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Divorce laws and intimate partner violence: Evidence from Mexico[☆]

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ABSTRACT

This paper examines whether divorce laws affect intimate partner violence (IPV) in the context of a developing country. Exploiting the state-level variation in the timing of the introduction of unilateral and no-fault divorce in Mexico, I estimate the causal effect of easier access to divorce on male-to-female IPV. The results show that IPV does not change in the short term, whereas it increases by 3.7 percentage points in the long term. These results are driven by women who remain married after the reform and they are consistent with IPV being used to prevent women from leaving the marriage.

1. Introduction

IPV¹ is a serious social problem. One in three women worldwide have experienced physical and/or sexual intimate partner violence at some point in their lives (WHO, 2013). IPV is associated with a wide range of negative outcomes for victims including unstable employment, reductions in productivity and earnings, and poor health (Campbell, 2002; Farmer and Tiefenthaler, 2004; Lloyd, 1997; Tolman and Wang, 2005); as well as negative externalities on children (Aizer, 2011; Carrell and Hoekstra, 2010; Pollak, 2004). Previous studies have also examined how IPV responds to factors such as labour market opportunities (Aizer, 2010; Anderberg et al., 2016; Bhalotra et al., 2021), conditional cash transfer programmes (Angelucci, 2008; Bobonis et al., 2013, 2015; Hidrobo et al., 2016), education (Erten and Keskin, 2018), arrest and prosecution policies (Aizer and Dal Bó, 2009; Chin and Cunningham, 2019; Iyengar, 2009), conflict (La Mattina, 2017), and historical aspects

(Alesina et al., 2021; Tur-Prats, 2019). There is still, however, little evidence on the impact of easier divorce on IPV.

A priori the impact of easier divorce on IPV is ambiguous. On the one hand, divorce threat models predict that IPV should decline either through an improvement in women's relative bargaining power among marriages who remain intact after the reform or through an increase in the probability that abused women divorce (Brassiolo, 2016). On the other hand, violence could increase if easier divorce challenges, either directly or indirectly, men's preferences or culturally prescribed gender norms, e.g., IPV could be used to prevent women from leaving the marriage or as a reaction to changes in labour market participation (Anderberg and Rainer, 2013; Angelucci, 2008; Eswaran and Malhotra, 2011). Thus, uncovering the sign of the effect of easier divorce on IPV is ultimately an empirical question. To the best of my knowledge only two studies have empirically analysed this research question (Brassiolo, 2016; Stevenson and Wolfers, 2006).^{2,3} Both find a significant decline in IPV following a reduction in the cost of divorce. They, however, focus

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¹ I use IPV, violence and abuse interchangeably.

² Stevenson and Wolfers (2006) exploit the state-level variation in the timing of the introduction of unilateral divorce in the United States. They employ a DiD strategy and two waves of a national survey on physical violence among married couples. Brassiolo (2016), using women in a relationship but not legally married as a comparison group, analyses the impact of a 2005 reform that significantly reduced the cost of divorce in Spain on male-to-female IPV.

³ In a recent short paper, Hoehn-Velasco and Silverio-Murillo (2020) analyse the impact of easier access to divorce on homicides and suicides in Mexico. They also provide some results on IPV, which I later discuss in more detail.

on developed countries. Developed and developing countries differ in a number of socio-economic and cultural factors, which can affect the research question of interest. In developing countries, for instance, IPV is relatively more common and divorce is less culturally acceptable, which is likely related to the importance of traditional social norms.⁴ In addition, there is less support for victims and fewer outside options, as well as more difficulties enforcing alimony payments and child maintenance (García-Moreno et al., 2005; Heise and Kotsadam, 2015; WHO, 2002, 2014). Taken together, this suggests that the results might differ compared to those observed in most of the previous studies.

This paper estimates the impact of making divorce easier on IPV in the context of Mexico. It exploits the variation in the timing of the introduction of unilateral and no-fault divorce (hereafter unilateral divorce) across Mexican states using a difference-in-differences (DiD) strategy. It employs four waves (2003, 2006, 2011 and 2016) of a nationally representative survey called National Survey on the Dynamics of Household Relationships (ENDIREH), which provides detailed information on women's experiences of IPV as well as a rich set of demographic and socio-economic characteristics. I also employ a number of other data sets including divorce and marriage statistics, migration statistics, homicide records, employment surveys and electoral data; as well as collect detailed information on divorce and domestic violence laws for each Mexican state.

I start by documenting that unilateral divorce has led to a significant increase in divorce rates and the stock of divorced women in reform states, while to an insignificant change in marriage rates and a decrease in the stock of married women. Moving to the impact of unilateral divorce on IPV, I find different results in the short and the long terms. In the short run (less than 6 years after the reform), unilateral divorce seems to have had a statistically insignificant and small impact on IPV. In contrast, in the long term (6–8 years after the reform), unilateral divorce has led to an increase in IPV of about 3.7 percentage points (21% of the baseline sample mean). These results are driven by women who continue to remain married after the reform. Moreover, they hold across three different types of IPV, namely physical, emotional and economic. The findings are robust to controlling for other reforms and state-level covariates, alternative definitions of the treatment and comparison groups, and they do not seem to be driven by changes in reporting rather than actual experiences of IPV. I also provide support for the parallel trends assumption by conducting an event study and a placebo test on an outcome that should not be affected by the reform, namely abuse by an unknown person.

The results of this paper are not in line with previous empirical findings in the context of developed countries (Brassiolo, 2016; Steven-son and Wolfers, 2006), which shows the need to analyse the effect of easier divorce on IPV in this different setting. In contrast, when comparable, they are consistent with a recent short paper that estimates the impact of unilateral divorce in Mexico on homicides, suicides, and IPV (Hoehn-Velasco and Silverio-Murillo, 2020). In particu-

lar, they find a statistically insignificant effect of easier divorce on the latter outcome. To the best of my knowledge, Hoehn-Velasco and Silverio-Murillo (2020) is the only paper that so far looks at the impact of easier divorce on IPV in the context of a developing country. My paper differs from theirs, however, in two main ways. First, I exploit the multiple survey waves to analyse the short- and long-term impacts of the reform as well as the validity of the identification strategy. Second, I examine the mechanisms behind the results.

Regarding the channels driving my findings, I argue that they are consistent with IPV being used as an instrument to prevent women from leaving the marriage. The difference between the short and the long terms could be explained by the dynamics of the marriage and by individuals adjusting slowly to changes in divorce laws. In addition, it could be that men use non-violent tools in order to impose their preferences in the short term, but they resort to violence when these are not sufficient. To further explore the divorce prevention mechanism, I check whether there are heterogeneous effects by a woman's educational attainment, which can serve as a proxy for her outside options. I find that the baseline results only hold for women who have completed at least primary education. For those who have completed less than primary, the estimates are all statistically insignificant. In sum, this provides further support for the divorce prevention explanation since women with better outside options are more likely to leave the marriage.

The rest of the paper is organised as follows. Section 2 describes the divorce reform in Mexico. Section 3 presents the data. Section 4 specifies the identification strategy. Section 5 reports the results, conducts some robustness tests, and examines the mechanisms. Section 6 provides some conclusions and discusses the implications of the study.

2. Institutional background and divorce reform

In Mexico, divorce laws are determined at the state level. The first state to introduce unilateral divorce was Mexico City in August 2008, followed by 14 other states until 18th November 2016.⁵ Column 1 of Table B.1 in the Appendix reports the date in which the reform was approved by the Legislative power, column 2 the date when it was published in the Official Journal of the state, column 3 the date when it came into force, and column 4 the decree number that reformed the Law. Unless otherwise specified, the relevant date throughout this paper is the date of approval. The reason is that the approval of unilateral divorce can have an effect on IPV by changing the dynamics of the relationship even when divorce is not in effect yet. Said this, as Table B.1 shows, in most states, unilateral divorce came into effect within two months after its approval.

Under the new legislation, any of the two spouses can file for divorce without the consent of the other and without the need to prove fault. In some cases, there is a one-year marriage requirement, which means that couples need to have been married for at least one year before they can file for divorce (see column 5 of Table B.1). The procedure of unilateral divorce is judicial and requires filing for it at the Family Court.

In the old divorce regime, if a partner wanted to unilaterally dissolve the marriage, she needed to prove a cause (e.g. domestic violence, infidelity or abandonment of the home). Fault divorce has been derogated in all reform states. Alternatively, both partners could file for divorce by mutual consent at the Family Court (judicial procedure) or, in some cases, at the Civil Registry (administrative procedure).⁶

⁴ Although I do not have information on the acceptance of divorce, I shed some light on this in two ways. First, the crude divorce rate in Mexico is low compared to that observed in developed countries. For instance, compared to the United States, Mexico's crude divorce rate was 0.7 in 2006, whereas it was 3.7 in the United States (data from the United Nations). Second, religion is relatively more important in Mexico. According to the 2010 Population Census, 83% of Mexicans consider themselves as Catholics. Moreover, a national survey on the values and attitudes of Catholics conducted in 2006 found that only 19% of Catholics do not (or very rarely) attend religious services. Although one might argue that Catholicism is also very important in Spain—the focus of one of the previous studies, the percentage of practicing Catholics has sharply decreased over time. According to the CIS Barómetro of 2006, 78% of Spanish consider themselves as Catholics of which 53% never (or very rarely) attend mass. Further support for the existence of differences related to divorce and religion views between Mexico and Spain/United States can be found in Hoehn-Velasco and Penglase, 2021.

⁵ The 2016 survey wave was conducted until this date. One state, Nuevo Leon, approved unilateral divorce on 14th November 2016, but this state is considered part of the comparison group in the main analysis.

⁶ Administrative divorce requires being above 18, not having children under custody and having reached an agreement about the dissolution of the matrimonial property. In some states it also requires having been married for at least one year.

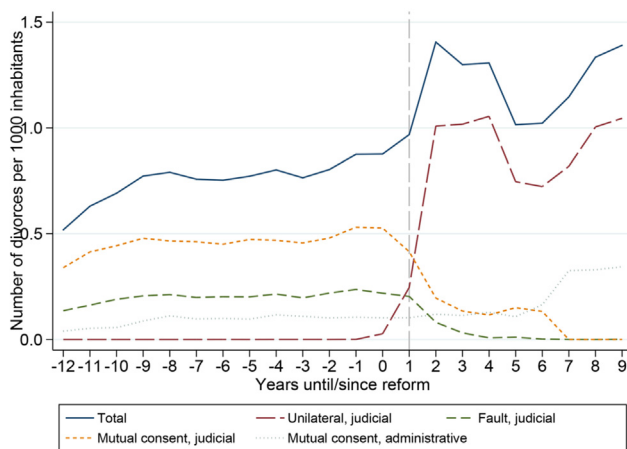


Fig. 1. Divorce rate. Notes: Sample is restricted to states where unilateral divorce came into effect by December 2016—i.e., the 15 reform states considered throughout the paper. The horizontal axis refers to the number of years until/since unilateral divorce came into force. The vertical axis reports the number of divorces per 1000 inhabitants. The vertical line refers to the year unilateral divorce came into force. Source: 2003–2016 Divorce, INEGI.

