Does Unilateral Divorce Impact Women's Labor Supply? Evidence from Mexico

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From 2008 to 2018, Mexican states introduced unilateral no-fault divorce. Using state-level variation in the timing and adoption of these divorce laws, we study how the legislation affected married women's labor supply. Our results suggest that married women did not increase their labor force participation. Among employed married women, hours worked increased, but the effect is not large enough to be observed in the full sample of women. We find suggestive evidence that social norms against female labor supply and lack of access to formal work may explain the limited labor supply response. Our results highlight the importance of the cultural context in studying the consequences of divorce legislation.

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1 Introduction

Divorce is common in high-income countries (OECD, 2014), and a large body of research has examined the consequences of more liberal divorce laws in these contexts. Less is known about divorce in low- and middle-income countries. Social norms limit the prevalence of divorce, and the lack of a social safety net makes divorce a riskier proposition, especially for women.

In this paper, we study the welfare effects of divorce in Mexico. At varying points between 2008 and 2018, all 32 Mexican states established unilateral, no-fault divorce laws. These laws made divorce more accessible, as one spouse could divorce their partner without their partner's consent or the need to prove cause. Past work in Mexico has demonstrated that these laws resulted in higher divorce rates (Hoehn-Velasco and Penglase, Forthcoming). As a result, the threat to divorce one's spouse is now more credible and may alter how couples interact with one another. Specifically, the partner with the improved outside option stands to benefit (McElroy and Horney, 1981). In this study, we examine how these laws affect the labor supply of couples who *remain married* after states passed more liberal divorce laws.

The relationship between divorce laws and labor supply is likely to depend on the cultural context. Gender norms against women working vary considerably across countries (see Section 3), and these norms may impact how women respond to unilateral divorce. In countries without gender norms against women working outside the household, we would expect that any divorce law that harms women's outside option to result in greater labor supply; women are worse off and therefore work more as leisure is a normal good. This mechanism is described formally by Chiappori et al. (2002) in the context of the United States. On the other hand, in countries where there is social stigma to women working, we may expect these laws to result in a decline in women's labor supply, as husbands can now more easily constrain their wife's employment. The relationship between gender norms, bargaining power, and labor supply is studied by Field et al. (2019) and Heath and Tan (2020) in the context of India. The United States and India differ considerably in regards to gender norms, and *a priori*, it is not obvious where Mexico falls in this spectrum.

We, therefore, begin our analysis by examining how cultural values in Mexico compare to other countries. Using the World Values Survey, we present evidence that Mexico has less gender equality than more developed countries, such as the United States and Spain, but is more equal than other Latin American countries, such as Colombia. We then present a simple model that demonstrates that the degree to which women's labor supply increases (or decreases) in response to the introduction of no-fault divorce will depend on the importance of gender norms against women working. The model generates testable predictions that we investigate empirically.

To measure changes in married women's labor supply, we use household-level, quarterly panel data from *The National Occupation and Employment Survey* (ENOE). The panel structure of

the data allows us to compare women's labor supply before the change in legislation, to their labor supply immediately after. Our results suggest that married women did not increase their employment levels or hours worked following the reform. However, *employed* married women did increase their weekly hours worked by one half of an hour. We next examine how the results differ by the marital property regime, as past work has shown that the state-level asset division upon divorce is important for interpreting the consequences of unilateral divorce laws (Gray, 1998; Voena, 2015). We find that the increase in female labor supply was largest in "separate property" states, where marital assets are not all treated as jointly owned.[1]

Overall, our labor supply results differ slightly from existing work on divorce laws in developed countries (Stevenson, 2008; Bargain et al., 2012), as we do not find a large increase in female employment. This difference suggests that the setting matters, and we provide several explanations for why our findings differ. First, the lack of an effect is potentially due to cultural norms against women working, which restricts their ability to adjust their labor supply. This hypothesis is consistent with hours worked increasing only among women who are already employed. Second, there may be cultural norms against divorce, which suggests that divorce is not necessarily a credible threat, and therefore unlikely to impact the behavior of married couples. Finally, previous work by Gray (1998) and Voena (2015) has established that the marital property regime is an important factor in understanding the impact of divorce on labor supply. In our context, the marital property regime is not observable (we are forced to use the state-level default option as a proxy). While our asset division measure allows us to identify some heterogeneity by property regime, this may be insufficient to fully disentangle the relationship.

An alternative explanation for the limited labor supply response is that the unilateral divorce laws did not affect women's bargaining power within the marriage. We test this directly using the collective household framework of (Chiappori, 1988, 1992; Apps and Rees, 1988), which models couples as a pair of individuals who bargain over goods, and reach a Pareto efficient allocation. Using this framework, we infer bargaining power by identifying resource shares, defined as the share of the total household budget controlled by each spouse. To accomplish this, we follow Dunbar et al. (2013) and identify resource shares using Engel curves for goods that are consumed exclusively by either men or women.[2] We find a small decline in women's bargaining power originating from the introduction of unilateral divorce, consistent with what we would expect. Models of intra-household decision making suggest that a decline in women's bargaining power should result in greater labor supply (Browning et al., 2011). Given that we do not observe an increase in labor supply, these results support the importance of gender norms in constraining women's employment.

This paper makes several contributions to the literature. First, we add to the ongoing dis-

[1] In Mexico, individual couples decide how assets are divided should they divorce. Since we do not observe this, we rely on variation in the state-level default option to proxy for the marital property regime. We discuss this further in Section 6.

[2] This methodology and similar approaches have been employed in a variety of contexts (Calvi, Forthcoming; Calvi et al., 2017; Tommasi, 2019; Sokullu and Valente, 2018; Brown et al., 2018; Penglase, 2020).

cussion on the employment effects of divorce laws (Parkman, 1992; Gray, 1998; Bremmer and Kesselring, 2004; Genadek et al., 2007; Stevenson, 2008; Bargain et al., 2012; Hassani-Nezhad and Sjögren, 2014). The findings of these studies appear to be context-dependent and not generalizable to a middle-income context. Our second contribution is then to emphasize cultural norms when examining the relationship between divorce laws and labor supply. We illustrate this both by comparing gender attitudes in Mexico to other countries and by presenting a simple model to explain how gender norms may factor into our analysis.

The remainder of this study is organized as follows. In Section 2, we summarize the existing literature in more detail. Section 3 discusses the cultural context and the introduction of unilateral divorce in Mexico. Section 4 examines the relationship between gender norms, divorce laws, and labor supply. In Section 5 we summarize the ENOE survey and the state-level changes in divorce laws. In Section 6, we discuss the empirical strategy. Section 7 presents the labor supply findings, Section 8 shows the heterogeneous results by property regime and marital status, and Section 9 performs robustness checks on the labor supply results. Section 10 concludes.

2 Literature Review

Unilateral divorce laws have become increasingly prevalent in high-income countries in recent years. These laws have increased divorce rates (over the short-run) in a variety of different contexts, including the United States (Friedberg, 1998; Wolfers, 2006), European countries (González and Viitanen, 2009; Kneip and Bauer, 2009), and more recently in Mexico (Hoehn-Velasco and Penglase, Forthcoming). How do more liberal divorce laws affect couples who remain married? Given that divorce becomes a more credible threat with the introduction of unilateral divorce, these laws benefit the spouse who places a higher value on exiting the marriage. This shift in marital bargaining power from one spouse to the other will then affect household behavior. An extensive literature has analyzed this hypothesis along several different dimensions, including labor supply, savings decisions, and children's investments. Our study primarily adds to the literature studying the relationship between unilateral divorce laws and labor supply.

A collection of studies (Peters, 1986; Parkman, 1992; Genadek et al., 2007; Stevenson, 2008; Bargain et al., 2012) find that the introduction of unilateral divorce laws increase women's labor supply. These results have been attributed to women wanting to insure themselves against divorce. Gray (1998) studies the consequences of unilateral divorce laws in the United States, and somewhat counter-intuitively, attributes changes in women's labor supply to *increased* women's bargaining power. Gray (1998) reaches this conclusion using variation in state-level marital property laws to show that women increased their labor supply and reduced household production in states with property regimes favoring women. By contrast, Chiappori et al. (2002) interpret higher labor supply as a decrease in bargaining power due to the decline in leisure (a normal good). An alternative explanation, discussed in Stevenson (2007) and

Stevenson (2008), hypothesizes that changes in women's labor supply are due to decreased investments in marriage-specific capital, such as household production. Roff (2017) finds evidence supporting this hypothesis as both men's and women's household work declined in the US as a result of the introduction of unilateral divorce laws.

Our study's primary contribution is in examining the effect of unilateral divorce on labor supply in a middle-income country.[3] Following Field et al. (2019) and Heath and Tan (2020), we provide a conceptual framework to understand the relationship between bargaining power, labor supply, and gender norms to illustrate why Mexico may differ from previously studied contexts. We therefore provide an explanation for why our results differ from the existing literature which finds an increase in female employment following the introduction of unilateral divorce. More broadly, we contribute to recent work on the importance of social stigma in limiting female labor supply (see Jayachandran (2020) for an overview). Research on gender norms and female labor supply in Mexico is somewhat limited. Notable exceptions include Talamas (2020), who studies the importance of gender norms for childcare in limiting women's entry into the labor market, and Aguilar et al. (2017) who highlight the importance of safety in public transportation in Mexico City on female labor supply.

