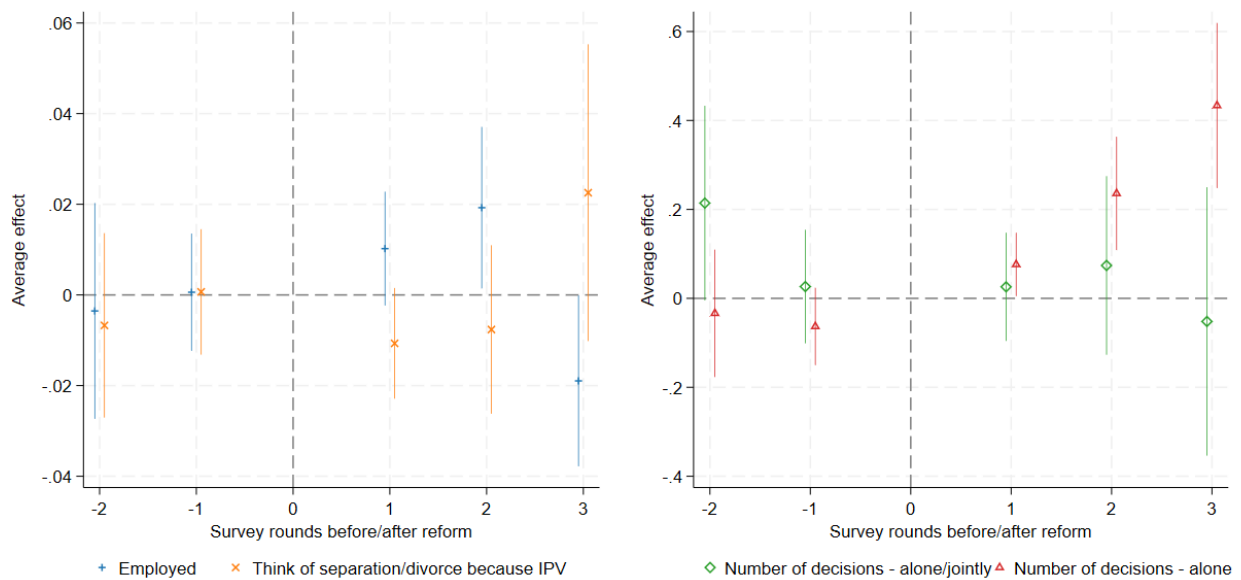
*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on women’s probability of having suffered any, physical, sexual, emotional, or economic IPV in the previous 12 months, excluding the states that implemented the policy in the 12 months preceding the ENDIREH survey round. In particular, the states of Hidalgo, Michoacán de Ocampo, Tlaxcala, Colima, Morelos, and Puebla are excluded. The outcome variables are binary variables equal to 1 if the woman has suffered at least one act of the corresponding kind of IPV during the last year. All models control for women’s age, indigenous background, educational level, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women affected by the reform, and the estimator used is the one by Sun & Abrahams (2021). Robust standard errors clustered by the state of residence, confidence level at 90%. Observations: 194,260. *Source:* 2006-2021 ENDIREH.