The benefits of the reform depend on the type of divorce spouses are likely to file for. If mutual consent is an option, this reform has not, in principle, introduced any benefit since spouses could file for mutual consent divorce by judicial procedure under the old regime. It has, however, reduced the cost in terms of time if unilateral divorce has come with the removal of the one-year marriage requirement. Moreover, if administrative divorce is available in the state, spouses could and still can after the reform file for it if they fulfill the requirements. Column 6 of Table B.1 reports whether the state had administrative divorce by 2003 or the date when it was introduced if after. Comparing administrative to unilateral divorce, couples that qualify for the former are expected to file for it given its lower time and likely economic cost.⁷ Support for this is found in Fig. 1, which depicts the total divorce rate and the divorce rate by type of divorce in reform states. It shows that the trend in the number of administrative divorces per 1000 inhabitants (dotted line) has not changed around the introduction of unilateral divorce (vertical line).

If mutual consent is not an option—which is likely in the context of violent marriages—or proving fault is difficult, this reform has clearly reduced the cost of divorce in time, monetary and emotional terms. There is indeed qualitative evidence showing that proving the cause of domestic violence can be difficult (Al, 2008; IAM, 2007). Moreover, in a recent paper, Hoehn-Velasco and Penglase, 2021 report a significant decline in the average length of the divorce process after divorce becomes easier. Nonetheless, the length of time and the cost needed to obtain unilateral divorce depend, among other things, on whether the spouses reach an agreement regarding the children's custody, alimony payments, use of marital home and dissolution of the matrimonial property.

The adoption of unilateral divorce has also been accompanied by changes in the economic compensation upon divorce. In particular, it has been made explicit that there must be a compensation for the spouse who has been responsible for the household chores and care of the chil-

dren, or who has not acquired property (or of significantly lower value) during the marriage. Although an economic compensation existed in the old divorce regime, it was received only in the case of fault divorce and by the innocent spouse.

The reduction in the cost of divorce had immediate effects on the total divorce rate with a growth rate of 14% in the year of the reform and 38% in the year after (see Fig. 1). Fig. C.1 in the Appendix shows the evolution of divorce rates in each reform state.

Furthermore, easier access to divorce could have had a differential impact on wives and husbands. According to divorce threat models, the spouse relatively more willing to exit the marriage, would have benefited the most from the reform. I investigate this in two ways. First, using divorce statistics and restricting the sample to reform states, I look at the percentage of divorces petitioned by wives and husbands grounded on the cause of domestic violence between 2003 and the year before unilateral divorce. Women initiated the petition for divorce in 80% of the cases, as compared to men who initiated it in 20%, and this split has been stable during the period.⁸

Second, I use information available in the ENDIREH survey, where abused women are asked who took the decision of divorcing or separating. In about 65% of the cases it was the woman, in 21% the man, and in 14% both.⁹ Taken together, women seem to be the most willing to exit the marriage in cases of violent relationships. In addition, upon divorce, the change in the economic compensation rules explained above is likely to have benefited women relatively more than men, since they usually fulfill the role of 'housewives' (57% versus 0.2% of men) and acquire less assets during the marriage.¹⁰

3. Data

3.1. Survey and sample

The main data source used in this paper is a cross-sectional national and state-level representative survey called ENDIREH, which collects detailed information on women's experiences of violence in Mexico. The information was collected through face-to-face interviews conducted by trained women between October and November of 2003, 2006, 2011 and 2016.¹¹ Interviews took place in private and participants were guaranteed confidentiality, which reduces concerns of

⁸ A very similar split is observed for states that do not approve unilateral divorce between 2003 and 2016, which suggests that these figures are not driven by specific characteristics of reforming states. Furthermore, considering both groups and all non-mutual consent divorces regardless of the cause, women filed for divorce in 66% of the cases, while men in 34%.

⁹ Excluding mutual consent divorces, the split for divorced women is 81–19% which is very similar to the one reported using divorce statistics.

¹⁰ In some of the states, e.g. Mexico City, this change only applies when the matrimonial property regime is separation. In Mexico, there is no matrimonial property regime by default, but spouses need to choose it at the time of the marriage. They have the option of community and separation regimes, or a mix of both types; and this has not changed with the reform. The survey provides information on the matrimonial property regime of couples in 2003, which indicates that 69% of them were married under a community property regime, 14% under a separation regime and 17% did not know the regime. If these numbers can be extrapolated to the other three years, community property regime seems to be the most prevalent. Moreover, marriage statistics recode the matrimonial property regime since 2009, which confirm that community property regime is the most common one. In this regime, the spouse who has acquired less assets during the marriage—usually the wife, would be the one benefiting the most from unilateral divorce (Dee, 2003; Voena, 2015). Consequently, I would expect that, overall, easier divorce has not damaged women in terms of property.

¹¹ The survey is representative in all states in 2006, 2011 and 2016, but only in eleven in 2003. To address any potential concern related to this data limitation, I do several things. First, I will present the baseline results excluding 2003 (section 5.2). Second, I will conduct a bunch of robustness checks both including and excluding 2003, which analyse the sensitivity of the baseline estimates (sections 5.3.2, 5.3.3 and 5.3.4).

⁷ Administrative divorce is usually granted within 15 and 30 days, while a judicial divorce requires at least one month. In terms of cost, taking Mexico City as an example, the cost of registering an administrative divorce is 1260 Mexican pesos, while a judicial decree 250 (SEFIN, 2020). Once legal fees have been taken into account, however, the overall cost of an administrative divorce is usually lower than that of a judicial divorce.

under-reporting (Ellsberg et al., 2001; WHO, 2001). In 2003, the target population were women aged 15 or older with a partner residing in the household. In 2006–2016, this population was expanded to include all women regardless of their marital status and whether they were residing with their partner.¹² A total of 34,184, 133,398, 152,636 and 111,256 women were interviewed in 2003, 2006, 2011 and 2016, respectively.

My sample is comprised of women who were married at the time of the reform and continue to be so or have divorced by the time of the survey. These are the women affected by the reform. I exclude those who have married after the reform, since there could be unobservable characteristics that differ between couples formed before and after the reform, which could in turn be correlated with IPV.

It is important to mention that married women include, in addition to those residing with their partner, women who are separated temporarily and those who are separated permanently but not divorced yet. In terms of the survey, women separated temporarily are considered as if they were residing with their partners, while those separated permanently (hereafter referred to as separated) as if they were divorced. This will have further implications for the analysis since the latter group was not surveyed in 2003. I will come back to this when I present the baseline results in section 5.2.

I define the sample using information on the relationship status of the woman at the time of the survey, the length of the marriage and the length since divorce/separation. I use the same definition for the survey waves before the reform in order to ensure comparability across years.¹³ After all these adjustments and excluding women with missing values in any of the variables¹⁴, the final sample is reduced to 214,972 women (of which 186,584 continue to be married after the reform): 23,333 in 2003 (all in intact marriages), 66,157 (59,782 continue to be married) in 2006, 74,770 (60,285 continue to be married) in 2011, and 50,712 (43,184 continue to be married) in 2016.

3.2. Variables

ENDIREH includes a specific module on women's experiences of IPV. In particular, women were asked whether they had experienced a series of events. In these questions, the words 'violence' or 'abuse' were not used. Not using these words and asking about specific events follows the World Health Organisation (WHO) guidelines on how to collect IPV information and further reduces concerns of under-reporting (Ellsberg et al., 2001; WHO, 2001).¹⁵

Table 1
Descriptive statistics of IPV.

	(1)	(2)
	Mean	Standard Deviation
Any	0.160	0.366
Physical	0.072	0.259
Sexual	0.037	0.188
Emotional	0.104	0.305
Economic	0.074	0.262

Notes: Sample includes women who were married or separated at the time of the reform regardless of their marital status at the time of the survey. Source: 2003–2016 ENDIREH.

I use a total of 28 events¹⁶, which I group into four categories of IPV, namely physical, sexual, emotional and economic. The Appendix presents the list of violent items included in each category. Physical IPV includes serious threats of physical violence since they can be considered to be similar in terms of their severity. This is consistent with the classification of physical abuse used in previous studies (e.g. La Matina (2017); Stevenson and Wolfers (2006)). For the rest of the items, I follow the classification suggested by ENDIREH.

For each of the four categories of IPV, I construct a dichotomous variable equal to one if the woman has experienced any violent item in the twelve months prior to when the survey was conducted and zero otherwise. In the case of emotional and economic abuse this indicator is equal to one if the woman has experienced at least two of the items in order to reduce potential measurement errors. In addition, I also construct an indicator of whether the woman has experienced any violent event, regardless of the type. Table 1 depicts the prevalence rate of my measure of IPV in the sample. As can be seen, 16% of women have experienced violence in the twelve months before the survey. The most prevalent is emotional abuse, followed by economic, physical and sexual, respectively.

ENDIREH also collects information on a rich set of individual, couple and household characteristics. I specifically use as covariates a woman's age, woman's indigenous background, woman's educational attainment, presence of children, number of children, urban residence and a socio-economic status (SES) index. When restricting the sample to women in intact marriages residing with their partners, I also control for the partner's characteristics including age, indigenous background and educational attainment; and for the length of the relationship (see details of the construction of these variables in the Appendix).

I do not control for a woman's employment status because it could be affected by the divorce reform (Bargain et al., 2012), as well as IPV (Tolman and Wang, 2005). Similar concerns could arise with educational attainment and number of children.¹⁷ To examine this, I cross-tab educational attainment at the time of the survey with that at the time of the reform. I construct the latter based on the age of the woman (partner) at the time of the reform, her (his) educational attainment at the time of the survey and the theoretical level of education the person should have at the time of the reform given her age. I find that

¹² For 2006–2016, this includes married (46%), cohabiting (15%), divorced (2%), separated (7%), widowed (7%), dating (8%) and single (15%) women.

¹³ For Mexico City and Hidalgo, which have two post-reform periods, this follows the definition used for the 2011 survey wave. The other reform states only have one post-reform survey wave, thus the definition corresponding to it is used. Regarding women living in non-reform states, I apply the same definition than the one for reform states but using the average length of the marriage and the average length since divorce/separation across reform states.

¹⁴ I decide to do pairwise deletion due to the small number of missing values, namely 3% of the total.

¹⁵ Two main alternatives to survey data have been used in previous studies analysing crimes against women, namely police data (Iyer et al., 2012) and hospitalisation data (Aizer, 2010). In the case of Mexico, both are likely to be highly underreported. For instance, only about 7% of women report either physical or sexual IPV to the police or another authority and this number drops to less than 2% when physical IPV is not involved.

¹⁶ In 2003, 29 questions were asked. In 2006–2011 30, while in 2016 36. Only 29 are, however, comparable across years. I further exclude one event, namely 'has your partner stopped talking to you?', since this question can be interpreted with a large degree of subjectivity. Moreover, this item is not included in the IPV modules of the Demographic and Health Survey or of the WHO questionnaire, which have been widely employed to capture information on IPV. As a robustness check, however, section 5.3.4 will show that the results do not change if I include this item.

¹⁷ For instance, the experience of IPV or the introduction of unilateral divorce could have led women to invest in education in order to have better outside options upon divorce, which could have in turn affected their probability of leaving the marriage as well as IPV. The number of children could have also changed as a consequence of IPV and the perceived risk of divorce.

both education variables are almost identical for both women and men. Regarding the number of children, 90% of women had the same number at both the reform and the survey times.¹⁸ This evidence suggests that defining these two variables at the time of the survey is unlikely to pose any major concern. To further provide support for this, I will present the baseline results both controlling and not controlling for covariates, which will show very similar estimates.