Our work also relates to research studying the effect of divorce laws on marital bargaining power. Identifying changes in bargaining power due to divorce laws is challenging as marital bargaining power is unobservable. The inner workings of the household are generally a black box due to data limitations. Nonetheless, Chiappori et al. (2002) provides a framework to study this relationship by building divorce laws into the collective labor supply model from Chiappori (1988, 1992), and Apps and Rees (1988). Voena (2015) extends Chiappori et al. (2002) to a dynamic setting and incorporates asset accumulation and marriage into a life-cycle structural model.[4] We deviate from this literature by using consumption data to test for changes in bargaining power.[5]

3 The Mexican Context

Obtaining a divorce in Mexico has traditionally been an arduous process. The divorcing spouse had to prove cause (e.g., domestic violence or infidelity), or both spouses needed to mutually consent to the divorce. Legal reforms in the 1990s and early 2000s relaxed some of the hurdles (Lew and Beleche, 2008; Beleche, 2019), but it was not until 2008 when couples were able to divorce without legal grounds. Mexico City was the first region to implement no-fault unilateral divorce, and every other state followed suit over the subsequent ten years.

Compared to other countries that have adopted unilateral divorce laws, Mexico is cul-

[3] Existing work on divorce in Mexico has emphasized the importance of the cultural context in the effect of divorce laws on both domestic violence and divorce rates. See, for example, Lew and Beleche (2008); Garcıa-Ramos (2017); Hoehn-Velasco and Silverio-Murillo (2020); Hoehn-Velasco and Penglase (Forthcoming).

[4] See Fernández and Wong (2014) and Eckstein et al. (2019) for other work in this area.

[5] Parallel work by Silverio-Murillo (2019) examines the effect of these laws on women's bargaining power within the household using subjective questions on which spouse makes decisions.

Figure I: *World Values Survey* and Cultural Differences between Countries

NOTES: Figure I reports how important individuals in each country view whether the wife should earn less than the husband, the acceptance of divorce, and views on gender equality in the workplace. All three measures are calculated on a scale from 0 to 1.

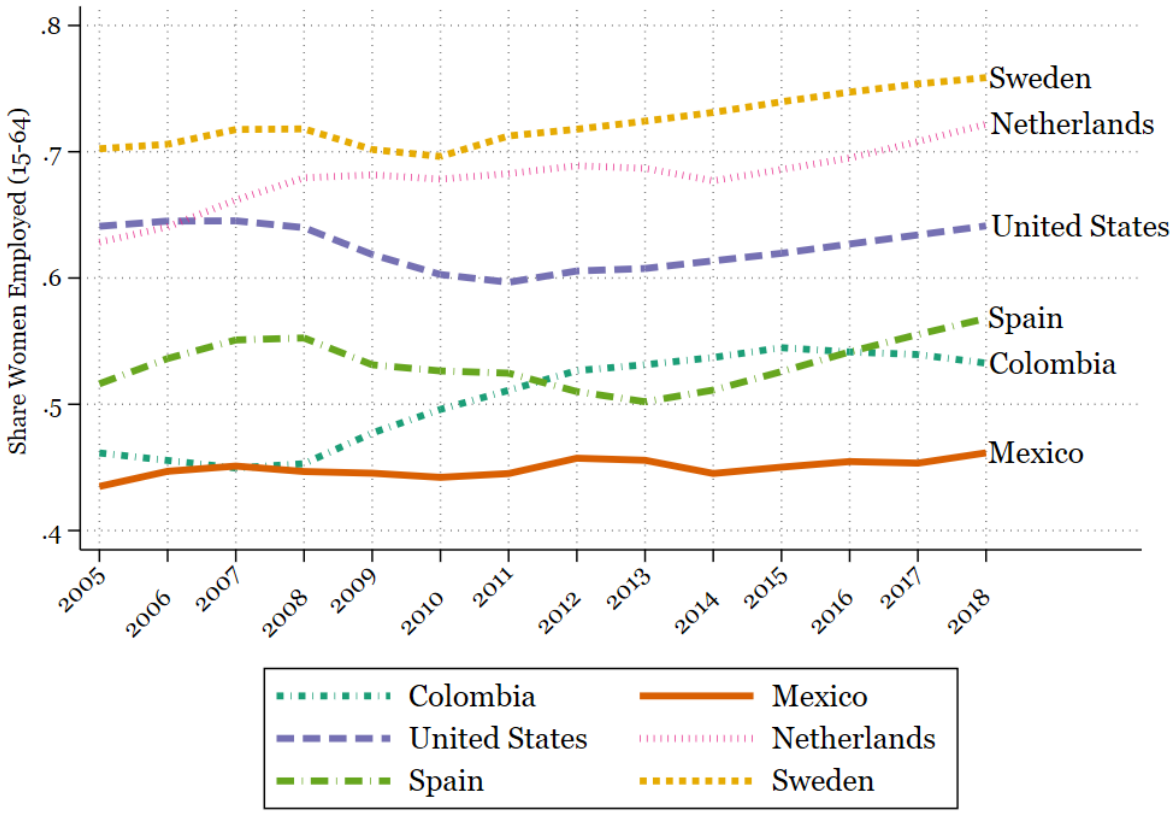
SOURCE: World Values Survey, Wave 6 (2010-2014).

turally more conservative. To illustrate these differences, we use the most recent wave of the *World Values Survey* to compare perspectives on divorce and gender equality in the workplace. Figure I presents societal attitudes towards gender equality and divorce in two representative Latin American countries, three European countries, and the United States.[6] The figure shows how respondents viewed the acceptability of divorce, whether the wife should earn less than her husband, and whether gender equality in the workplace is important. Of the countries presented, Colombia and Mexico are the least receptive to divorce and have the lowest view of equality in the workforce. Nearly half of the respondents in Colombia and Mexico think that women should earn less than their husbands. At the other extreme, Spain, Sweden, and the Netherlands view divorce as acceptable and place a high value on gender equality. In the United States, fewer individuals believe that a wife must earn less than her husband, and the U.S. places a higher emphasis on gender equality.

Figure I demonstrates that Mexico, and Latin America more generally, differ in fundamental ways from the previously studied contexts. Based on these differences, the results from Europe and the United States cannot necessarily be generalized. We expect the findings

[6]Not every country is included in the World Values Survey, and our choice of European countries is therefore driven primarily by data availability.

Figure II: Share of Women Employed, 15-64



NOTES: Figure reports the share of women employed relative to the total population of women who are between 15 and 64.
SOURCE: OECD labor force and population statistics.

in Mexico to differ in two main ways. First, due to cultural beliefs on divorce acceptability, Mexican couples that remain married may be systematically different from U.S. and European households. This claim is supported by Garcıa-Ramos (2017), Hoehn-Velasco and Silverio-Murillo (2020), and Silverio-Murillo (2019) who all find that women who experience domestic violence are not necessarily able to escape abusive relationships following the introduction of unilateral divorce. These cultural differences are relevant because they suggest divorce may be less of a credible threat in Mexico than in countries that have previously been studied. And if divorce is not a credible threat, the impact of unilateral divorce laws on marital behavior may be minimal.

Second, gender norms may constrain women from entering the labor market. As discussed in Goldin (1994), women in middle-income countries are significantly less likely to work than women in poorer and wealthier countries, and cultural norms against women working are a likely cause. This is particularly true in Mexico, even relative to other Latin American countries (Arceo-Gomez and Campos-Vazquez, 2010; Novta and Wong, 2017; Bustelo et al., 2019). Figure II compares women's labor supply across Mexico and the remaining countries shown in Figure I. Figure II shows that less than 50% of Mexican women participate in the labor force, which is lower than in Colombia. Based on the cultural difference in Mexico, we hypothesize that women prefer to increase their labor supply in the presence of unilateral di-

vorce laws but may be unable to do so. In Section 4, we provide a model to illustrate these constraints.

4 Conceptual Framework

We motivate our empirical analysis by presenting a simple model to examine the relationship between divorce laws, female labor supply, and bargaining power. Standard results in the collective household literature suggest that any law that decreases female bargaining power will result in less leisure (and therefore greater labor supply), as leisure is a normal good (Browning et al., 2011). However, recent work in India (Field et al. (2019) and Heath and Tan (2020)) has demonstrated that this result does not necessarily apply to settings where there are gender norms against market work for women. Following our discussion in Section 3, we therefore set out a collective household model that allows for gender norms against female labor supply.[7]

Our goal is to highlight the importance of social stigma in limiting the response of women's labor supply to the introduction of no-fault unilateral divorce. If we observe women working more when divorce laws are unfavorable, that will illustrate that gender norms against working are not a meaningful constraint. On the other hand, if we observe a decrease (or no change) in labor supply, social stigma may matter when examining the impact of divorce laws on female labor supply. Gender norms against women's labor supply operate on a spectrum; they could be highly constraining, as has been found in India, or they could be less pertinent, as in western contexts where female labor supply is high. Based on our discussion in Section 3, Mexico likely falls between these two alternatives.

4.1 Model Setup

The model builds upon Field et al. (2019) and Heath and Tan (2020) who examine the relationship between bargaining power, gender norms, and labor supply. We present a simple collective household model with the goal of establishing testable predictions. Our model abstracts from household production, altruism, and consumption sharing, as the goal is to highlight the importance of gender norms in studying the consequences of divorce laws.