**Figure B.14.** Unilateral divorce legalization effects on physical IPV: Exclude 1 state at a time



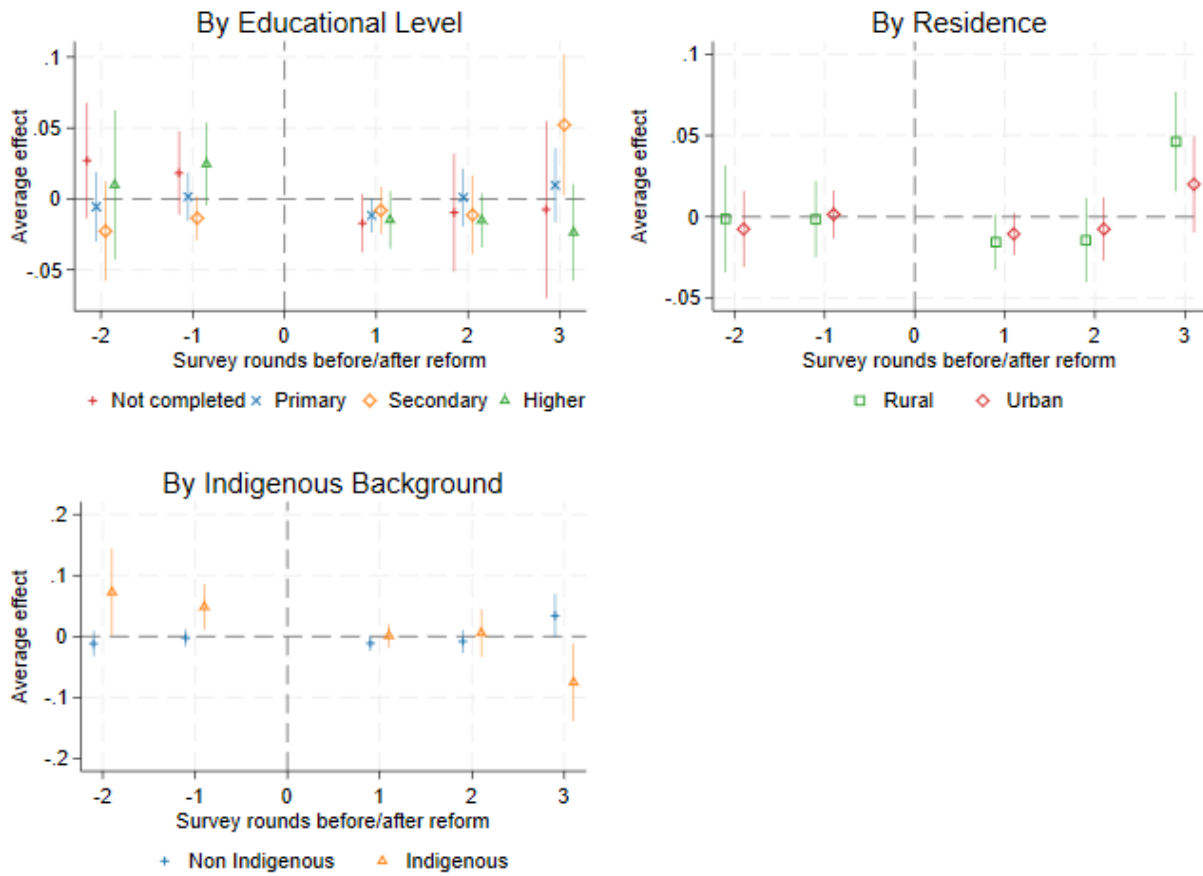
*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on women’s probability of having suffered at least one act of physical IPV in the previous 12 months, excluding one state at a time. Every specification excludes one state, in alphabetical order. All models control for women’s age, indigenous background, educational level, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women affected by the reform, and the estimator used is the one by Sun & Abrahams (2021). Robust standard errors clustered by the state of residence, confidence level at 90%. *Source:* 2006-2021 ENDIREH.