In addition to ENDIREH, I employ a number of other data sets. First, I use divorce statistics to examine the impact of unilateral divorce on divorce rates.¹⁹ The divorce rate is constructed as the number of divorces per 1000 inhabitants or per 1000 married people in the previous year. In the latter case, I measure the number of divorces in year t and the number of married people in year $t-1$ in order to avoid double counting a divorce in both the numerator and the denominator. The period used is 1998–2016. Divorce data come from the National Institute of Statistics and Geography (INEGI). Population data by state and year are obtained from the National Council of Population (CONAPO). Information on married people is obtained from the 1990, 2000 and 2010 population censuses and the 2015 inter-census survey. For the years with no data I use linear extrapolation. Second, I look at marriage rates which are defined as the number of marriages per 1000 people. Marriage statistics also come from INEGI. Third, I employ migration data from CONAPO for the period 1998–2016 to analyse whether unilateral divorce has changed the inter-state immigration rate of women and men.

Fourth, I use the number of intentional homicides per 100,000 inhabitants (homicide rate) as a proxy for the violent environment in the state. Homicides come from death certificates and are provided by INEGI. To more precisely match the homicides to the reference period of ENDIREH, I use the first three quarters of the calendar years corresponding to ENDIREH and the last quarter of the previous calendar years. Fifth, I employ data from ENOE and ENE (National Labour Force Survey). These are quarterly surveys, which allows me to precisely match their timing with that of ENDIREH. I use these data to construct two covariates. The first is the unemployment rate as a proxy for the business cycle, which is measured as the ratio of the unemployed population to the economically active population. The second is the gender gap (male – female) in labour force participation rate as a proxy for gender equality. Finally, I construct a categorical variable which captures information on the ruling party in the state during the year before the survey. I collect these data from each of the 32 electoral institutes in Mexico (one for each state). When there is more than one ruling party, I consider the one that has been in office for a longer period of time.

4. Identification strategy

The state-level variation in the timing of the adoption of unilateral divorce provides an ideal setting for a DiD framework. More formally, I pool the four survey years and estimate the following linear probability model (LPM):

$$IPV_{ist} = \gamma + \delta Reform_{st} + X_{ist}\beta + \alpha_s + \mu_t + \varepsilon_{ist} \quad (1)$$

where ist refers to woman i living in state s at the time of the survey observed in survey year t . IPV_{ist} is an indicator of whether the woman has experienced IPV in the past twelve months. $Reform_{st}$ is a dichotomous variable equal to one if the state has approved unilateral divorce by the survey year t , and zero otherwise. δ is the DiD estimator or, in other words, the average change in the prevalence of IPV due to

the divorce reform. X_{ist} includes a set of covariates at the individual, couple and household levels. These variables control for differences in observable characteristics across women that could have affected the selection into the state of residence—and hence whether they live in treatment states, as well as IPV. α_s is a vector of state fixed effects for a woman's state of residence, which controls for time-invariant unobservable characteristics across states, such as historical and cultural factors, and policies that were introduced before my time span. μ_t is a vector of survey year fixed effects, which captures aggregate shocks including nationwide reforms (e.g. General Law on Women's Access to a Life Free of Violence of 2007). I cluster the standard errors by the woman's state of residence in order to allow for within-state autocorrelation of unobservable shocks (Bertrand et al., 2004). However, previous studies have shown that, when the number of clusters is small (in my case 32), t-tests based on cluster-robust standard errors tend to over-reject. One usual way of dealing with this is to use wild cluster bootstrap (Cameron et al., 2008). Thus, I present the p values associated with the wild cluster bootstrap (restricted version) in square brackets in most of the tables.

I also exploit the fact that there are two waves of post-reform data for two states, namely Mexico City and Hidalgo, to examine whether the reform might have had a differential impact on IPV depending on the length of exposure to it. Fig. C.2 in the Appendix provides a detailed overview of the timing of the reforms, which has already been shown in column one of Table B.1. More specifically, I estimate the following model:

$$IPV_{ist} = \gamma + \sum_{k=1}^2 \delta_k Reform_{st}^k + X_{ist}\beta + \alpha_s + \mu_t + \varepsilon_{ist} \quad (2)$$

$Reform_{st}^k$ is a set of indicators where k captures the number of survey waves since unilateral divorce was approved. In the baseline specification, k takes two values only. It is equal to one when the state has been exposed to unilateral divorce for one survey wave, i.e., equal to one if the state is Mexico City or Hidalgo in 2011, or any other reform state in 2016. This is equivalent to considering states where the survey has been conducted within five years after reforming. Thus, δ_1 can be considered to measure the short/mid term (short term hereafter) effect of the reform. k equals two when the state has been exposed to the reform for two survey waves, i.e., if the state is Mexico City or Hidalgo in 2016. In other words, it is equal to two when the length between the reform and the survey wave is 6–8 years. δ_2 measures the long term effect of the reform.

In the robustness checks section (section 5.3), I will split $k = 1$ into two groups in order to further examine the short-term impact of the reform. Moreover, for assessing the parallel trends assumption, I will conduct an event study in which I additionally include three leads in equation 4. To further examine this assumption, I will estimate the impact of unilateral divorce on abuse committed by an unknown person, which should not be affected by the reform.

In section 5.3, I will also conduct a number of tests in order to rule out potential threats to the identification of the effect of interest. First, I will control for reforms other than unilateral divorce which were approved during my time span, as well as for additional state-level covariates, in order to mitigate concerns of omitted variables. Second, I will rerun equation 4 excluding one state at a time in order to check whether my results are sensitive to any state in particular. These two first exercises will, among other things, address the fact that Mexico City is usually considered to be a more gender-equal and progressive state compared to the rest of the country. This could be of concern if there are time-varying unobservable factors correlated with the approval of unilateral divorce and IPV. Third, I will use alternative definitions of the treatment and comparison groups. Fourth, I will examine the potential self-selection of women into reform states by looking at inter-state immigration. Fifth, I will exclude states that reformed within a year before the survey since the IPV measure refers to the twelve months prior to the survey and, thus, these states could be captured

¹⁸ Even when there are 10% of women who have had children after the reform, part of this is capturing women who were already pregnant when the reform was approved. Unfortunately, one cannot observe whether some couples have not had children as a response to the reform.

¹⁹ Divorce statistics provide two dates: the date when the divorce was filed, and the date when it was sentenced. I use the latter one.

Table 2
Effect of easier divorce on stock of divorced and married women.

	(1)	(2)	(3)	(4)	(5)	(6)
	Divorced		Divorced/Separated		Married	
Reform	0.005** (0.001) [0.001]	0.003** (0.001) [0.005]	0.013*** (0.004) [0.001]	0.010*** (0.003) [0.001]	−0.015 (0.008) [0.052]	−0.015** (0.006) [0.021]
Covariates	No	Yes	No	Yes	No	Yes
Mean	0.016	0.016	0.080	0.080	0.469	0.469
Number states	32	32	32	32	32	32
Observations	392,397	392,397	392,397	392,397	392,397	392,397

Notes: OLS estimates reported. Robust standard errors clustered by state of residence in parentheses. P values computed using the restricted wild cluster bootstrap in square brackets. Sample includes all women in ENDIREH. Dependent variable is an indicator of whether the woman is divorced (columns 1–2), divorced or separated (columns 3–4), or married (columns 5–6). ‘Mean’ refers to the average of the outcome in treatment states in the pre-reform period. Specifications 2, 4 and 6 control for woman’s age, indigenous background and educational attainment, presence of children, number of children, urban residence and a SES index. All specifications include state and year fixed effects. ***significant at 1% level, **at 5%, *at 10%. Source: 2006–2016 ENDIREH.

ing experiences of violence before the reform. Finally, I will analyse whether the findings could be driven by changes in reporting rather than prevalence.

One issue when estimating the long-term effect of unilateral divorce on IPV is that there are only two treated states, namely Mexico City and Hidalgo. As several recent papers point out, when only a few states are treated, inference based on wild cluster bootstrap might be invalid. In particular, the restricted wild cluster bootstrap is expected to result in under-rejection while the unrestricted one in over-rejection (MacKinnon and Webb, 2017, 2018; Roodman et al., 2019). Several extensions have been proposed. For instance, MacKinnon and Webb (2018) (see also MacKinnon and Webb (2017)) propose using subcluster wild bootstrap (clustering at an intermediate level such as state-year) and ordinary wild bootstrap (no cluster), as well as comparing both the restricted and unrestricted versions. I will come back to this when discussing the results.

5. Results

5.1. Effect of easier divorce on divorce and marriage

I start by analysing the impact of easier divorce on divorce rates (measured per 1000 inhabitants), as well as the proportion of women who are divorced. The first measure captures the flow of divorces, while the second one the stock of divorcees at a point in time.²⁰ Columns 1–2 of Table B.2 in the Appendix report the results for the first outcome. Focusing on column 1, the introduction of unilateral divorce increased the divorce rate by 0.24. Compared to the average divorce rate in reform states over the baseline period, this coefficient is sizeable—about 34%. Column 2 adds state-specific linear time trends, which increases the point estimate and significance.²¹ Looking at the wild cluster bootstrap p values (in square brackets), one reaches the same conclusions. Furthermore, an event study is depicted in the top

²⁰ The impact of easier divorce on divorce rates was one of the earliest studied outcomes in the divorce literature. Most of these studies have found a significant increase in divorce rates following a reduction in the cost of divorce—see Friedberg (1998) and Wolfers (2006) for a state-level analysis in the United States, and González and Viitanen (2009) and Kneip and Bauer (2009) for a cross-country analysis in Europe. Other outcomes studied include women’s labour supply (Bargain et al., 2012; Gray, 1998; Stevenson, 2008), children’s outcomes (Gruber, 2004), fertility decisions (Bellido and Marcén, 2014; Drewianka, 2008) and violent crime (Cáceres Delpiano and Giolito, 2012).

²¹ To calculate the state-specific linear time trends for reform states, I use data from the pre-treatment period only in order to avoid the problems described in Wolfers (2006), and then extrapolate this to the post-treatment period.

graph of Fig. C.3 as well as in columns 1–2 of Table B.3 in the Appendix. It shows that the effect of easier divorce on divorce rates seems to have been persistent throughout the post-reform period. It also shows that, before the reform, the coefficients are all close to zero and statistically insignificant, which suggests that there was no pre-existing increasing or decreasing trend in treatment, as compared to comparison, states before unilateral divorce.

These results are consistent with those reported in a recent paper analysing the impact of the same reform on divorce rates in Mexico (Hoehn-Velasco and Penglase, 2021). Unlike Hoehn-Velasco and Penglase, 2021, who consider unilateral divorce reforms as of 2017 and employ ten quarters of pre- and post-reform data, I use annual data from 1998 to 2016. The findings are also in line with previous studies in Europe and the United States, which report an increase in divorce rates at least during the decade following the reform (González and Viitanen, 2009; Wolfers, 2006).

Specifications 1–4 of Table 2 report the results from estimating the effect of unilateral divorce on the probability of being divorced (or divorced/separated) at the time of the survey. For analysing this question I pool the whole sample of women surveyed in ENDIREH. Columns 1–2 define the dependent variable as an indicator of whether the woman is divorced, while columns 3–4 as an indicator of whether she is either divorced or separated. After controlling for the covariates, column 2 reports an increase of 0.3 percentage points (or 19% of the sample mean in reform states in the pre-reform period) in the proportion of women who are divorced in treatment states relative to comparison ones after the reform.