Suppose the household consists of a married man and woman, indexed by $j \in m, f$. Each spouse derives utility from consumption c_j and leisure l_j. Spouses also derive a utility cost γ_j from the wife working. The utility cost could potentially operate through domestic violence, or more generally, through increased conflict within the household. The dis-utility from working is not required to originate solely from the husband's displeasure; for example, social stigma may worsen the workplace atmosphere faced by women. The level of γ_j will vary across

[7] In the collective household literature, households are modeled as a collection of individuals with their own distinct preferences who bargain over goods and reach a Pareto efficient allocation (Chiappori, 1988, 1992).

Mexican states, across marriages, and may also vary across types of employment. For example, we may think that women who live in states where female labor force participation is high face less resistance to entering the labor market than those who do not. Similarly, we may see women with children facing greater resistance from their husbands to entering the labor force due to gender expectations regarding childcare. Finally, social stigma may be different for work in the formal sector relative to informal work for a household enterprise.

For simplicity, we assume that all components of utility are separable and that the utility costs from the wife working increase linearly in hours worked. Moreover, we assume that each spouse divides their time between market work and leisure. Let h_j denote spouse j's labor supply, with $h_j + l_j = 1$, with $h_j \geq 0$. Then, following a standard collective model, the household maximizes a weighted sum of individual utilities:

$$\begin{aligned} \max_{c_f, c_m, h_f, h_m} \; & \mu(z)[u_f(c_f, 1 - h_f) - \gamma_f h_f] \\ & + (1 - \mu(z))[u_m(c_m, 1 - h_m) - \gamma_m h_f] \\ & \text{s.t. } p_f c_f + p_m x_m \leq w_f h_f + w_m h_m + y \end{aligned} \tag{1}$$

where y is non-labor income and $\mu(z)$ are the Pareto weights, which are functions of distribution factors z, as well as prices and incomes which we omit for conciseness.[8] In our context, the divorce regime in which the couple resides will be a distribution factor (Chiappori et al., 2002). The key modification of the household problem relative to the standard collective setup is the dis-utility each spouse may receive from the wife working due to gender norms, given by γ_j. If γ_m or γ_f is greater than zero, then there are gender norms against the wife working. If both γ_m and γ_f are zero, then the household's problem reduces to the original framework of Chiappori (1992).

In order for the model to satisfy separability, we follow Field et al. (2019) and rewrite the wife's and husband's utilities as $u_f(c_f, 1 - h_f) - \gamma_f h_f - \frac{1-\mu}{\mu}\gamma_m h_f$, and $u_m(c_m, 1 - h_m)$, respectively. With Pareto efficiency, we can use standard duality theorems to redefine the household's problem as a two-stage process; in the first stage, household resources are optimally allocated across spouses, and in the second stage, each spouse maximizes their utility function subject to their share of the household budget. The wife's maximization problem is given below:

$$\begin{aligned} \max_{c_f, h_f} \; & u_f(c_f, 1 - h_f) - \gamma_f h_f - \frac{1 - \mu(z)}{\mu(z)} \gamma_m h_f \\ & \text{s.t. } p_f c_f \leq w_f h_f + \phi(z) \end{aligned} \tag{2}$$

where $\phi(z)$ denotes the net transfer from her husband. Any change in women's bargaining power will have two competing effects on the wife's labor supply. First, a decline in bargaining power will lower the net transfer she receives from her husband (i.e., her share of household income). Given that leisure is a normal good, this change incentivizes her to *increase* her labor supply. Second, the decline in her bargaining power increases the weight she places on

[8] Distribution factors are variables that affect the bargaining outcome but not individual preferences.

her husband's dis-utility from her working. If the husband's dis-utility is sufficiently large, the decline in bargaining power may *decrease* her labor supply. Thus, which effect dominates depends on the restrictiveness of norms against women working. We state these predictions more formally below:

Prediction 1: With low gender norms against women working, a divorce law that lowers women's bargaining power will increase married women's labor supply.

Proof. See Appendix Section B.1 □

Prediction 2: With restrictive gender norms against women working, i.e., if γ_m is sufficiently large, a divorce law that lowers women's bargaining power will result in a decrease in married women's labor supply.

Proof. See Appendix Section B.1 □

A priori, it is not apparent which case is more applicable to the Mexican context. Our discussion in Section 3 suggests that there is a social stigma against women working in Mexico, however, it is not clear whether it does to such a degree to limit female labor supply. When are norms likely to be the most restrictive? We expect these norms to be more restrictive, 1.) among women who live in states with low female labor force participation, 2.) for formal work relative to informal work, and 3.) among women who have children relative to those without. Our empirical analysis in Section 7 provides evidence regarding these hypotheses.

Social stigma is not the only reason women may not enter the labor force in response to a decline in their bargaining power. Insufficient labor demand would also limit women's ability to enter the labor force. For example, it is conceivable that unilateral divorce laws decrease women's reservation wage, but not sufficiently low to push them into the labor market if there are no acceptable wage offers. While we do not incorporate this mechanism into the model, it is a potential factor. We, therefore, investigate this empirically as well in Section 7.

Finally, the above framework takes as given that we know how divorce laws will affect women's bargaining power. Specifically, we assume that women are likely to be harmed by legislation that facilitates divorce. This relationship is likely to hold unless the marital property regime is favorable towards them. When assets are jointly owned, as in a "community-property" regime, the wife's standing within the marriage may improve when unilateral divorce is legalized. In contrast, women will be worse off in "separate-property" regimes. We think this is a reasonable assumption and consistent with past work (Gray, 1998; Voena, 2015). Nonetheless, we provide a test of this assumption building upon Dunbar et al. (2013) in Section B.2 of the Appendix and find some supportive evidence.

5 Data

5.1 The National Occupation and Employment Survey

We use individual employment records from the *National Occupation and Employment Survey* (*Encuesta Nacional de Ocupación y Empleo* or ENOE). The ENOE is Mexico's official labor force survey and reports quarterly information on household employment and time use. The data are a representative sample of the 32 Mexican states and is the largest continuous data project in Mexico.

Table 1 presents summary statistics. We restrict the sample to married women aged 22 to 65 who are interviewed both before and after the reforms. In the sample, 41 percent of married women are employed, with 17 percent working in the formal sector and 25 percent in the informal sector.[9] Married women work 14 hours per week, on average. Among employed women, weekly hours worked increases to 34 hours per week. Married women also spend a significant amount of time on household production, as they devote 28 hours per week to chores and other work around the house. They spend an additional 9 hours per week caring for their children, family members who are ill, or the elderly.[10] The average woman in our sample has three children and is age 43.

One of the primary advantages of the ENOE data is that it follows households for five consecutive quarters. The ENOE's panel structure allows us to measure employment status for all household members for up to one year before and after the reform. Each quarter, one-fifth of the sample is replaced with an incoming cohort. We use this short panel to observe how individuals respond to the change in legislation. We compare each woman's labor supply pre- and post-reform using this panel of information on employment and hours worked.

While the panel structure of the ENOE is beneficial, there are several limitations to the data. First, the survey only follows individuals for five quarters, which prevents us from using this data to study the long-run labor supply dynamics. However, there is no *a priori* reason to suspect the effect should vary over time. To account for this limitation, we conduct a similar analysis using the full sample of the ENOE and consider state-level effects. In this specification, we use state fixed effects and state-specific time trends rather than individual-level trends. The results in Table A10 reflect the baseline specification with individual-level trends and fixed effects.

A second concern is attrition due to changes in household structure that occur during the study as individuals may exit the sample if they become divorced or separated. If the couple

[9] We use ENOE's definition of informal work. Mexico has specific laws that classify formal work as salaried and informal work as non-salaried workers (Levy, 2010). Formal workers have protections against layoffs and firings, while informal work is not directly illegal, workers who work for informal firms are not technically employees (Levy, 2010).

[10] Time use categories are based upon ENOE definitions. Individuals with missing values for all time use categories are dropped from the sample. Otherwise, we code missing values as zeros.

Table 1: Summary Statistics for Married Women

	Observations	Mean	Median	St. Dev.
Employment				
1(Working)	204,476	0.41	0.00	0.49
1(Informal)	204,476	0.25	0.00	0.43
1(Formal)	204,476	0.17	0.00	0.37
Hours				
Working	204,476	14.21	0.00	20.71
Working (Employed)	90,404	34.39	36.00	18.53
Informal Work (Employed)	49,862	30.86	30.00	19.75
Formal Work (Employed)	40,542	39.59	40.00	15.15
Household Production	200,955	27.76	28.00	12.11
Care for Children/Elderly/Sick	200,905	9.41	0.00	13.91
Demographics				
Age	204,476	43.12	43.00	11.04
1(Urban)	204,476	0.43	0.00	0.49
Number of Children	204,466	3.01	3.00	1.93
Age of Youngest Child	122,640	8.47	8.00	5.39

SOURCE: Individual-level data from the National Occupation and Employment Survey (ENOE) for the years 2007 Q1 through 2019 Q2.

divorces, either the husband or the wife will leave the data (or both will). This attrition has drawbacks in estimating the exact effect of the reform on labor supply. In an ideal world, we would identify women married before the reform and track them afterward, regardless of their marital status. Since we cannot fully do this, we expect a slight downward bias in the estimates. To examine whether households exit the sample in predictable ways, Table A3 presents the probability of leaving the sample based on the reform, as well as other individual characteristics. The results in Columns (1) to (3) suggest that individuals are no more likely to leave the panel after the introduction of unilateral divorce. Women with fewer children and women who are younger are most likely to leave the sample. The results are similar when looking at the probability of attrition, specifically due to divorce (Columns (4) to (6)). Despite little evidence of the reform affecting attrition, we take several additional precautions in our robustness checks. First, we restrict the sample to individuals in the survey all five periods. Second, we adjust the sample weights for attrition. The results are unchanged when using either adjustment (see Table A9 in the Appendix).