**Figure B.15.** Unilateral divorce legalization effects on empowerment, married women



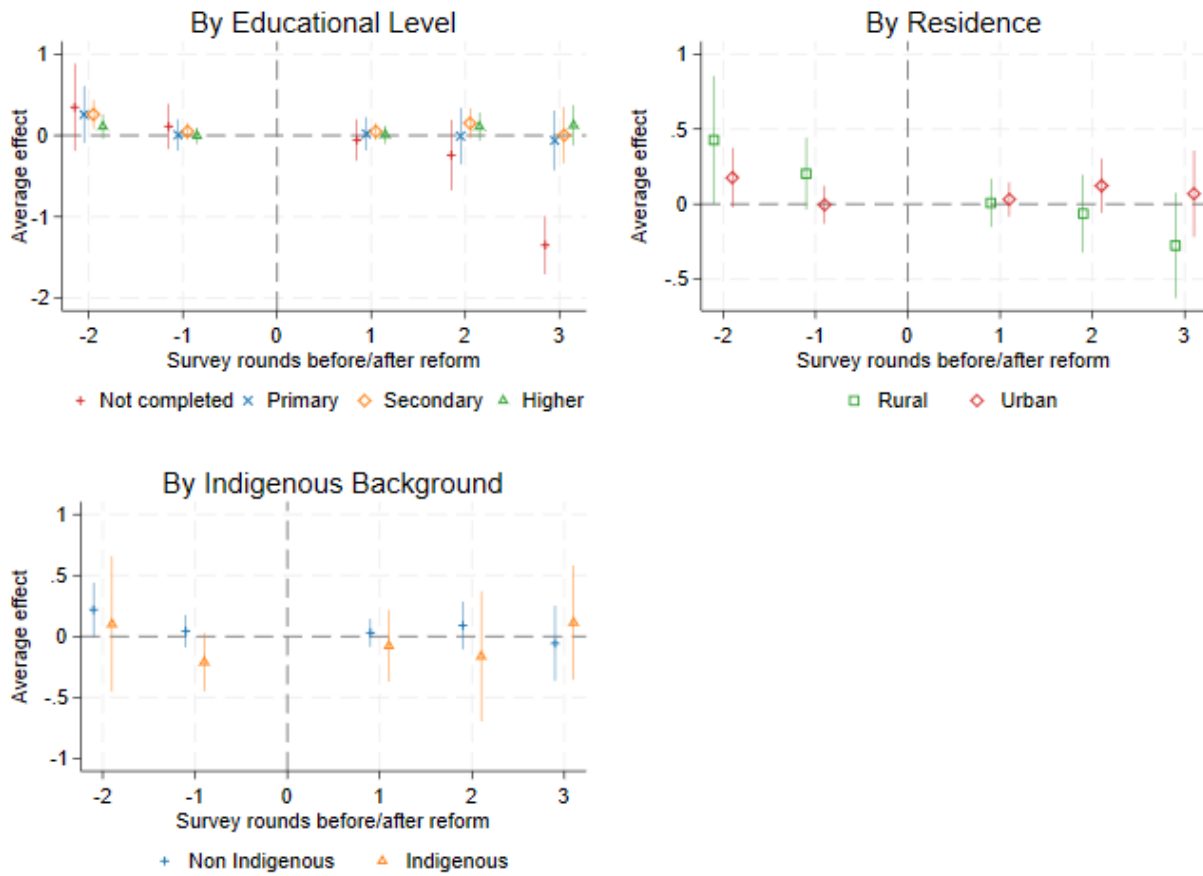
*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on variables related to female empowerment, for women who remained married. In particular, the outcomes on the left side are binary variables for the probability that the woman worked during the previous week and that she thinks about ending the relationship because of the tensions with the partner. The outcomes on the right side are the number of decisions, ranging from 0 to 11, that the woman takes either alone and alone or jointly with the partner. All specifications control for women’s and partners’ age, indigenous background, and educational level, relationship length, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women affected by the reform and married at the time of the survey, and the estimator used is the one by Sun & Abrahams (2021). Robust standard errors clustered by the state of residence, confidence level at 90%. Observations: 204,995 for “Employed” and “Number of decisions”; 81,751 for “Think of separation/divorce because IPV” (asked only to women who suffered IPV during the current relationship). *Source:* 2006-2021 ENDIREH.

**Figure B.16.** Heterogenous effects on IPV attitudes, married women



*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on married women’s probability of thinking about divorce because of tensions or IPV by educational level, urban/rural residence, and indigenous background. The outcome is a dichotomous variable equal to 1 if the woman has thought about divorce or separation because of IPV (asked only to women who have suffered IPV during the current relationship). In the Figure “By Educational Level”, the analysis is conducted separately for women who did not finish primary education (14,317 obs., 7% of the sample), who completed primary education (65,445 obs., 32%), who completed secondary education (87,032 obs., 42%), and who have educational attainment higher than secondary school (38,201 obs., 19%). In the Figure “By Residence”, the analysis is conducted separately for women living in a rural (45,436 obs., 22%) or urban (159,559 obs., 78%) area. In the Figure “By Indigenous Background”, the analysis is conducted separately for women without (190,968 obs., 93%) or with (14,027 obs., 7%) an indigenous background. Robust standard errors clustered by the state of residence. The sample is restricted to women married at the time of the survey. All specifications control for women’s and partners’ age, indigenous background, and educational level, relationship length, urban residence, SES index, number of children, and state and year fixed effects. Observations (total): 81,751. *Source:* 2006-2021 ENDIREH.