Unilateral divorce may have also changed marriage rates.²² However, specifications 3–4 of Table B.2, which look at the effect of the reform on the number of marriages per 1000 people, do not show evidence for this. Columns 3–4 of Table B.3 and the middle graph of Fig. C.3 show that both before and after the reform all the coefficients were statistically indistinguishable from zero and small in magnitude.

Even when marriage rates seem to not have changed after the reform, the population ‘at risk’ (i.e. stock of married people) could have decreased due to the observed increase in divorce rates. Columns 5–6 of Table 2 examine this for the case of women using ENDIREH. The estimates show a decline of 1.5 percentage points (3% of the baseline sample mean) in the proportion of married women after the reform in treatment states compared to comparison ones. Given the potential

²² Marriage rates could have increased if the reduction in the cost of divorce encourages people to marry, whereas they could have decreased if the benefits from marriage have decreased. At the same time, the new marriages could be of worse or better quality which would eventually affect divorce (González and Viitanen, 2009; Rasul, 2006).

Table 3
Effect of easier divorce on IPV.

	(1)	(2)	(3)	(4)	(5)	(6)
	All			Married		
<i>Panel A: Static</i>						
Reform	0.003 (0.008) [0.729]	0.001 (0.007) [0.917]	0.000 (0.008) [0.964]	0.002 (0.008) [0.838]	0.001 (0.008) [0.861]	0.009 (0.007) [0.230]
<i>Panel B: Dynamic</i>						
Reform ^{k=1}	0.003 (0.008) [0.728]	0.001 (0.007) [0.920]	0.000 (0.008) [0.992]	0.001 (0.008) [0.920]	0.000 (0.008) [0.957]	0.008 (0.008) [0.334]
Reform ^{k=2}	0.035*** (0.006) [0.032]	0.037** (0.006) [0.027]	0.039*** (0.005) [0.025]	0.040*** (0.005) [0.031]	0.042*** (0.005) [0.027]	0.055 (0.038) [0.464]
Year 2003	No	No	No	Yes	Yes	Yes
Covariates	No	Yes	Yes	No	Yes	Yes
State × Time	No	No	No	No	No	Yes
Mean	0.173	0.173	0.174	0.176	0.176	0.176
Number states	32	32	32	32	32	32
Observations	191,639	191,639	170,763	186,584	186,584	186,584

Notes: OLS estimates reported. Robust standard errors clustered by state of residence in parentheses. P values computed using the restricted wild cluster bootstrap in square brackets. Columns 1–2 include women who were married or separated at the time of the reform regardless of their marital status at the time of the survey. Columns 3–6 include women who were married at the time of the reform and continue to be so at the time of the survey. Dependent variable is an indicator of whether the woman has experienced any IPV in the year before the survey. ‘Mean’ refers to the average prevalence of IPV in treatment states in the pre-reform period. ‘State × Time’ are state-specific linear time trends. Columns 2–3 and 5–6 control for woman’s age, indigenous background and educational attainment, presence of children, number of children, urban residence and SES index. Columns 5–6 in addition control for partner’s age, indigenous background and educational attainment, and length of the relationship. All specifications include state and year fixed effects. ***significant at 1% level, **at 5%, *at 10%. Source: 2006–2016 ENDIREH (columns 1–3), 2003–2016 ENDIREH (columns 4–6).

change in the population ‘at risk’ of divorce, columns 5–6 of [Tables B.2 and B.3](#) and the bottom graph of [Fig. C.3](#) consider the number of divorces per 1000 married people. Column 5 of the first table shows that, after the introduction of unilateral divorce, there is an increase of 0.86 divorces per 1000 married people in reform compared to non-reform states. This represents a change of 38% over the baseline mean. Adding state-specific linear time trends increases the estimate. Moreover, this effect has been persistent over the post-reform period. Comparing the point estimates in columns 5–6 to those in columns 1–2, the former are larger. This was expected since married people represent about 42% of the total population during my time span. The findings in columns 3–6 further corroborate those already reported in [HoeHN-Velasco and Penglase, 2021](#).

In sum, the results of this section show that unilateral divorce has increased both the divorce rate and the stock of women who are divorced. Regarding the marriage market, marriage rates do not seem to have changed after the reform, while the stock of married people has declined. Even when marriage rates have not changed, the quality of the new marriages could have done so. This is nevertheless not a concern in my analysis since, as mentioned in section 3 (Data), the sample is restricted to women who were married (or separated) at the time of the reform and, thus, explicitly removes women who marry after unilateral divorce is introduced.

5.2. Effect of easier divorce on IPV: baseline results

In this section I estimate the effect of easier divorce on the prevalence of IPV. Panel A of [Table 3](#) reports the DiD estimate associated with equation 1, while panel B the one associated with equation 2. The dependent variable is an indicator of whether the woman has experienced any type of IPV. Column 1 of panel A shows a statistically insignificant and close to zero effect of easier divorce on IPV. Panel B shows different results depending on the length of exposure to the reform. Women living in treatment states exposed to unilateral divorce

for less than six years have experienced, on average, a change in IPV statistically indistinguishable from zero as compared to those living in comparison states. In contrast, IPV has significantly increased in states exposed for 6–8 years—i.e., Mexico City and Hidalgo in 2016.

Column 1 does not control for covariates. Adding them (specification 2) barely changes the point estimates and standard errors, which suggests that the results are not driven by observed heterogeneity at the individual, couple or household levels. Moreover, it shows that, even when educational attainment and the number of children could be potentially affected by the reform, this does not represent any threat to the identification, as already suggested in section 3. Regarding the magnitude of the estimate in the long term, the increase has been of 3.7 percentage points, which represents about 21% of the pre-reform IPV mean in treatment states.^{23,24}

²³ The time reference used for defining IPV for divorced/separated women in 2006 is not fully comparable to that used in 2011–2016. In 2006, IPV refers to violence experienced since the couple split, whereas in 2011–2016 to violence experienced in the year before the survey (same definition than the one used for married women). This is unlikely to represent a concern since the proportion of women who split before the year prior to the survey is the same in both treatment and comparison groups (90%) in 2006.

²⁴ In 2011, I cannot disentangle whether a separated woman refers to a woman who is currently married but separated permanently (the relevant group for the analysis) or to a woman who was cohabiting before separating permanently. I can disentangle this for 2006 and 2016, when 55% of women who report being separated are still married. This number suggests that a non-negligible proportion of women should not be part of the sample in 2011. In this year, the sample of separated women represents 16% of the total, so these are the women who could potentially be affected by this data limitation. In [Table 3](#), I use the precise definition of separated women for 2006 and 2016. Using the same definition in these two years than that for 2011 (i.e. including all women who report being separated regardless of whether they are married) does not affect the conclusions, which suggests that this data limitation is unlikely to be of concern.

The results presented in columns 1–2 could be driven by couples who remain in the same marital status after the reform (either married or separated) or by those who transition (i.e. from married to separated or divorced, or from separated to divorced). I will further examine this in the mechanisms section (section 5.4). One can, however, expect that the findings will be driven by the sample of women who were married at the time of the reform and continue to be so at the time of the survey, since they represent 89% of the total. This is indeed what column 3, which restricts the sample to this group, suggests.

There are two advantages of looking at women in intact marriages, namely they were surveyed in 2003 and I can control for the partner's characteristics.²⁵ When I include the year 2003 and control for the partner's characteristics, the estimates essentially point in the same direction than those reported in columns 2–3 (see columns 4–5). More specifically, treatment states have experienced a statistically insignificant and close to zero effect in the short run, whereas a 4.2 percentage point (or 24% of the baseline sample mean) increase in IPV in the long run as compared to comparison states after divorce becomes easier.

The sample in specification 5 will be the one used to examine the validity of the DiD framework, since, as already mentioned, ENDI-REH survey does not capture information about divorced or separated women in 2003; a year that is needed to have at least two pre-treatment data points in all states. Moreover, given that the results in columns 2 and 3/5 are very similar in terms of the sign, statistical significance and magnitude of the estimates, restricting the sample to intact marriages is unlikely to lead to sample selection concerns, and it will be capturing most of the effect of easier divorce on IPV. After analysing the validity of the DiD approach, I will present all the results for both the overall sample (column 2) and that restricted to intact marriages (column 5). The tables will show very similar findings, which reduces any concern associated with sample selection and having only one pre-reform year in two states (Mexico City and Hidalgo).

In column 6 I add state-specific linear time trends. The results show point estimates larger in magnitude than those reported in column 5. For instance, 0.008 versus 0.000 in the short run, and 0.055 versus 0.042 in the long term. The magnitude in the long term is also economically significant representing a 31% increase with respect to the baseline sample mean (24% in column 5). The long-term estimate is, however, not precisely estimated and fails to reach statistical significance at conventional levels. Given that there are only a few (two-three) pre-treatment data points, there might not be enough information to accurately capture time trends, as pointed out in Wolfers (2006). Thus, one needs to interpret these results carefully. This is the reason why in the rest of the paper I take specification 5 as my preferred one.

An important issue regarding the long-term estimates is whether inference based on cluster-robust standard errors or wild cluster bootstrap yields valid conclusions. As mentioned in section 4, tests based on robust-cluster standard errors severely over-reject, while tests based on wild cluster bootstrap either under-reject (restricted) or over-reject (unrestricted) (MacKinnon and Webb, 2017, 2018; Roodman et al., 2019). In order to examine the sensitivity of my results to the fact that only two states are treated in the long term, I compute wild bootstrap p values following three different procedures, namely cluster (cluster by state), subcluster (cluster by state-year) and ordinary (no cluster). I then compare the restricted and unrestricted versions of each of them, such as done in Roodman et al. (2019) (section 8.3). When both restricted and unrestricted p values are similar, this provides support for the validity of the inference using that particular approach. As an illustration, I do this for column 5 of Table 3.

The p values associated with each bootstrap type are reported in Table B.4 in the Appendix. The last three columns focus on the long-term effect. The restricted wild cluster bootstrap p value (reported in the main table) is 0.027, whereas the unrestricted one is 0.000. The former is larger as the theory would predict. The difference between the restricted and unrestricted versions decreases when I use subcluster bootstrapping (0.008 vs 0.000). The ordinary wild bootstrap reports a p value of 0.000 in both versions, which provides credibility for these p values.²⁶ However, given the number of observations that I have, this last procedure is computationally very intense. Therefore, unless otherwise specified, in the rest of the paper I present the p values computed using the restricted wild cluster bootstrap. Since this method clearly under-rejects, any p value below 0.10 provides strong evidence that the estimate is statistically significant at conventional levels.

Before moving to the next section, Table 4 presents the results of the effect of easier divorce on each type of IPV. With the exception of sexual IPV, they show a similar pattern to that observed in Table 3. Physical, emotional and economic violence do not significantly change in the short term, whereas they significantly increase in the long run ranging the estimates from 0.012 (14% of the baseline sample mean in reform states) for economic abuse to 0.025 (23% of the sample mean) for emotional abuse. The results for sexual IPV show a statistically significant decline in the short run—about 14% of the sample mean. Table B.5 in the Appendix takes into account the binary nature of the dependent variable and presents the marginal effects from estimating a Probit model. The results confirm the OLS estimates reported in Table 4.