5.2 Divorce Legislation Data

To measure the timing of the reform, we collect the quarter-year passage of unilateral no-fault divorce from the state civil and family laws.[11] We also collect the observed (*de facto*)

[11] We frequently rely on popular press articles surrounding the reform to measure the precise dates that the law passed. We corroborate our findings with the reform dates provided in Mendez-Sanchez (2014), Silverio-Murillo

quarter-year date of the unilateral reform. This observed date records when states began to allow unilateral divorce "in practice" and is based on data reported in the *Instituto Nacional de Estadística y Geografía* (INEGI) divorce statistics. Table A1 shows the year that the legislation passed (*de jure*), when states began to allow unilateral divorce from the INEGI data (*de facto*), and the location of the divorce legislation in the state's legal code.

A notable issue with the divorce reforms is that the state's legal (*de jure*) dates of the reform often differ from the state's observed (*de facto*) dates (based on the INEGI data). These differences are highlighted by separating *de jure* years from *de facto* years in Table A1.[12] Comparing across *de facto* and *de jure* years, there are clear discrepancies between when a state passed the reform and when an individual could exercise the right to a no-fault divorce (in practice). The blue text indicates when there is a mismatch by year. There are 13 states that mismatch years. An additional seven states are off by one quarter (but match in terms of years). A further seven states match in terms of years, but the dates differ by more than one quarter. Only four states exactly match between *de facto* and *de jure* dates. For our main analysis, we rely on the observed *de facto* dates rather than the *de jure* legal reforms. We default to the observed *de facto* dates because we are most interested in when states allowed individuals to obtain a unilateral divorce, not when the states codified the legislation. See Hoehn-Velasco and Penglase (Forthcoming) for a more detailed discussion on this legislation.

6 Empirical Strategy

To study the effect of unilateral divorce laws on labor supply, we exploit state-level variation in the legislation's quarter-year adoption. We restrict the sample to women who were surveyed both before and after the introduction of unilateral divorce. This restriction allows us to measure labor supply changes in the year leading up to, and following the date of implementation.

We follow Voena (2015) and estimate the labor supply of married woman i in state s during quarter t as:

$$Y_{ist} = \alpha + \beta(\text{Uni}_s \times \text{Post}_t) + \mathbf{X}_{it}'\gamma + \pi_s + \tau_T + \alpha_i + \phi_i t + \epsilon_{ist} \tag{3}$$

where Y_{ist} is a measure of labor supply. The interaction $\text{Uni}_s \times \text{Post}_t$ indicates state-level adoption of the reform in at time t. The vector of individual characteristics is given by X_{ist} and includes indicators for age and the number of children. We include state and quarter-year fixed effects, given by π_s and τ_T, respectively. We also include individual fixed effects α_i, which capture time-invariant individual characteristics. In the full specification, we include individual time trends $\phi_i t$, which account for any linear increase or decrease in labor supply over time. This inclusion allows us to control for, e.g., any gradual increase in labor supply due to children aging. Since individual outcomes may be correlated over time, we cluster standard

(2019), and García-Ramos (2017), who also study no-fault divorce in Mexico.

[12] In the *de jure* column, states that had not passed unilateral divorce as of 2017 have blank years.

errors at the individual level (Bertrand et al., 2004a).

Potential Threats to Validity. There are several potential threats to the validity of our specification. First, there is a high degree of heterogeneity in historic state-level divorce laws. For example, Garcıa-Ramos (2017) notes that Baja California, Chiapas, and Quintana Roo revised their divorce codes as late as 2004. One might worry about disentangling the long-run effect of these reforms from the short-run effect of subsequent unilateral divorce laws. However, as these states introduced unilateral divorce beginning 2014 or later, we do not expect these earlier reforms to influence either the pre-reform or post-reform period considered. Moreover, our baseline specification includes state, individual, and year fixed effects, which help to alleviate any concerns regarding time-invariant differences in norms and legal standards by state.

Second, the timing of the state-level adoption of unilateral divorce may not be exogenous. We, therefore, examine whether the timing of divorce laws is predicted by prior levels of state-level labor supply. We test whether women's labor supply in 2007Q1, before the adoption of any unilateral divorce law, predicts the state-level timing of unilateral divorce. We use several measures of labor supply, including an indicator for employment, hours worked, and hours worked conditional on employment for married, single, and all women in the first year of our sample (2007Q1). Table A4 displays the results. Across all 12 columns, the quarter-year passage of the reform is indeed unrelated to any measure of labor supply.

The final assumption underlying this specification is that women are changing their labor supply only in response to the reform. If women systematically increase their labor supply over time, this pattern will inflate the employment estimates. For example, women may mechanically increase their labor supply due to finishing school (and entering the labor force), or re-entering the labor force as their children enter primary school. We address this issue with individual-level time trends that capture any linear changes in individual labor supply that are not directly due to the reform. We also restrict the sample to women age 22 and over to avoid capturing the effect of women leaving school.

7 Labor Supply Results

Main Results. Table 2 presents the main results from Equation 3. Columns (1) to (3) measure changes in employment, where employment is an indicator that equals zero if the woman is not working and one if she is. Columns (4) to (6) present the findings for weekly hours worked. Across both labor supply measures, married women do not appear to be changing their employment status or their hours worked.

The lack of any change in labor supply differs from previous results in high-income countries. Recall that Bargain et al. (2012) finds that women increased their labor force participation in response to the legalization of divorce in Ireland, and this increase occurred mostly at the extensive margin. Similarly, Stevenson (2008) estimated an increase in labor force participa-

Table 2: Unilateral Divorce Reform and Married Women's Labor Supply

	1(Working)			Hours Worked			Hours Worked (Employed)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Uni x Post	-0.0023	-0.0023	0.0006	0.1420	0.1380	0.2370	0.5165**	0.5245**	0.6310**
	(0.0038)	(0.0038)	(0.0045)	(0.1521)	(0.1521)	(0.1781)	(0.2502)	(0.2499)	(0.3211)
N	204,157	204,157	204,157	204,157	204,157	204,157	85,083	85,083	85,083
Adj R-sq	0.592	0.592	0.613	0.619	0.619	0.642	0.524	0.524	0.560
Mean Dep	0.413	0.413	0.413	14.212	14.212	14.212	34.392	34.392	34.392
State and Quarter-Year FE	X	X	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X	X	X
Age and Child FE		X	X		X	X		X	X
Individual Trend			X			X			X

NOTES: OLS coefficients reported. The sample includes all married individuals who are 22 to 65. The panel of individuals includes those who were married and experienced both the unilateral reform and the pre-reform period. Baseline fixed effects include state fixed effects, quarter-year fixed effects, individual fixed effects, indicators for the number of children, and age indicators. Robust standard errors are clustered at the individual level and are reported in parentheses. ***, **, * represent statistical significance at 1, 5 and 10 percent levels.
SOURCE: Individual-level data from the National Occupation and Employment Survey (ENOE) for the years 2007 Q1 through 2019 Q2.

tion in the United States as a result of the introduction of unilateral divorce. The difference between the results presented in Table 2 and the existing literature suggests that the findings in high-income settings may be context-dependent.

There are several explanations for why women in Mexico fail to change their employment. First, as discussed in Sections 3 and 4, working women may experience stigma surrounding employment due to gender norms against female labor supply. Second, there may be insufficient labor demand or other labor market frictions that limit women's ability to adjust their labor supply in the formal sector. We next explore these hypotheses in Table 2 and Table 3.

First, women who are *already working* may be less constrained by either gender norms or demand-side frictions. To test whether this is the case, we restrict the sample to employed women, and measure changes in hours worked among these women. These results are presented in Columns (7) to (9) of Table 2. The estimates suggest that employed women increased their hours worked by half an hour following the reform. This finding is robust to the inclusion of individual time trends in Column (9). Given that these women were already working, this finding provides support for Prediction 1, which suggests that women who do not face restrictive gender norms (or who face less stigma) are likely to increase their labor supply.

We further investigate the role of gender norms in Table 3 by examining the importance of state-level attitudes towards female employment, as proxied by state-level female labor force participation rates. In Columns (1) to (3), we restrict the sample to states with less restrictive gender norms against female employment ("High Female Employment"). In Columns (4) to (6), we restrict the sample to states with more restrictive gender norms ("Low Female Employment"). The cutoff occurs at 44.3 percent.[13] Again, we find modest evidence in support of the importance of gender norms. In states with higher female employment levels, among

[13] Alternative thresholds did not impact the results.