**Figure B.17.** Heterogenous effects on decisions-making power (alone/jointly), married women



*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on married women’s number of decisions that she takes wither alone or jointly with the partner by educational level, urban/rural residence, and indigenous background. The outcome is the number of decisions that the woman takes alone or jointly with the partner, ranging from 0 to 11. In the Figure “By Educational Level”, the analysis is conducted separately for women who did not finish primary education (14,317 obs., 7% of the sample), who completed primary education (65,445 obs., 32%), who completed secondary education (87,032 obs., 42%), and who have educational attainment higher than secondary school (38,201 obs., 19%). In the Figure “By Residence”, the analysis is conducted separately for women living in a rural (45,436 obs., 22%) or urban (159,559 obs., 78%) area. In the Figure “By Indigenous Background”, the analysis is conducted separately for women without (190,968 obs., 93%) or with (14,027 obs., 7%) an indigenous background. Robust standard errors clustered by the state of residence. The sample is restricted to women married at the time of the survey. All specifications control for women’s and partners’ age, indigenous background, and educational level, relationship length, urban residence, SES index, number of children, and state and year fixed effects. Observations (total): 204,995. *Source:* 2006-2021 ENDIREH.

## Appendix C – Sample Weights

The main analysis does not use sample weights. Indeed, there is an open discussion on whether and when it is appropriate to weigh the data in estimating causal effects, while this is the common practice in computing descriptive statistics. In particular, there are three reasons why sample weights may be used when estimating causal effects (Solon et al., 2015). The first motivation concerns correcting for heteroskedastic error terms. However, in the analysis, I am already taking care of this possibility by estimating heteroskedasticity-robust standard errors clustered at the state level. In this case, estimates from Ordinary Least Squares (OLS) may be more precise than those from Weighted Least Squares (WLS) regressions (Lee & Solon, 2011). Another reason for weighting is the endogeneity of sampling and non-responses. However, in the main specification, I control for the majority of the variables used to construct the sample design, so I assume that sampling and non-responses are independent of IPV conditional on the control variables. Again, in this case, both OLS and WLS estimators are consistent, but OLS may be more precise. Finally, one may want to weigh to identify the population average partial effects, but WLS estimates could not be the answer. Indeed, if the model is misspecified, meaning for instance that I am not modeling some heterogeneous effects, then both OLS and WLS estimators are inconsistent and none is better than the other<sup>35</sup>. Given this brief discussion on the possible motivations for weighting, it is clear that in my analysis I have tackled the first two reasons, while the third remains open. However, the third motivation is also the one to which there is not a clear answer on whether it is better to weigh or not, while for the other two issues, it seems that OLS estimates may be more precise. This is why I am not using sample weights in the main analysis. Moreover, this choice makes the comparison to García-Ramos (2021) easier, since the author does not use the weights either.

In any case, I show the results using the Sun and Abraham (2021) estimator for the effects of the *de jure* divorce legalization on physical, sexual, emotional, and economic IPV without and with sample weights. In Figure C.1, the blue one is my preferred specification without sample weights and controlling for women's age, indigenous background, educational attainment, and SES index. The orange specification uses sample weights; the green one uses the weights and excludes the state of Mexico, which is the most populous one; the red one excludes Mexico City, which is the second most populous entity and is usually considered the most gender-equal; and the black one excludes both Mexico state and Mexico City. In general, the results do not change too much, but some differences are worth discussing more in detail. First of all, with sample weights, there are significant negative effects on economic and emotional IPV in the third survey round after the reform. These results are