Overall, the results of this section are not in line with previous empirical findings in the context of developed countries (Brassiolo, 2016; Stevenson and Wolfers, 2006), which reported a decrease in IPV following easier divorce. In contrast, they are consistent with Hoehn-Velasco and Silverio-Murillo (2020) when examining the static effect of the reform.

5.3. Effect of easier divorce on IPV: robustness checks

5.3.1. Falsification tests

I first examine the validity of the parallel trends assumption by conducting two falsification tests. The first test is an event study. I take equation 2 as baseline, which already includes two lags, i.e., $k = 1$ and $k = 2$. I depart from this equation by adding three leads to $Reform_{st}^k$, which capture whether the reform will be introduced in 3 ($k = -2$), 2 ($k = -1$) or 1 ($k = 0$) survey wave(s). The excluded category is the survey year before the reform, i.e., $k = 0$. If $k = -2$ and $k = -1$ are statistically insignificant and small in magnitude, this would provide evidence in favor of the parallel trends assumption.

Fig. 2 presents the results graphically and Table B.6 in the Appendix reports the associated estimates and standard errors. As can be seen, all the coefficients that precede the divorce reform are statistically insignificant. One could be a bit concerned with the magnitude of the coefficient associated with $Reform^{k=-2}$ in column 1, namely 0.017. However, this is less than half of that for $Reform^{k=2}$ (0.043) and of the opposite sign, and it is mainly driven by emotional abuse.²⁷

The second falsification test is based on estimating the effect of the divorce reform on a type of violence against women different to IPV inflicted by an unknown person, which should not be affected by the reform. It refers to events experienced in several settings including

²⁵ I also exclude women who do not live with their partners temporally—e.g., because they have moved to the United States. The main reason is to make the sample comparable across years since these women were not surveyed in 2003. In addition, it allows me to control for the partner's characteristics.

²⁶ In the case of panel A and the short-term part of panel B, both the restricted and unrestricted versions of the three methods are very similar, as one would expect given that the number of treated states is 15.

²⁷ The prevalence of IPV in treatment and comparison groups by survey year can be found in Fig. C.4 in the Appendix. Unlike Fig. C.4, Fig. 2 explicitly focuses on the DiD estimates to highlight the pre-treatment trends and the short- and long-term effects of the reform.

Table 4
Effect of easier divorce on IPV – By type of IPV.

	(1)	(2)	(3)	(4)	(5)
	Any	Physical	Sexual	Emotional	Economic
<i>Panel A: Static</i>					
Reform	0.001 (0.007) [0.917]	0.003 (0.003) [0.468]	−0.006* (0.002) [0.028]	−0.002 (0.006) [0.798]	−0.003 (0.004) [0.475]
<i>Panel B: Dynamic</i>					
Reform ^{k=1}	0.001 (0.007) [0.920]	0.003 (0.003) [0.465]	−0.006* (0.002) [0.028]	−0.002 (0.006) [0.801]	−0.003 (0.004) [0.476]
Reform ^{k=2}	0.037*** (0.006) [0.027]	0.018*** (0.004) [0.075]	0.001 (0.002) [0.765]	0.025*** (0.006) [0.031]	0.012** (0.004) [0.027]
Mean	0.173	0.076	0.043	0.110	0.084
Number states	32	32	32	32	32
Observations	191,639	191,639	191,639	191,639	191,639

Notes: OLS estimates reported. Robust standard errors clustered by state of residence in parentheses. P values computed using the restricted wild cluster bootstrap in square brackets. Sample includes women who were married or separated at the time of the reform regardless of their marital status at the time of the survey. Dependent variable is an indicator of whether the woman has experienced (any, physical, sexual, emotional, economic) IPV in the year before the survey. ‘Mean’ refers to the average prevalence of IPV in treatment states in the pre-reform period. All specifications control for woman’s age, indigenous background and educational attainment, presence of children, number of children, urban residence, SES index, and state and year fixed effects. ***significant at 1% level, **at 5%, *at 10%. *Source:* 2006–2016 ENDIREH.

work, school, streets, public transport, parties and home.²⁸ I exclude violence inflicted by a known person because this could be correlated with a woman’s marital status, as well as IPV. Results are shown in Table B.7 in the Appendix. Column 1 reports the estimates for all women while column 2 those for women in intact marriages. Regardless of the sample, data are only available for 2006–2016, which explains the smaller number of observations in specification 2 compared to that reported in column 5 of Table 3. As expected, the DiD coefficients are all statistically insignificant and small in magnitude. The percentage points lie between −0.4 and 0.6, which represent 3–8% of the baseline mean in treatment states.

Overall, the results from these two falsification tests provide support for the parallel trends assumption. The first one indicates that the increase in IPV is not likely to be driven by an alternative phenomenon that predated the divorce change. Moreover, both tests suggest that changes in IPV do not seem to be driven by systematic unobservable differences between treatment and comparison groups.

5.3.2. Taking into account potential confounders

Even when Fig. 2 shows no significant difference in pre-reform trends in IPV between treatment and comparison states, there could still be time-varying unobservable factors that might confound the estimates of interest. I next discuss some of them.

A first potential confounder refers to the approval of the case law 1a./J.28/2015 (10a.), which was published in the Judicial Weekly of the Federation in July 2015. This case law states that any divorce law that requires to prove fault when there is no mutual consent is unconstitutional. Consequently, judges should not base the grant of a divorce on any fault. Given that this case law was approved at the national level, all states could potentially be affected by unilateral divorce. However, even when unilateral divorce could be granted based on it, the intensity of the treatment differs greatly which still leaves variation across states in 2015 and 2016. First, this case law does not regulate any other

aspect of the divorce including the economic compensation, which is still based on the existing divorce regime in the state or the discretion of the judge. Second, there could be lack of knowledge about the case law. Even if individuals could have anticipated the divorce reforms occurring from 2015,²⁹ the exact timing of their approval has been presumably unexpected.³⁰

To further examine the impact of this case law, Fig. C.5 in the Appendix shows the divorce rate by type of divorce in non-reform states. As one can see, most divorces occurring in 2015 and 2016 were still fault-based ones. This figure also shows that only in some non-reform states unilateral divorce increased in 2016 and to a lesser extent in 2015. This seems to be of particular concern for Campeche and Nuevo Leon. Excluding these two states, slightly raises the coefficients of interest—see column 2 of Table 5 and B.8 in the Appendix (column 1 reproduces the baseline estimates in columns 2 and 5 of Table 3, respectively). The first table restricts the sample to intact marriages while the second one includes all women who were married at the time of the reform regardless of their marital status at the time of the survey. Specification 3 goes a step further and excludes non-reform states where unilateral divorce represents more than 2% of all divorces in 2016. The estimates point in the same direction as those in column 1. The results hold if I instead use a more restrictive cutoff, namely 1%.

²⁹ Before the case law 1a./J. 28/2015 (10a.), anticipation effects are unlikely to be of concern. To examine this, I run a search on google to look for whether there were news related to unilateral divorce published in newspapers both before and after the reform in each state. In all cases, there were only news published after. The initiatives for reforming the law were, however, published in the corresponding legislative bodies before the reform was approved, but this is unlikely to have been accessed by most of the population. Even if it was, the outcome of these early initiatives was unclear because of the ‘pro-traditional family’ support of large part of the Mexican population, which is reflected in the composition of the Legislative Assembly. This is related to the importance of Catholicism in Mexico.

³⁰ The states that have reformed after July 2015 are spread all over the Mexican territory, thus there is no geographical cluster of reforming states. Moreover, as it has already been shown, divorce reforms do not seem to have been adopted following an upwards pre-existing IPV trend.

²⁸ The violent items refer to having been humiliated, ignored for being a woman, been physically assaulted, been touched without permission, and been forced to have sex.

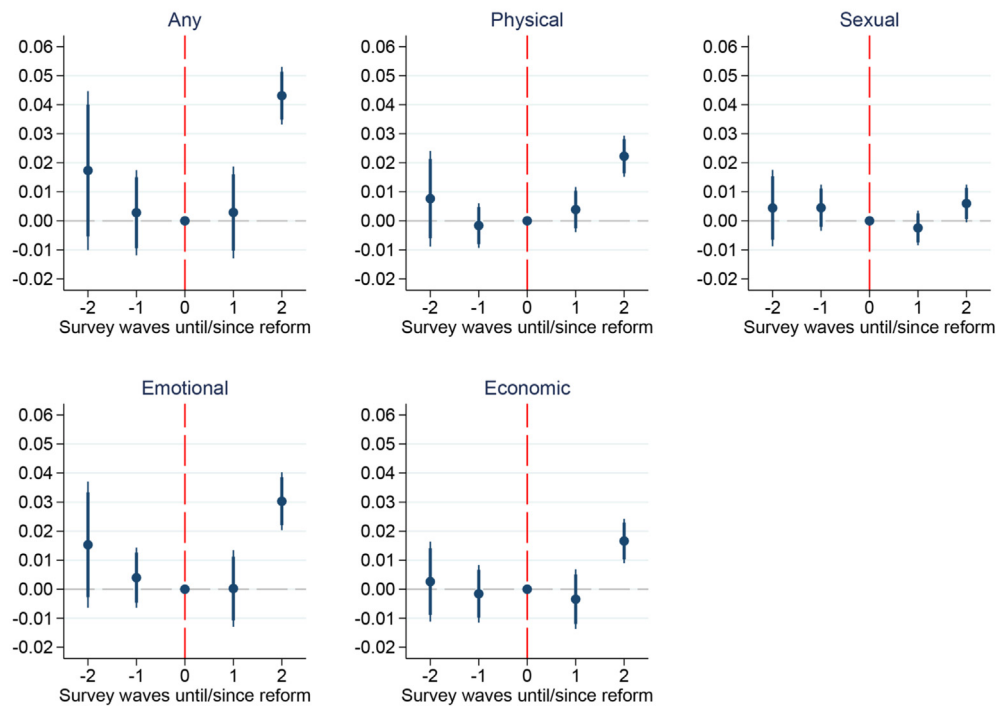


Fig. 2. Effect of easier divorce on IPV – Event study. *Notes:* The horizontal axis is the number of survey waves until/since the approval of the divorce reform. It corresponds to k in equation 2 where three leads to $Reform_{st}^k$ have been added. These leads capture whether the reform will be introduced in 3 ($k = -2$), 2 ($k = -1$) or 1 ($k = 0$) survey wave(s). $k = 1$ and $k = 2$ refer to whether the state has been exposed to the reform for one (i.e. less than six years, short term) or two (i.e. 6–8 years, long term) survey waves, respectively. The excluded category is the survey year before the reform (vertical dashed line). Dots are OLS estimates. Robust standard errors clustered by state of residence. Vertical solid lines are 90% and 95% confidence intervals. Sample includes women who were married at the time of the reform and continue to be so at the time of the survey. Dependent variable is an indicator of whether the woman has experienced (any, physical, sexual, emotional, economic) IPV in the year before the survey. All specifications control for both partners' age, indigenous background and educational attainment, length of the relationship, presence of children, number of children, urban residence, SES, and state and year fixed effects. *Source:* 2003–2016 ENDIREH.