Table 3: Evidence for the Main Mechanism

Panel A: Social Stigma

	High Female Employment			Low Female Employment		
	(1)	(2)	(3)	(4)	(5)	(6)
	1(Working)	Hours Worked	Hours Worked (Employed)	1(Working)	Hours Worked	Hours Worked (Employed)
Uni x Post	-0.0048	0.2279	0.7899*	0.0039	0.1998	0.2663
	(0.0063)	(0.2538)	(0.4305)	(0.0078)	(0.2918)	(0.5735)
N	112,575	112,575	50,717	91,582	91,582	34,366
Adj R-sq	0.611	0.643	0.557	0.612	0.640	0.561
Mean Dep	0.447	15.132	33.835	0.382	13.357	35.001
State and Quarter-Year FE	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X
Age and Child FE	X	X	X	X	X	X
Individual Trend	X	X	X	X	X	X

Panel B: Access to Labor Markets

	High Informal			High Formal		
	(1)	(2)	(3)	(4)	(5)	(6)
	1(Working)	Hours Worked	Hours Worked (Employed)	1(Working)	Hours Worked	Hours Worked (Employed)
Uni x Post	-0.0013	0.5394*	1.1312**	-0.0064	0.0369	0.4376
	(0.0081)	(0.3189)	(0.5558)	(0.0061)	(0.2485)	(0.4289)
N	89,788	89,788	39,921	101,353	101,353	43,623
Adj R-sq	0.582	0.632	0.566	0.645	0.668	0.552
Mean Dep	0.435	14.438	33.189	0.437	15.000	34.303
State and Quarter-Year FE	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X
Age and Child FE	X	X	X	X	X	X
Individual Trend	X	X	X	X	X	X

NOTES: OLS coefficients reported. The sample includes all married individuals who are 22 to 65. The panel of individuals includes those who were married and experienced both the unilateral reform and the pre-reform period. Baseline fixed effects include state fixed effects, quarter-year fixed effects, individual fixed effects, indicators for the number of children, and age indicators. Robust standard errors are clustered at the individual level and are reported in parentheses. ***, **, * represent statistical significance at 1, 5 and 10 percent levels.
SOURCE: Individual-level data from the National Occupation and Employment Survey (ENOE) for the years 2007 Q1 through 2019 Q2.

employed women, those previously employed increased their hours worked. In states with low female employment, there is no significant effect.

We also show the results over high formal employment states (over 19.9%) and high informal female employment states (over 24.2%). Here the findings imply that the primary increase in hours worked occurs in states with high informal labor force participation. This finding suggests that the increase in hours worked is concentrated within states where more women participate in the informal section. Combined, these findings provide evidence that social norms may play a role in suppressing the general effect of women's labor force participation and that women may have limited access to the formal sector.

Next, we turn to several alternative measures of labor supply in Table 4. In Columns (1)

and (2), we attempt to parse whether there are differential effects by type of work. Women in Mexico may disproportionately choose informal work if labor market opportunities for women are scarce. The distinction is especially relevant for the middle-income setting, where women often lack access to formal labor markets. Furthermore, gender norms against formal work may be greater than those for informal work. In terms of labor force participation in these sectors, there is no difference after the reform. On the intensive margin, however, we do see that women increase their hours worked in the informal sector, but not the formal one. This finding corroborates the importance of the state-level informal sector demonstrated in Table 3. Put together, we find modest evidence that labor supply constraints may be driving differences in our context, which can help explain the divergence from the existing literature.

Table 4: Additional Specifications for Married Women's Labor Supply

	1(Informal Work)	1(Formal Work)	Informal Hours	Formal Hours	Time Children Elderly	Time Household Production	Hours Children (Working)	Hours Household (Working)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Uni x Post	-0.0027	0.0033	1.0062*	0.1306	-0.2001	0.1053	-0.1645	0.3187
	(0.0046)	(0.0025)	(0.5448)	(0.3992)	(0.1529)	(0.1488)	(0.2263)	(0.2394)
N	204,157	204,157	43,520	37,857	200,520	200,571	83,292	83,319
Adj R-sq	0.477	0.766	0.574	0.477	0.439	0.261	0.454	0.266
Mean Dep	0.246	0.167	30.861	39.599	9.407	27.763	7.373	22.820
State and Quarter-Year FE	X	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X	X
Age and Child FE	X	X	X	X	X	X	X	X
Individual Trend	X	X	X	X	X	X	X	X

NOTES: OLS coefficients reported. The sample includes all married individuals who are 22 to 65. The panel of individuals includes those who were married and experienced both the unilateral reform and the pre-reform period. Baseline fixed effects include state fixed effects, quarter-year fixed effects, individual fixed effects, indicators for the number of children, and age indicators. Robust standard errors are clustered at the individual level and are reported in parentheses. ***, **, * represent statistical significance at 1, 5 and 10 percent levels.
SOURCE: Individual-level data from the National Occupation and Employment Survey (ENOE) for the years 2007 Q1 through 2019 Q2.

We then examine women's time use along other dimensions in Columns (5) to (8). We find little evidence that women are reallocating their time to non-labor activities. This finding differs from Roff (2017), who finds that women (and men) in the United States decreased their time spent in household production after the passage of unilateral divorce.

In summary, our results are supportive of two main findings. First, while married women do not enter the labor force, already employed women increase their weekly hours worked. Second, this increase in employment mainly arises from women in the informal sector rather than the formal sector. Both findings are consistent with gender norms playing a role in constraining female labor supply. Additionally, a lack of sufficient labor demand may also partially explain our results. These mechanisms are less applicable to other contexts in Europe and the United States and therefore help explain why our results differ from past research by Bargain et al. (2012) and Stevenson (2008).

8 Heterogeneous Effects

Results by Property Regime. Our initial analysis assumes that women are worse off due to the introduction of unilateral no-fault divorce. However, the magnitude of this harm may depend on the divorce property regime. The spouse with the better outside option, which is partially determined by how assets are divided upon divorce, will have more power within the marriage. This hypothesis is supported by Gray (1998) and Voena (2015), who both highlight the need to account for laws governing the marital property regime. These studies document the importance of property laws for labor supply, savings, and bargaining power in the United States. Using state-level variation in laws governing the marital property regime, Voena (2015) finds a decline in women's employment in states with community property division and no corresponding change in states with title-based or equitable distribution of property. Stevenson (2008), however, finds evidence that the effect of unilateral divorce on women's labor supply does *not* depend on the property regime.

Unfortunately, we cannot directly replicate these studies as we cannot observe the marital property distribution upon divorce. In Mexico, property division is decided at the marriage level. At the time of marriage, the couple can decide to operate under a *community* property regime, where all assets are shared by both spouses (56.9 percent of marriages in the INEGI marital records). Alternatively, the couple can decide on a *separate* property regime, where each spouse individually owns certain assets (26.5 percent of marriages).[14]

We work around this data limitation by using the state-level default property regime that couples face when they get married. We use the classification outlined in Ortega-Díaz (2020) and show these categorizations in Table A2. States fall into three regimes: "conjugal society (conventional and legal), legal society, and property separation" (pg. 4). To maintain consistency with the literature (Stevenson, 2008; Voena, 2015), we classify states into community property if they are in the conjugal social or legal society (only Jalisco) and separate property in cases of property separation.[15]

Using the classification reported in Table A2, we test whether the labor supply results are sensitive to laws governing property division. We interact the state-level *default* property regime with the indicator for the reform. Thus, we augment our baseline equation for woman i in state s during quarter t as follows:

$$\begin{aligned} Y_{ist} = \alpha + \beta_1(\text{Uni}_s \times \text{Post}_t) + \beta_2(\text{Uni}_s \times \text{Post}_t \times \text{Community Regime}_s) \\ + \beta_3(\text{Uni}_s \times \text{Post}_t \times \text{No Regime}_s) + \mathbf{X}_{it}'\gamma + \pi_s + \tau_T + \alpha_i + \phi_i t + \epsilon_{ist} \end{aligned} \tag{4}$$

where $\text{Uni}_s \times \text{Post}_t \times \text{Community Regime}_s$ indicates state-level adoption of the reform in a community property state. $\text{Uni}_s \times \text{Post}_t \times \text{No Regime}_s$ indicates state-level adoption of the reform in a state without a default property regime. The excluded baseline comparison group

[14]The remaining marriages are either a mixture of community and separate, or they are unknown.
[15]See Ortega-Díaz (2020) for additional details regarding the nature of each regime.

is the separate property regime.

Table 5 displays the property regime results. We begin by looking at the impact on employment in Columns (1) to (3), and find that the effect of the baseline reform (separate property states) is positive and significant. We also find that the effect in no-default and community property states is a statistically smaller than the comparable effect in separate property states. These results suggest that the effect of unilateral divorce on labor supply does depend upon the marital property regime. In aggregate, these competing effects across different state-level regimes and different regimes by marriages may cancel each other out. Moving to the intensive margin in Columns (4) to (6), we see that hours worked increases by one half of an hour in separate property states. In the no-default states and community states, the effect is not statistically different from the effect in separate property states once we add linear trends. Finally, for hours worked conditional on employment, there are no heterogeneous effects.