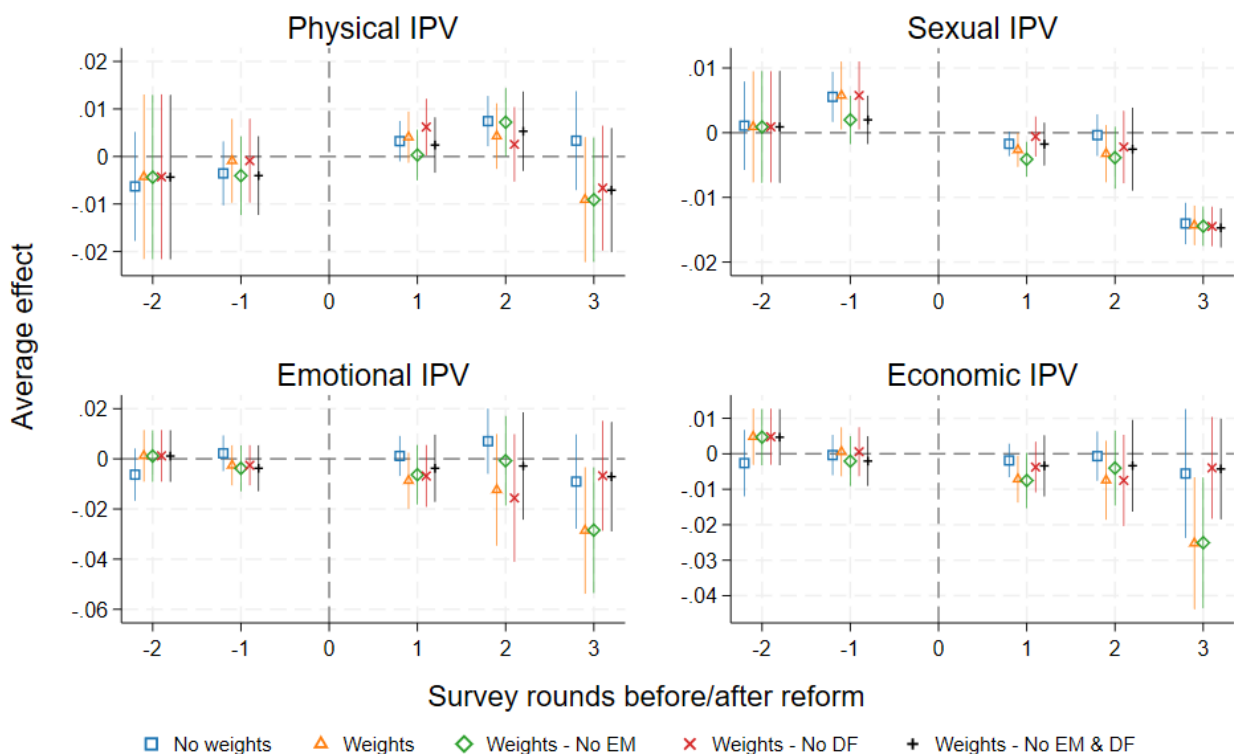
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<sup>35</sup> See Solon et al. (2015) for a detailed discussion on why this is the case.



entirely driven by Mexico City, and they may suggest that I am failing to model some heterogeneous effects. At the same time, as mentioned, the effects in  $k = 3$  should be interpreted with caution, since only two states (Mexico City and Hidalgo) have been treated for more than 9 years. Looking at the results for physical violence, the effect in the medium run is no longer significant with sample weights, but it is still positive. The loss of statistical significance may be attributed to the lower precision of the weighted estimates, and indeed the standard errors appear larger in the last four specifications. The very similar results for the blue and green identifications in  $k = 2$  may suggest that, beyond an accuracy issue, the original dissimilarity may be due, also in this case, to some unmodeled heterogeneity. Therefore, further research is needed to better understand based on what characteristics the effect may change. However, given the above discussion, I am convinced that it is more appropriate to not use sample weights in this context, and the dissimilarities when using them do not seem too large to pose serious threats to the validity of the main result.

**Figure C.1** – Unilateral divorce legalization effects on IPV with sample weights



*Notes:* The Figure shows the unilateral divorce legalization dynamic effects on women’s probability of having suffered physical, sexual, emotional, or economic IPV in the previous 12 months, with and without using sample weights, and/or excluding more populous states. “Weights” refers to sample weights provided by the ENDIREH surveys. “EM” is the state of Mexico and “DF” is Mexico City. All models control for women’s age, indigenous background, educational level, urban residence, SES index, number of children, and state and year fixed effects. The sample is restricted to women affected by the reform, and the estimator used is the one by Sun & Abrahams (2021). Robust standard errors clustered by the state of residence, confidence level at 90%. Observations: 240,703. *Source:* 2006-2021 ENDIREH.