A second potential confounder refers to the approval of domestic violence specific laws. In particular, during my time span, several states have introduced the divorce cause of domestic violence or criminalised domestic violence in the Penal Code. In a recent paper, [Beleche \(2019\)](#) finds a decrease in IPV in states that criminalised domestic violence as compared to those that did not. If the approval of these reforms is correlated with the approval of unilateral divorce the coefficients of interest would be biased. Columns 7 and 8 of [Table B.1](#) report the date of these two reforms, respectively. A quick look at the dates does not seem to suggest any correlation between the timing of the domestic violence reforms and the approval of unilateral divorce. I further address this concern by controlling for whether the state has introduced any of the two domestic violence reforms during my time span. Specification 4 reports the results. The point estimates are slightly smaller than those in column 1, but the conclusions do not change.

Column 5 refers to the decriminalisation of elective abortion in the first twelve weeks of pregnancy, which came into force in Mexico City in April 2007. This could have affected IPV in either direction.³¹ This type of abortion is performed free of charge at any of the health clinics and hospitals part of the Ministry of Health for women resident in Mexico City. Non-residents can abort for free at any of the health clinics, while in the hospitals they need to pay a sliding-scale fee, which depends on their SES. Statistics show that almost 97% of women abort-

ing between 2007 and 2017 were residents of either Mexico City or its neighbouring state Estado de Mexico. One way of controlling for the possible influence of this reform on the estimates of interest is to drop these two states. Removing them increases slightly the magnitude of the estimates but does not affect the conclusions.

The wild cluster bootstrap p value in column 5 is larger than 0.10 in the long term (0.328–0.379). As already mentioned, the restricted wild cluster bootstrap tends to under-reject when the number of treated clusters is small, and this is specially severe when it is one—given that Mexico City has been removed from the regression, only one state, Hidalgo, is treated in the long term. As in the case of [Table B.4](#), I compute the p value using cluster, subcluster and ordinary wild bootstrap for both the restricted and unrestricted versions. When I do this (as an example for women in intact marriages), the unrestricted version of the wild cluster bootstrap has a p value of 0.000, which is smaller than 0.379 (restricted) as predicted by the theory. In the case of the subcluster wild bootstrap, the p values are 0.134 (restricted) and 0.000 (unrestricted). The ordinary wild bootstrap p values are 0.018 and 0.017, which are very close, and are above 0.10.

In specification 6, I control for four time-varying state-level variables, namely the male's homicide rate, the male's unemployment rate, the gender gap in the labour force participation rate and the ruling party in the state, in order to further diminish concerns of omitted variables. I control for the homicide rate as a proxy for the general level of violence. Since 2008, some regions of the country have experienced a

³¹ On the one hand, easier abortion could reduce IPV through an improvement in women's bargaining power or a reduction in unwanted children. On the other hand, it could have increased IPV if it has led to an increase in conflict between partners over the number of children or other family planning issues.

Table 5
Effect of easier divorce on IPV – Potential confounders (Married).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline	Ex. NL/ Campeche	Ex. > 2% divorce	DV Reforms	Abortion	State Covariate	Covariate × Year
<i>Panel A: Static</i>							
Reform	0.001 (0.008) [0.861]	0.003 (0.008) [0.741]	0.001 (0.009) [0.937]	0.001 (0.008) [0.927]	0.006 (0.008) [0.526]	0.002 (0.008) [0.818]	0.001 (0.008) [0.893]
<i>Panel B: Dynamic</i>							
Reform ^{k=1}	0.000 (0.008) [0.957]	0.002 (0.008) [0.817]	0.001 (0.009) [0.885]	−0.000 (0.008) [0.983]	0.005 (0.008) [0.596]	0.001 (0.008) [0.885]	0.000 (0.008) [0.975]
Reform ^{k=2}	0.042*** (0.005) [0.027]	0.044*** (0.005) [0.022]	0.043*** (0.007) [0.018]	0.040*** (0.004) [0.021]	0.043*** (0.005) [0.379]	0.045*** (0.005) [0.019]	0.039*** (0.005) [0.027]
Num. states	32	30	23	32	30	32	32
Observations	186,584	173,228	132,101	186,584	176,366	186,584	186,584

Notes: OLS estimates reported. Robust standard errors clustered by state of residence in parentheses. P values computed using the restricted wild cluster bootstrap in square brackets. Sample includes women who were married at the time of the reform and continue to be so at the time of the survey. Column 1 reproduces the baseline estimates in column 5 of Table 3. Column 2 excludes Campeche and Nuevo Leon. Column 3 excludes states where unilateral divorce represents more than 2% of all divorces in 2016 (i.e. Campeche, Chiapas, Durango, Guanajuato, Michoacan, Nuevo Leon, Sonora, Tabasco and Veracruz). Column 4 controls for whether the state has modified the divorce and criminal legislations with regard to domestic violence. Column 5 excludes the states of Mexico City and Estado de Mexico in order to control for the decriminalisation of abortion in Mexico City. Column 6 controls for the male's homicide rate, male's unemployment rate, the gender gap in labour force participation rate and the ruling party in the state. Column 7 interacts the covariates with year fixed effects. 'NL' stands for Nuevo Leon, 'Ex.' for exclude and 'Num.' for number. Dependent variable is an indicator of whether the woman has experienced any IPV in the year before the survey. All specifications control for both partners' age, indigenous background and educational attainment, length of the relationship, presence of children, number of children, urban residence, SES index, and state and year fixed effects. ***significant at 1% level, **at 5%, *at 10%. *Source:* 2003–2016 ENDIREH; 2002–2016 Homicides, INEGI; 2002–2016 Population, CONAPO; 2002–2003 ENE; 2005–2016 ENOE; 2002–2016 Ruling party, electoral institute of each state.

large increase in violence.³² It could be that IPV is higher in these states due to a greater tolerance for it or higher stress levels. At the same time, individuals could have migrated to less violent states or to other countries (Orozco-Aleman and Gonzalez-Lozano, 2018), which would have affected the composition of the individuals living in treatment and comparison groups. I exclude female homicides, since these would include intimate partner homicides, which previous studies have found to be affected by a reduction in the cost of divorce (Stevenson and Wolfers, 2006).

The unemployment rate serves as a proxy for the macroeconomic trend in the state. Previous studies have found that gender-specific unemployment rates have an effect on IPV (Anderberg et al., 2016; Bhalotra et al., 2021). In addition, economic reasons might have also led to migration. As in the previous case, I only control for the male's unemployment rate. The gender gap in labour force participation rate is a proxy to account for differences in gender equality across states. This could be correlated not only with IPV but also with the approval of unilateral divorce if more 'female friendly' states did so earlier—a clear example is Mexico City, which was the first state to introduce unilateral divorce and usually considered to be one of the most progressive states in terms of gender equality. To further address this latter issue, I also control for the ruling party in the state. In Mexico, there are three major parties: Institutional Revolutionary Party (PRI), National Action Party (PAN) and Party of the Democratic Revolution (PRD). PRD is the most progressive party of the three. Column 6 shows that, after controlling for these four covariates, the estimates remain largely unaffected.

As a final robustness check, column 7 interacts the covariates in X_{ist} with year fixed effects. The objective is to purge the estimates of interest of differential time-varying changes in observable characteristics between treatment and comparison groups. As can be seen in the

table, adding these interactions slightly decreases the estimates, but the conclusions remain unchanged.

5.3.3. Alternative treatment groups

In this section I modify the definition of the treatment group. Table 6 and B.9 in the Appendix report the results. Column 2 restricts the treatment states to Mexico City and Hidalgo. This allows me to more accurately examine the short- and long-term effects since these are the only two states for which I can distinguish between the two periods. In panel B, the short-term coefficient is now negative, although it fails to reach statistical significance. The coefficient associated with the long term continuous to be positive and statistically significant although its magnitude decreases. In sum, models 1 and 2 lead to similar conclusions even when the magnitude of the coefficients varies to some extent. This suggests that, overall, the different findings in the short and long terms are not likely to be driven by the different composition of the treatment states. If anything, the difference between the two periods would be sharper if only Mexico City and Hidalgo were considered. To further examine the implications for inference of having only two states in the treatment group, I conduct the same p value analysis as before (as an example for women in intact marriages). The ordinary wild bootstrap is 0.748–0.748 in the static case, 0.653–0.654 in the short term and 0.040–0.041 in the long term (restricted-unrestricted).

Column 3 restricts the treatment group to all reform states other than Mexico City and Hidalgo. The results show a small and insignificant effect, which points in the same direction to the one reported in column 1.

Column 4 considers Campeche and Nuevo Leon, which seem to have been affected by the case law 1a./J.28/2015 (10a.), as part of the treatment group. In the sample of intact marriages, the estimates are slightly larger than those in column 1, which is probably explained by the fact that, by considering these two states as part of the comparison group, they were downwards biasing the estimates. Model 5 excludes states that reformed after the approval of the case law 1a./J.28/2015 (10a.). The reason for this test is that individuals living in these states are

³² Previous studies have found this to be closely related to the 'war on drugs' initiated by the former President Felipe Calderón (Calderon et al., 2015; Dell, 2015).

Table 6
Effect of easier divorce on IPV – Alternative treatment groups (Married).

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline	Only MC/Hidalgo	Ex. MC/Hidalgo	In. NL/Campeche	Ex. case law	Split short term
<i>Panel A: Static</i>						
Reform	0.001 (0.008) [0.861]	0.007 (0.014) [0.738]	0.003 (0.009) [0.789]	0.003 (0.007) [0.677]	0.006 (0.011) [0.646]	
<i>Panel B: Dynamic</i>						
Reform ^{k=1}	0.000 (0.008) [0.957]	−0.013 (0.018) [0.708]	0.003 (0.009) [0.789]	0.003 (0.007) [0.719]	0.003 (0.012) [0.842]	
Reform ^{k=2}	0.042*** (0.005) [0.027]	0.037*** (0.007) [0.209]		0.044*** (0.005) [0.013]	0.043*** (0.005) [0.032]	0.042*** (0.005) [0.021]
Reform ^{k=1} (1–2)						−0.003 (0.008) [0.736]
Reform ^{k=1} (3–5)						0.005 (0.015) [0.777]
Number states	32	19	30	32	25	32
Observations	186,584	111,385	175,911	186,584	147,301	186,584

Notes: OLS estimates reported. Robust standard errors clustered by state of residence in parentheses. P values computed using the restricted wild cluster bootstrap in square brackets. Sample includes women who were married at the time of the reform and continue to be so at the time of the survey. Column 1 reproduces the baseline estimates in column 5 of Table 3. Column 2 includes only Mexico City and Hidalgo in the treatment group. Column 3 includes all reform states other than Mexico City and Hidalgo in the treatment group. Column 4 considers Campeche and Nuevo Leon as part of the treatment group. Column 5 excludes reform states that approved unilateral divorce after the case law 1a./J.28/2015 (10a.) (i.e. Aguascalientes, Campeche, Colima, Nuevo Leon, Morelos, Puebla, Queretaro, Tamaulipas and Tlaxcala). Column 6 splits the short-term treatment effect into two groups based on whether the survey took place within two years after the state reformed ($Reform^{k=1}(1-2)$) or between three and five years ($Reform^{k=1}(3-5)$). ‘MC’ stands for Mexico City and ‘NL’ for Nuevo Leon. ‘Ex.’ stands for exclude and ‘In.’ for include. Dependent variable is an indicator of whether the woman has experienced any IPV in the year before the survey. All specifications control for both partners’ age, indigenous background and educational attainment, length of the relationship, presence of children, number of children, urban residence, SES index, and state and year fixed effects. ***significant at 1% level, **at 5%, *at 10%. Source: 2003–2016 ENDIREH.

likely to have anticipated the approval of unilateral divorce—although unlikely to have anticipated the timing of the reforms as commented above, which could have affected their behaviour. Removing these states leads to the same conclusions than those reached in column 1.