These results are somewhat consistent with the existing literature despite our rough measure of the marital property regime. While Stevenson (2008) finds an increase in labor supply regardless of the property regime, Voena (2015) only sees a decline in employment in states with community property as we do here. If we observed a more accurate measure of the property regime, it is possible our results would more closely align with Voena (2015). These results suggest another reason for the similarity in the labor supply of married before and after the reform. The heterogeneous effects by the property regime may cancel out the impact in aggregate. Nonetheless, these results should be interpreted with caution, as we do not precisely observe the marriage-level property regime.

To examine the robustness of the above results, we use an alternative measure of property regimes. Instead of using the state-level default option, we use the 2011 observed property regimes. More specifically, we categorize each state by the most common property regime based on the INEGI marriage records for 2011.[16] We define the property regime using the majority property division declared at the time of marriage from the marital records in 2011. We define a state as being a community property state if more than 50% of marriages choose community property upon marriage. We categorize a state as having a separate property regime if more than 50% of marriages choose separate property division. We categorize the remaining states as having an 'other' property regime if the majority of marriages choose a mixed property regime or less than 50% of either property regime chosen.[17]

Table A5 presents the results. The estimates suggest similar findings by property regime, whether we use the "on-the-books" default regime or the majority observed regime based on the 2011 marriage records. Employment and hours worked decline significantly in the other property regime states, but the baseline reform coefficient is positive. For hours worked conditional on employment, there is again no observed impact.

[16] We use the 2011 marriage records as the marital property regimes were not well reported in 2010 and were not reported at all prior to 2010.

[17] Four states did not report any property regime data in 2011. For these states, we use the state-level default property regimes. Of these four states, two are coded as 'other' and the other two are re-categorized as community property.

As discussed in Ortega-Díaz (2020), divorce in Mexico often leaves women in a difficult position, and asset division upon divorce is an important factor in the level of vulnerability upon divorce. While in certain cases, couples can sue each other for assets upon divorce, this is not true in the majority of cases.[18] To exacerbate the situation Ortega-Díaz (2020) notes that in separate property marriages "in the case of divorce some states' laws specifically bestow the assets to those who have paid for them, which in most cases favours men as they are the providers. Nevertheless, custody of children in 52% of the cases is given to women" (pg 2). Thus, women who divorce are left with the primary child-caring responsibility but are without assets or formal work to support themselves and their dependents. Overall, our findings, combined with the discussion in Ortega-Díaz (2020), suggest that property regime matters significantly for women.

The above analysis attempts to infer the importance of bargaining power by looking at heterogeneity by the marital property regime. In Section B.2 of the Appendix, we look more directly at this question. Specifically, we use a collective model of intra-household resource allocation to identify changes in women's bargaining power in response to these laws. We find evidence in support of our presumption that women's bargaining power declined.

Table 5: Married Women's Labor Supply by Default Property Regime

	1(Working)			Hours Worked			Hours Worked (Employed)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Uni x Post=1	0.0103*	0.0099*	0.0159*	0.4660**	0.4434**	0.5948*	0.4705	0.4759	0.9072
	(0.0055)	(0.0055)	(0.0086)	(0.2196)	(0.2197)	(0.3421)	(0.3646)	(0.3645)	(0.6091)
No Default × Uni x Post=1	-0.0152**	-0.0151**	-0.0257*	-0.5369*	-0.5182*	-0.7406	-0.1251	-0.1089	-0.4208
	(0.0072)	(0.0072)	(0.0135)	(0.3035)	(0.3034)	(0.5630)	(0.4581)	(0.4587)	(0.9404)
Community Property × Uni x Post=1	-0.0186***	-0.0181***	-0.0215**	-0.4455*	-0.4170*	-0.4654	0.1215	0.1215	-0.4055
	(0.0060)	(0.0060)	(0.0107)	(0.2404)	(0.2406)	(0.4291)	(0.3949)	(0.3952)	(0.7671)
N	204,157	204,157	204,157	204,157	204,157	204,157	85,083	85,083	85,083
Adj R-sq	0.592	0.592	0.613	0.619	0.619	0.642	0.524	0.524	0.560
Mean Dep	0.413	0.413	0.413	14.212	14.212	14.212	34.392	34.392	34.392
State and Quarter-Year FE	X	X	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X	X	X
Age and Child FE		X	X		X	X		X	X
Individual Trend			X			X			X

NOTES: OLS coefficients reported. The sample includes all married individuals who are 22 to 65. The panel of individuals includes those who were married and experienced both the unilateral reform and the pre-reform period. Baseline fixed effects include state fixed effects, quarter-year fixed effects, individual fixed effects, indicators for the number of children, and age indicators. Robust standard errors are clustered at the individual level and are reported in parentheses. ***, **, * represent statistical significance at 1, 5 and 10 percent levels.
SOURCE: Individual-level data from the National Occupation and Employment Survey (ENOE) for the years 2007 Q1 through 2019 Q2.

Subsamples of Men and Women. We conclude by selecting alternative subsamples of women in Table A6. The results are presented with individual fixed effects and time trends. Column (1) shows all women (married, cohabiting, and single). Column (2) presents single women, and Column (3) shows cohabitating women. In Column (4), we present results for

[18] Ortega-Díaz (2020) notes that "This civil code states that in a divorce the spouses may sue each other for compensation of up to 50% of the value of the goods that were acquired during the marriage, provided that they have been married under the regime of separation of property, the plaintiff was dedicated during the marriage predominantly to household labour and childcare, and during the marriage, the plaintiff did not acquire or purchase goods hat are noticeably lower than those of the counterparty". (pg 11)

women who were either married, separated, or divorced pre-reform. Column (5) displays divorced or separated women. Column (6) shows married women with one to three kids, and Column (7) presents married women without kids. Across the subsamples of women, employment patterns are similar before and after the reform, except for cohabitating women, divorce and separated women, and the smaller samples of married women. For completeness, we end by showing the reform effect on men in Appendix Table A7. When individual time trends are included, married men fail to change their labor supply or time use, except for a small increase in household production.

9 Robustness

For robustness tests, we estimate several alternative specifications. First, we use an event-study design and measure the effect by quarter-year surrounding the date of adoption. Second, we provide several alternative methods for estimating standard errors. We then conduct a balance test to compare pre-treatment conditions to the timing of treatment. Next, we examine several different subsamples of married women. Finally, we consider a longer time horizon of the reform and use the full sample of women instead of the individual panel surrounding the reform.

Event Study Analysis. We begin by testing whether women changed their labor supply in the time periods leading up to the reform. To accomplish this, we estimate a flexible event-study specification, for married woman i in state s during time t as:

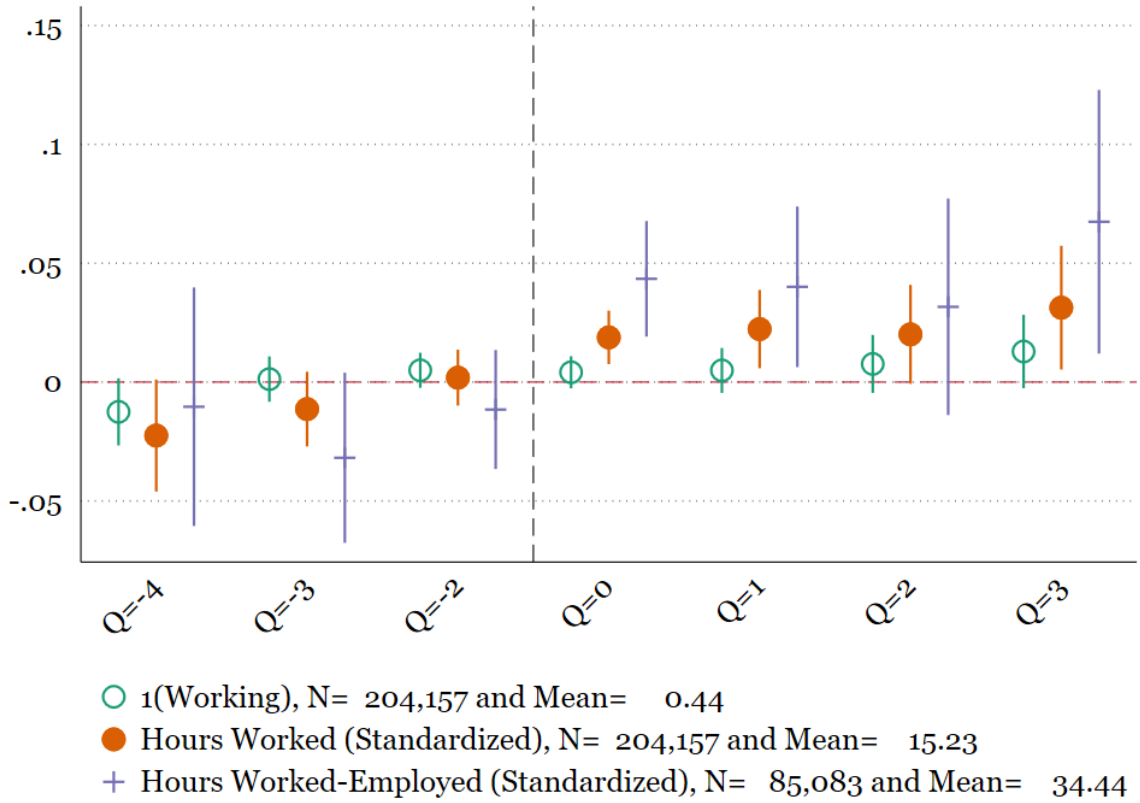
$$Y_{ist} = \alpha + \sum_{Q=-4}^{3} \beta_Q \text{ Unilateral}_{sQ} + \mathbf{X}_{it}'\gamma + \pi_s + \tau_t + \alpha_i + \epsilon_{ist} \tag{5}$$

where the main effect of the reform is captured by the event-study indicator variable, Unilateral_{sQ}. Q represents the period relative to the reform and covers four quarters before and the three quarters after the reform. We exclude the quarter before the reform, $Q = -1$, to capture the baseline labor supply of women just before the passage of the unilateral reform. We choose to exclude the period just before the reform as we expect any effect of the reform to be immediate. Thus, the pre-treatment period Q=-1 provides a better baseline measure of women's labor supply than the quarter the reform went into effect, $Q = 0$.