Finally, in specification 6 I split the variable $Reform_{st}^{k=1}$ into two groups based on whether the survey took place i) within two years after the state reformed ($Reform^{k=1}(1-2)$) or ii) between three and five years ($Reform^{k=1}(3-5)$). None of the coefficients associated with these two variables is statistically distinguishable from zero.

5.3.4. Additional tests

A limitation of the ENDIREH survey is that I only observe the state of residence of the woman at the time of the survey, which could have changed as a consequence of unilateral divorce. For instance, women might have moved to reform states in order to benefit from easy divorce given that spouses can divorce in the state where they set their residence, regardless of where they married. If these women are more (less) likely to experience abuse, the estimates will be upward (downward) biased. I examine whether this is the case by estimating the effect of the reform on the inter-state immigration rate of female and male aged 15 or older. Table B.10 in the Appendix reports the estimates. In all specifications, the coefficients of interest are statistically indistinguishable from zero and small in magnitude, which suggests that the reform has not led to migratory divorce.

A second test is related to the fact that the variable ‘length of the marriage’ is based on the question ‘How old were you when you married or started living with your partner?’. Thus, the age reported is not necessarily the age at marriage, but the age at which the couple started living together. Consequently, the computed length of the marriage is not precisely calculated. To address this, I restrict the sample to women

who were married for at least 10 years at the time of the reform (column 2 of Tables B.11 and B.12 in the Appendix), which leads to the same conclusions than those obtained from column 1.

A third issue is that, in some states, the survey was conducted within one year after the approval of unilateral divorce. Since the IPV measure refers to the twelve months before the survey, it might be capturing experiences of violence that could have occurred either before or after the reform. To present more accurate estimates of the effect of interest, I exclude those states that reformed less than a year before the survey. The results, which are reported in column 3, show estimates that are similar to those in column 1. Moreover, when looking at the ordinary wild bootstrap, the p value in the long term is 0.080 (restricted) and 0.079 (unrestricted).

When defining the dependent variable, I excluded one violent event, namely ‘has your partner stopped talking to you?’, since this question can be interpreted with a large degree of subjectivity. Column 4 reports the results from including it. As can be seen, the estimates barely change.

In specification 5, I use cohabiting women in treatment states as an alternative comparison group. The results point in the same direction than those reported in column 1, especially for intact marriages. These should be, however, interpreted carefully since, in some states, the economic compensation obtained upon the breakdown of a cohabiting relationship (as well as the length needed for considering a relationship cohabiting in terms of legal rights) has changed with the approval of unilateral divorce. Thus, cohabiting women might have been affected to some extent by the divorce reform.

Finally, I examine whether any particular state is driving the observed results. For doing so, I run 32 regressions excluding one state at a time. The results for women who continue to be married at the

time of the survey are presented in Fig. C.6 in the Appendix, while those for all women regardless of their marital status in Fig. C.7. In both cases, they show very similar point estimates across the 32 specifications and compared to the baseline ones. This analysis has three main implications. First, excluding Mexico City or Hidalgo does not affect the observed long-term impact of the reform. Second, excluding Mexico City shows that the results are not influenced by the fact that this state is usually considered to be more gender equal and progressive than the rest of the country. Third, excluding the states whose sample was not representative in 2003 one at a time (see footnote 11) does not influence the findings (this can be observed in Fig. C.6 which is the one including the year 2003). Moreover, C.7, which excludes the year 2003, reports very similar results.

5.3.5. Reporting behaviour

A final point to address, which is common to any study on IPV, is whether the observed changes in violence are due to changes in prevalence or reporting. On the one hand, it could be that after the reform women in treatment states are more likely to report IPV because they, for instance, feel more empowered, compared to when obtaining a divorce was more restrictive. If this was the case, the estimates in both the short and the long runs would be upwards biased. On the other hand, it could be that women are less likely to report IPV if, after the reform, they see divorce as a more likely option to end abuse than reporting it. In this case, the estimates would be downwards biased.

Although one cannot directly disentangle prevalence from reporting, I examine the potential direction of the bias by looking at the impact of unilateral divorce on a variable that captures women's reporting behaviour. In particular, ENDIREH survey provides information on whether the woman reports IPV to the police, Public Ministry or another authority. I construct an indicator of whether she reports it to any of these public authorities in the year before the survey.³³

A woman's reporting behaviour is only observed for abused women, which can introduce sample selection bias if the probability that men use violence is non-randomly distributed. For instance, men who think that their wives would report IPV upon abuse might be less likely to use it. To correct for this, I use a Heckman sample selection model for which I need a variable that affects the man's decision to use IPV, but not the woman's probability of reporting it directly. A candidate for this role is the man's experience of violence in his childhood. Previous literature has shown that men are more likely to inflict violence if they have experienced it in the past (Gover et al., 2008; Kwong et al., 2003; Pollak, 2004). At the same time, there is no reason for expecting this variable to be associated with the woman's reporting behaviour through any channel other than the IPV one. However, man's experience of violence during childhood has the problem of having a large number of missing values (29%). Given this, I also estimate the baseline regressions using this restricted sample, which provides very similar results to the ones reported in columns 2 and 5 of Table 3 (see Table B.13 in the Appendix).

Table B.14 in the Appendix presents the results. Columns 1 and 4 report the OLS estimates corresponding to an ordinary LPM model, while columns 2 and 5 take into account sample selection and report the maximum likelihood estimates associated with the Heckman model. In the four columns, the results are very similar and show that the probability of reporting IPV to the police, Public Ministry or any other authority has significantly declined in reform compared to non-reform states after the approval of unilateral divorce. As mentioned above, one possible explanation for the significant decrease in reporting could be

that, after the reform, women in the treatment group see divorce as a more likely option to end abuse than reporting it. Columns 3 and 6 report the Probit estimates associated with the selection equation. They show that a man's experience of violence in his childhood is positively associated with a woman's experience of IPV as an adult, which is statistically significant at the 1% level.

Taken together, the estimates in Table B.14 suggest that, if reporting of IPV in the survey has changed in the same way as the reporting of IPV to public authorities (i.e. women in treatment states are less likely to report it after unilateral divorce), the estimates in both the short and the long runs would be downwards biased and should be taken as a lower bound. Even if this was the case, the increase in IPV observed in the long term could not be explained by a differential propensity to report IPV between treatment and comparison groups after the reform.

Furthermore, concerns about reporting bias in general are likely to be minimised given the design and implementation of the survey, which closely follows the guidelines of the WHO on research on IPV. As mentioned in section 3, interviews took place in private and participants were guaranteed confidentiality. Moreover, the interviewers were all women and they had been trained to collect information about sensitive issues. In addition, the survey asks about a wide range of violent events, and when doing so it never uses the words 'violence' or 'abuse'. All this reduces concerns of under-reporting (Ellsberg et al., 2001; WHO, 2001).

In sum, although I cannot completely rule out that the reporting behaviour of women could have to some extent changed as a consequence of the reform, the discussion above suggests that at least partly the estimates are likely to be driven by changes in prevalence rather than in reporting. Moreover, the fact that the design and implementation of the survey follows the recommended guidelines by the WHO, reduces concerns of reporting bias in general.

5.4. Mechanisms

Throughout the paper I have shown robust evidence that unilateral divorce has led to a statistically significant and large in magnitude increase in IPV in the long term, whereas the effect in the short term seems to be small and statistically insignificant. These results are not in line with the classical prediction of divorce threat models outlined in the second paragraph of section 1 whereby IPV should decline following easier divorce. Yet divorce threat models make an interesting contribution by highlighting that the effect of easier divorce on IPV can work either through changes in marriages that remain intact after the reform or through changes in marriages that dissolve. I next examine the role of each of these two channels by considering the joint probability of IPV victimisation and (not) changing marital status between the reform and the survey.

Table 7 presents the results. Column 1 reproduces the baseline estimates from column 2 of Table 3. Specifications 2 and 3 refer to the joint probability of experiencing IPV and continuing to be in the same marital status after the reform, namely either staying married (column 2) or staying separated (column 3), respectively. The estimated coefficients in column 2 are very similar to those reported in column 1. Moreover, the mean value of IPV in treatment states in the pre-reform period is also very similar. In column 3, the estimates are small in magnitude and statistically insignificant when looking at the cluster-robust standard errors. When considering the wild cluster bootstrap p value, one can reject the null hypothesis that the coefficient in the short term is equal to zero at the 10% level, but overall the mean of the outcome is very small.

Specification 4 looks at the joint outcome of being a victim of IPV and changing marital status from married to separated/divorced or from separated to divorced. The estimates report a statistically significant decrease in the short term (30% of the baseline mean in treatment states). This decline could mean that now the divorce process is less likely to trigger violence.

³³ This variable is only asked for women who report either physical or sexual violence in 2003 and 2006, so for comparison reasons I use the same definition in 2011 and 2016. This is likely to be capturing most of the reporting cases following IPV since women who only experience emotional or economic abuse are in general unlikely to report it.

Table 7
Effect of easier divorce on joint probability of IPV and marital status.

	(1)	(2)	(3)	(4)
	IPV	IPV & Stay married	IPV & Stay separated	IPV & Change MS
<i>Panel A: Static</i>				
Reform	0.001 (0.007) [0.917]	0.001 (0.007) [0.896]	0.002 (0.001) [0.070]	−0.003* (0.001) [0.023]
<i>Panel B: Dynamic</i>				
Reform ^{k=1}	0.001 (0.007) [0.920]	0.001 (0.007) [0.897]	0.002 (0.001) [0.070]	−0.003* (0.001) [0.023]
Reform ^{k=2}	0.037*** (0.006) [0.027]	0.036*** (0.005) [0.018]	−0.001 (0.001) [0.215]	0.002 (0.002) [0.572]
Mean	0.173	0.154	0.009	0.010
Number states	32	32	32	32
Observations	191,639	191,639	191,639	191,639

Notes: OLS estimates reported. Robust standard errors clustered by state of residence in parentheses. P values computed using the restricted wild cluster bootstrap in square brackets. Sample includes women who were married or separated at the time of the reform regardless of their marital status at the time of the survey. Column 1 reproduces the baseline estimates in column 2 of Table 3. The outcomes in columns 2–4 refer to joint probabilities of IPV victimisation and continuing to be married (column 2), continuing to be separated (column 3), and changing marital status (column 4) between the reform and the survey. ‘Mean’ refers to the average of the outcome in treatment states in the pre-reform period. ‘MS’ stands for marital status. All specifications control for woman’s age, indigenous background and educational attainment, presence of children, number of children, urban residence, SES index, and state and year fixed effects. ***significant at 1% level, **at 5%, *at 10%. Source: 2006–2016 ENDIREH.

Taken together, the results in Table 7 show that the overall effect of unilateral divorce on IPV is driven by women who remain married after the reform, which is presumably related to the fact that they represent 89% of the sample, as already highlighted in section 5.2. This is in line with the findings in Brassiolo (2016). Given that women in intact marriages are driving the results, the rest of this section discusses the potential channels explaining the findings for this group of women.

Intact marriages are those that ‘survive’ the divorce reform. Thus, they were likely of better quality at the time of the reform than those that dissolve.³⁴ Over time, however, the quality of intact marriages could have changed, as well as the attitudes and behaviours of the partners. For instance, conflict might arise between partners, which could make them think of divorcing. This might in turn trigger more conflict. The thought of divorce could also come together with changes in labour market participation or IPV attitudes, which might be associated with violence. The reform could have caused or exacerbated any of these changes, but they might not have necessarily occurred immediately following easier divorce. This could explain the insignificant impact in the short run while the significant increase in the long run. I am particularly going to examine two potential mechanisms. In the first one, IPV is used as an instrument to prevent women from leaving the marriage. In the second case, IPV is related to changes in variables that could challenge men’s preferences and/or culturally prescribed gender roles, and which have been previously linked with IPV (e.g. labour market outcomes, IPV attitudes).

Regarding the first mechanism, there is qualitative evidence pointing to men using IPV when women try to leave the relationship. Indeed, this seems to be one of the riskiest moments for women (Brennan, 2017;

Fernández Teruelo, 2013). It might nevertheless be that they do not think of living or try to leave the relationship immediately following the law change but only after a few years, which can be related to the dynamics of the marriage. This could explain the insignificant effect in the short term. In addition, even if they think of leaving or try to do so immediately after the reform, IPV is unlikely to be the only way of preventing the dissolution of the marriage. There are presumably non-violent tools that help to achieve the same purpose, such as reallocating resources towards the woman, threatening to sue her, or persuading her with affectionate behaviour. These non-violent tools might be sufficient to prevent divorce in the short term which would explain the insignificant effect.³⁵ In the long term, men might need to resort to IPV in order to achieve their goal. Whether the man uses IPV to prevent his partner from leaving the marriage would presumably depend on whether he expects her to leave, e.g., if she has better outside options. Following this argument, women with better outside options would be the ones holding the bulk of the increase in IPV.

A proxy for capturing outside options is a woman’s educational attainment. Thus, in order to further examine the proposed channel, I next analyse whether the effect of easier divorce on IPV varies by a woman’s educational attainment. For doing so, I interact the treatment with a variable with three categories, namely having completed less than primary education; having completed primary, secondary or A levels, or having attended technical vocational training (with completed secondary); and having attended technical vocational training

³⁴ One way of inspecting the quality of the marriage at the time of the reform is to compare the prevalence of IPV in the treatment group in the pre-reform period between women who stay in intact marriages and those who split. The prevalence of violence is 0.17 in the first group, while 0.35 in the second. This shows that women who split experienced a higher rate of IPV before the reform and thus they were more likely to be in low quality marriages compared to those who stay.

³⁵ Anderberg and Rainer (2013) develop a model in which economic abuse is used to align the woman with her partner’s preferences, which are to devote her time to household chores instead of working for pay. They show that, when the market relative wage is low enough, there is no need to use economic abuse since income transfers would be enough to align her with his preferences. In my case, the preference of the husband is not to keep the woman out of the labour force but to keep her in the relationship.

(with completed A levels), undergraduate or graduate studies.³⁶

Table 8 reports the estimated impact of easier divorce on IPV for each of the three categories of education. Column 1 includes women who remain married after the reform, whereas column 2 all women who were married at the time of the reform regardless of their marital status at the time of the survey. I focus on specification 1 but the results are very similar to those reported in column 2, as I have been finding throughout the previous sections. The estimates suggest heterogeneous effects across levels of education. Women in the bottom of the education distribution have experienced a statistically insignificant change in IPV in both the short and the long runs. In contrast, women who have completed at least primary education (i.e. the other two groups) show a pattern that is similar to the one reported in the baseline regressions. Moreover, the difference between these two latter groups is not statistically distinguishable from zero.³⁷

In short, the results in Table 8 provide support for the ‘divorce prevention’ mechanism. The findings suggest that the threat of divorce of women in the bottom of the education distribution is likely to be lower than that of women with better outside options.³⁸ Although inflicting IPV on women in the middle and top of the education distribution might sound counterintuitive *a priori* given that these women are presumably more likely to divorce upon IPV, this would not necessarily be the case. It is widely known that abuse tends to follow a cyclical nature which makes it difficult for women to leave the relationship permanently, regardless of their level of education. Moreover, many of them do not associate their experiences with violence and, among those who do, they usually only decide to leave the relationship definitely after years of experiencing it (Aizer and Dal Bó, 2009; Walker, 1979). Indeed, in my sample of abused women, 54% are not thinking or do not want to divorce as a consequence of these events.³⁹

A second potential explanation for the IPV findings refers to the fact that the reform might have changed either directly or indirectly variables that have been previously linked with IPV. If changes in these variables or their consequences do not occur immediately after the

Table 8
Effect of easier divorce on IPV – Heterogeneous effects by women’s education.

	(1)	(2)
	Married	All
<i>Panel A: Static</i>		
Reform (low)	−0.009 (0.011) [0.435]	−0.012 (0.010) [0.250]
Reform (middle)	0.006 (0.009) [0.530]	0.005 (0.008) [0.550]
Reform (high)	−0.005 (0.008) [0.574]	−0.002 (0.008) [0.787]
<i>Panel B: Dynamic</i>		
Reform ^{k=1} (low)	−0.010 (0.012) [0.407]	−0.012 (0.010) [0.242]
Reform ^{k=2} (low)	0.009 (0.009) [0.407]	0.013 (0.009) [0.178]
Reform ^{k=1} (middle)	0.005 (0.009) [0.613]	0.005 (0.008) [0.548]
Reform ^{k=2} (middle)	0.054*** (0.005) [0.028]	0.043*** (0.007) [0.043]
Reform ^{k=1} (high)	−0.005 (0.008) [0.558]	−0.002 (0.008) [0.834]
Reform ^{k=2} (high)	0.033 (0.018) [0.047]	0.038* (0.019) [0.029]
Number states	32	32
Observations	186,584	191,639

Notes: OLS estimates reported. Robust standard errors clustered by state of residence in parentheses. P values computed using the restricted wild cluster bootstrap in square brackets. Sample includes women who were married at the time of the reform and continue to be so at the time of the survey (column 1) and women who were married or separated at the time of the reform regardless of their marital status at the time of the survey (column 2). ‘Low’ refers to having completed less than primary education; ‘middle’ to having completed primary, secondary or A levels, or having attended technical vocational training (with completed secondary); and ‘high’ to having attended technical vocational training (with completed A levels), undergraduate or graduate studies. Dependent variable is an indicator of whether the woman has experienced any IPV in the year before the survey. All specifications control for woman’s age, indigenous background and educational attainment, presence of children, number of children, urban residence, SES index, and state and year fixed effects. Column 1 also controls for partner’s age, indigenous background and educational attainment, and length of the relationship. ***significant at 1% level, **at 5%, *at 10%. Source: 2003–2016 ENDIREH (column 1), 2006–2016 ENDIREH (column 2).

³⁶ To be more specific, this variable takes the four-categories variable used throughout the paper and groups the second and third groups together. The percentage of women in each category is 22%, 60% and 18%, respectively.

³⁷ Heterogeneous effects by a woman’s educational attainment was also examined by Brassiolo (2016), who finds the decreasing effect of easier divorce on IPV to be concentrated on women in the bottom and middle of the education distribution. In addition, the author looks at heterogeneity by the presence of children below the age of 18 in the household. When doing this latter exercise, I find results very similar to the baseline ones regardless of whether the woman has children below the age of 18 (see Table B.15 in the Appendix). Moreover, the percentage of women with children below this age is 58% in my sample. I have also looked at whether there are heterogeneous effects by a man’s experience of violence during childhood. This can be considered a proxy for whether he learned to use IPV in everyday situations or whether he learned that violence is the only way to impose his preferences. In the first case, we should not observe any change in IPV for the ‘violent’ types of men. In the second case, violence could already increase in the short term or the increase could be larger in the long one. The results of this analysis, which are reported in Table B.16 in the Appendix, do not provide in general support for heterogeneity by ‘types’ of men.

³⁸ To further examine this, I look at the percentage of women who are divorced over the total sample of women in ENDIREH in each of the three educational groups in 2006. For women with low education this is 0.7%, for those with middle education 1.2%, and for the ones with high education 2.4%. These differences are statistically significant at conventional levels. I also look at the proportion of abused women who mention that they are thinking of divorcing as a consequence of IPV in 2006. 19% of women with low education are thinking of divorcing. This number is 26% and 25% for those in the middle and top of the education distribution, and the difference with respect to the other group is statistically significant.

³⁹ The main reasons that abused women give for not divorcing are ‘the problems are not that serious’ (31%), and ‘a wife has to overcome difficulties and be together with her partner’ (17%).

reform, this could also explain the different results in the short and long terms.

The data available in ENDIREH allows me to examine whether labour market outcomes or IPV attitudes have changed due to the reform. Table B.17 in the Appendix reports the results (details of the construction of the variables are in the Appendix). Overall, there is no evidence of a significant change in the probability that women work for pay or in the wage of working women in either the short or the long terms. In the case of IPV attitudes, I use two proxies. The first

one refers to whether the woman rejects IPV (information available in 2003–2011), while the second one to whether she is thinking of divorcing as a consequence of it (information available in 2006–2016). In both cases, the estimates are statistically indistinguishable from zero. In sum, there is no evidence that labour market outcomes or IPV attitudes have changed for women in treatment states compared to those in comparison ones following the reform. Thus, changes in these variables are unlikely to be explaining the observed pattern in IPV.

Overall, this section provides suggestive evidence that the results for intact marriages are consistent with an explanation whereby IPV is used to prevent women from leaving the marriage. The difference between the short and the long terms could be explained by the fact that women do not consider leaving the marriage immediately following easier divorce or that men use non-violent tools to align their partner's behaviour with their preferences in the short term.

6. Conclusion

This paper has provided empirical evidence of the relationship between divorce laws and IPV in the context of a developing country. It has specifically estimated the causal effect of reducing the cost of divorce on male-to-female IPV by exploiting the state-level variation in the timing of the introduction of unilateral divorce in Mexico.

My results have shown robust evidence that, in the long term, unilateral divorce has led to a statistically significant and large in magnitude increase in IPV (3.7 percentage points or 21% of the baseline sample mean). In contrast, the impact has been statistically insignificant in the short term. These results are driven by women who continue in the marriage after the reform. I also provide evidence that they are consistent with IPV being used as an instrument to prevent women from leaving the marriage.

The findings of this paper highlight the importance of analysing the relationship between divorce laws and IPV in the context of a developing country by showing different intra-household dynamics than those observed in developed countries. My results particularly suggest that laws aimed at making the dissolution of marriage easier would not necessarily decrease IPV in a developing setting, but can lead to the opposite outcome. One way of reducing this adverse effect is to complement easier divorce with interventions that challenge individuals' perceptions of marriage as well as intra-household relationships between spouses. Furthermore, the findings of this paper are of particular importance in the current context of Mexico, where states are gradually introducing unilateral divorce, as well as for other countries considering such measures.

Declaration of competing interest

None.

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Appendix A. Supplementary data

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