Figure III presents the estimates for each quarter leading up to and following the reform. The results are shown in an event-study framework where the excluded period is the quarter before the reform (period -1). The plotted coefficients reflect the changes in labor supply relative to the excluded period. The event-study specification also acts as a placebo test to visually inspect whether the response in hours worked is due to the reform or trends surrounding the reform.

We focus on our key measures of labor supply: an indicator for employment (green open

Figure III: Event Study Labor Supply Response – Married Women



NOTES: Event-study coefficients are reported for the reform quarter as well as three quarters post reform. Lines represent 95 percent confidence intervals. The excluded quarter is the quarter just before the reform (period -1). Robust standard errors are clustered at the individual level. OLS coefficients reported. The panel of individuals includes those who were married and experienced both the unilateral reform and the pre-reform period. Baseline fixed effects include state fixed effects, year fixed effects, individual fixed effects, indicators for the number of children, and age indicators.
SOURCE: Individual-level data from the National Occupation and Employment Survey (ENOE) for the years 2007 Q1 through 2019 Q2.

circles), hours worked (orange circles), and hours worked conditional on employment (purple crosses). To plot estimates on the same graph, we standardize hours worked to have a mean of zero and a standard deviation of one. Following the reform, indicated by the vertical dotted-black line, there is an increase in hours worked for married women, but the estimates appear to increase linearly before the reform. There is a smaller increase in employment, but the response is also on a pre-trend. Both hours worked and employment appear to have been increasing before the reform went into effect.

Despite the pre-trend for our main measures, the coefficients on hours worked for working women clearly shift upward after the reform. The estimates for employed hours worked do not appear to be on a pre-trend. Instead, employed hours worked immediately increase after the passage of unilateral divorce. The event-study results are similar to the baseline results, as already employed women increase their labor supply at the intensive margin. However, the findings consistently indicate that married women are not entering the labor force.

Additional Sensitivity Analyses. We next examine the sensitivity of our results to dif-

ferent methods of calculating the standard errors. We initially clustered standard errors at the individual level due to downward bias in the state-level clusters. Mexico has 31 states plus Mexico City. Using the state-level standard errors would mean that our specification would have fewer than 50 clusters, which would pose problems for interpreting our estimates (Bertrand et al., 2004b). Because we cluster at the individual level in our main results, the small number of clusters is not a problem for our preferred specification. Further, our baseline calculation of the standard errors also alleviates concern over serial correlation in the same individual's labor supply over time.

To ensure our findings are not dependent on this choice of standard error calculation, we present results clustered at the state level. The choice of state-level clusters corresponds to the treatment and would be more appropriate if our number of clusters were closer to 50 (Bertrand et al., 2004b; Abadie et al., 2017). Table A8 shows that the standard errors clustered at the state level are smaller (but similar in magnitude) to the individual-level clusters. For hours worked conditional on employment, the results are still statistically significant at the 5% level. As a secondary check, at the bottom of Table A8, we show the p-values using wild-cluster bootstrapped standard errors. Cameron et al. (2008) suggests this bootstrapping method for dealing with a low number of clusters. The p-values from this handling of standard errors are shown at the bottom of Table A8 (underneath the means). The alternative p-values suggest the results are still significant with alternative standard errors.[19] Both the bootstrapped standard errors and the state-level standard errors produce a similar interpretation of the main findings.

Next, we conduct a balance test by comparing early and late-adopting states following Goodman-Bacon (2018). These results are presented in Figure A1. The balance test suggests that there are some differences in states treated earlier in the sample. However, given that we conduct an individual fixed effects model, this effect is less concerning than if we were considering the state-level aggregate effects. This balance test, however, indicates the treatment effect may vary across states and over time.

Based on the balance test results, we estimate several additional specifications in Table A9. As a direct extension of the balance test, we first present the results for the early versus the later-treated states. The motivation behind this restriction is that early-adopting states differ in observable ways from later-adopting states. Column (1) shows the results restricting the sample to states that passed the reform before 2015. Column (2) shows the post-2015 group. In Columns (1) to (2), the estimates for both early and later-treated groups both fail to be significant for employment and hours worked. For employed hours worked, the results are only significant in the early-treated group and the estimates suggest an additional hour in employment per week. The effect in early-treated states is one half-hour higher than the main effect.

Then, in Column (3), we remove states where the *de jure* date comes before the *de facto* reform. In cases where the law is on the books before it is in practice, labor supply may respond

[19] We are unable to compute the p-values for the specification with linear trends due to the multiple interaction terms.

by increasing in anticipation of the *de facto* law change and bias the main effect downward towards zero. The results in Column (3) for hours conditional on employment suggests that this may be the case. The coefficient in Column (3) is larger than the baseline effect in Table 2. However, the estimates across employment and hours worked still show no effect. This issue is not a major driver of the findings, as the *de jure* date coming before the *de facto* reforms only happens in two states, Quintana Roo and Michoacan.

Next, in Column (4), we remove Mexico City from the estimation sample. Mexico City is the most liberal state in Mexico and was the first adopter of the unilateral no-fault legislation. Mexico City has also implemented unique reforms, including rights for cohabitating couples through the *Ley de Sociedad de Convivencia* in 2006 and the decriminalization of abortion in 2007.[20] The differences between Mexico City and other states will be exacerbated if individuals in Mexico City are incentivized to cohabitate rather than marry or have access to the termination of unwanted pregnancies. After excluding Mexico City, in Column (4), the results again fail to show an effect on employment and hours worked, but the significant impact remains for hours worked conditional on employment. In Columns (5) to (7), we examine additional subsamples of interest, including rural, urban, and women with children under five. The results across these sub-samples show the findings for employed hours worked are more significant in rural areas.

We next address individual attrition from the panel. Individuals leave the survey for various reasons, including divorce. Since attrition may bias the results towards those who do not leave the panel, we present results applying Inverse-Probability Weights to account for sample selection in Column (8) (Wooldridge, 2010; Voena, 2015). For hours worked conditional on employment, the adjusted results are still statistically significant, and the coefficient is positive. Second, in Column (9), we show the results with the balanced panel of individuals (in the sample for five quarters). There is no attrition in this sample, and the results appear slightly higher in magnitude. Together, the balanced panel and Inverse-Probability Weights suggest that attrition is not the primary reason for the observed effect on employed hours worked.

Then, in Column (10), we show the results only for women who are out of school. These results are similar to Column (9). In Column (11) we determine treatment using the legal *de jure* legislation dates instead of the *de facto* dates. With the legal *de jure* dates, we observe no effect of the reform, suggesting that when individuals could exercise divorce was more important for the labor supply response. Finally, in Column (12), we conduct a placebo test, where the reform is now coded as a year in advance. Here all findings are insignificant and the employed hours worked is negative. The placebo test helps to confirm that our findings are not due to the underlying mechanics of the data.

We conclude by showing the long-run dynamic effect of the unilateral reform in Table A10. We perform this final check to ensure that there are not dynamic effects or high search costs that prevent women from changing their labor supply in the small panel provided in the ENOE.

[20] Oaxaca later decriminalized abortion in 2019, but after the final quarter of the sample.

We replace the individual fixed effects with state fixed effects and state linear trends. We also only include the first quarter the individual is observed in the data to avoid duplicate observations of the same individual. The results in Table A10 reflect the baseline specification with individual trends and fixed effects.

10 Conclusion

Unilateral divorce laws are present in at least nine countries and continue to spread across the world. Over the past 30 years in Latin American alone, Brazil, Argentina, Colombia, and Chile have all made efforts to ease the divorce process. Lowering the cost to divorce impacts all aspects of marriage: who gets married, how married couples behave, and which couples get divorced. Moreover, unilateral divorce may have different effects in low and middle-income countries where women, at times, have a lower standing in society, and there are fewer social protections for vulnerable individuals. It is, therefore, necessary to empirically study the welfare effects of these laws.

This paper analyzes the consequences of no-fault unilateral divorce laws in Mexico. We see no change in married women's labor force participation rates but find an increase in hours worked for employed married women. These results differ from past research on this topic, which have shown larger increases along the extensive margin (Stevenson, 2008; Bargain et al., 2012; Voena, 2015). We hypothesize that these differences are due to the cultural context. In Mexico, there are restrictive social norms against women working, and women may have limited formal labor market opportunities.

We formalize the relationship between social stigma against women working with a collective household model where the husband may experience dis-utility from his wife working. The model provides an explanation for our labor supply results: a decline in bargaining power will not push women into the labor market when gender norms against women working are high, despite leisure being a normal good. We provide evidence in support of the model in our labor supply analysis. Moreover, we provide a separate analysis using consumption data to confirm that women did, in fact, experience a decline in bargaining power due to the introduction of unilateral divorce.

There are several limitations to our analysis that motivate future work. First, the relationship between divorce laws, labor supply, and bargaining power is complicated. We've examined this relationship in several ways. First, we allow the effect of unilateral divorce on labor supply to vary with the state-level default marital property regime. A better analysis would account for the actual marital property regime, which is determined at the marriage level in Mexico. Second, we directly test the effect of unilateral divorce on bargaining power by estimating changes in intra-household resource allocation. While there are limitations to this analysis, we believe this is a worthwhile first step. Future work can simultaneously study how consumption goods and leisure are allocated to estimate changes in bargaining power as

a result of the divorce laws. Lise and Seitz (2011) conduct an analysis along these lines. However, there are several obstacles that prevent us from taking this route. First, non-participation in the labor market is high in Mexico, and offered wages are therefore unobserved for the majority of women. Moreover, even among women who do work, many work in the informal sector without an observable wage. Future work can attempt to overcome these obstacles.

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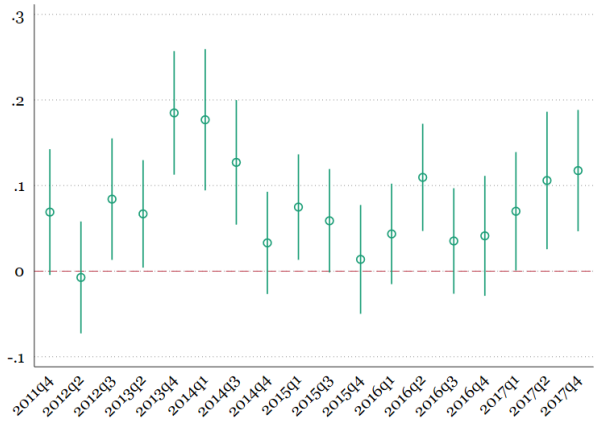
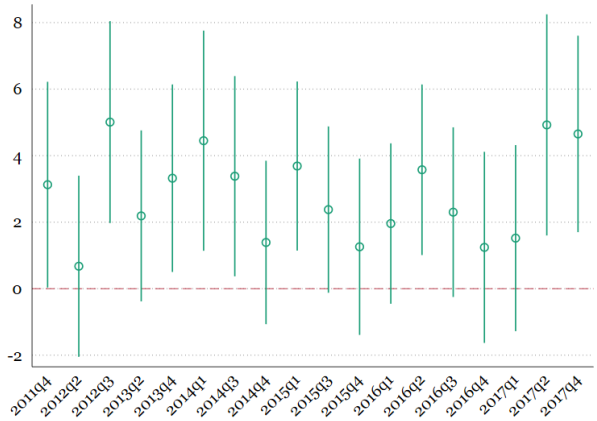
A Additional Figures

Figure A1: Balance Test – Married Women

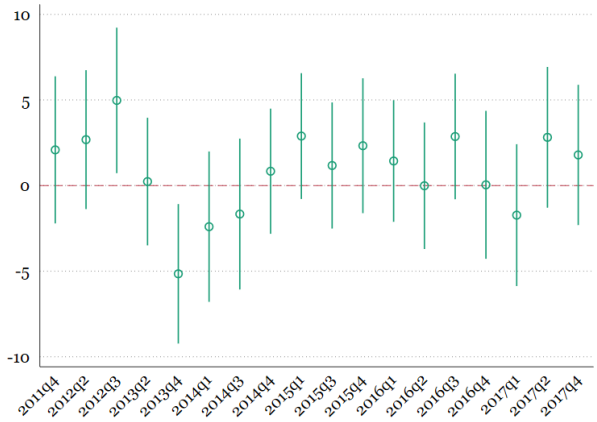
Panel A: 1(Working)



Panel B: Work Hours



Panel C: Employed Hours Worked



NOTES: Reported coefficients are from regression of 1(work) or hours worked on indicators for the timing of the reform (quarter-year). Controls include indicators for age, education, and number of children. Robust standard errors reported.
SOURCE: Individual-level data from the National Occupation and Employment Survey (ENOE) for the years 2007 Q1 through 2019 Q2.

B Additional Tables

Table A1: Unilateral Divorce Legislation Year and State, 2008-2017

Region	State	De Facto Year	De Jure Year	Legal Code (Family v. Civil)	Divorce Articles (#)
Central	Mexico City	2008	2008	Civil	266, 267, 272
	Guanajuato	2015		Civil	328, 323, 329
	Hidalgo	2011	2011	Family	102, 103
	Mexico	2012	2012	Civil	4.89, 4.91, 4.191, 4.102, 4.105
	Morelos	2016	2016	Family	174, 175
	Puebla	2016	2016	Civil	442 - 453
	Queretaro	2015	2016	Civil	246, 249, 252, 253
	Tlaxcala	2016	2016	Civil	123, 125
North	Aguascalientes	2015	2015	Civil	288, 289, 294, 295, 296, 298
	Baja California	2016		Civil	264, 269, 271
	Baja California Sur	2017	2017	Civil	305, 273, 277, 278, 279, 284, 288, 289
	Coahuila	2013	2013	Civil	362, 363, 369, 374
	Chihuahua	2016		Civil	255, 256
	Durango	2016		Civil	261-286
	Nuevo Leon	2014	2016	Civil	267, 272, 274
	San Luis Potosi	2016	2017	Family	86, 87
	Sinaloa	2013	2013	Family	181, 182, 184
	Sonora	2015	2015	Family	141-156
	Tamaulipas	2014	2015	Civil	248, 249, 253
	Zacatecas	2017	2017	Family	214, 215, 223, 224, 231
West	Colima	2016	2016	Civil	267, 268, 272, 273, 278
	Jalisco	2016	2018	Civil	404, 405
	Michoacan	2016	2015	Family	253- 258
	Nayarit	2015	2015	Civil	221, 260, 261, 263, 265
South-East	Campeche	2014		Civil	281, 282, 283, 284, 287
	Chiapas	2014		Civil	263, 268, 269, 270
	Guerrero	2012	2012	Ley de Divorcio	4, 11, 12, 13, 16, 17, 27, 28, 44
	Oaxaca	2017	2017	Civil	278, 279, 284, 285
	Quintana Roo	2014	2013	Civil	798, 799, 800, 801, 804, 805
	Tabasco	2015		Civil	257, 258, 267, 268, 269, 272
	Veracruz	2015	2015	Civil	141, 146, 147, 148, 150
	Yucatan	2013	2013	Family	191, 192

KEY: Blue indicates conflict between the *de facto* and *de jure* years. There are additional states including Guerrero, Hidalgo, Morelos, Oaxaca, Sonora, Veracruz, and Yucatan, where the quarters differ by more than a single quarter between *de facto* and *de jure* practices.
NOTES: When the sources conflict, for our baseline analysis, we default to the *de facto* quarter-year combination where the number of unilateral divorces sentenced exceeds ten (see INEGI). Based on our research, states with blank years had not passed unilateral divorce *as of 2017*.
SOURCES: Author's combination of the sources including: (i) family and civil codes of each state, (ii) popular press articles, (iii) Garcia-Ramos (2017), (iv) Mendez-Sachez (2014), and (v) Municipality-level variables from the INEGI divorce statistics.

Table A2: State Property Division

State	(1) Separate	(2) Community	(3) None	(4) Separate Share	(5) Community Share	(6) Other Share
Aguascalientes		X		53.7	25.3	21.0
Baja California			X	0.0	0.0	100.0
Baja California Sur			X	29.9	39.8	30.3
Campeche	X			71.9	27.7	0.4
Chiapas			X	11.7	87.7	0.5
Chihuahua		X		0.0	0.0	100.0
Coahuila de Zaragoza	X			30.7	64.2	5.1
Colima			X	0.1	0.0	99.9
Distrito Federal			X	37.7	58.0	4.3
Durango			X	17.2	39.9	42.9
Guanajuato	X			60.9	0.2	38.9
Guerrero	X			8.5	90.2	1.3
Hidalgo		X		23.5	76.0	0.5
Jalisco		X		14.6	85.4	0.0
Mexico	X			61.0	22.9	16.1
Michoacan de Ocampo	X			66.2	31.5	2.3
Morelos		X		34.1	64.7	1.3
Nayarit			X	16.1	83.6	0.3
Nuevo Leon		X		7.1	92.8	0.1
Oaxaca		X		6.1	93.9	0.0
Puebla		X		5.2	93.4	1.4
Queretaro		X		66.0	13.3	20.7
Quintana Roo		X		0.1	1.0	98.9
San Luis Potos		X		94.1	0.0	5.8
Sinaloa			X	22.6	71.6	5.9
Sonora		X		13.4	70.5	16.1
Tabasco		X		11.4	88.2	0.4
Tamaulipas		X		5.6	90.0	4.4
Tlaxcala	X			18.6	80.6	0.9
Veracruz de Ignacio de la Llave		X		16.5	82.9	0.6
Yucatan	X			30.4	69.4	0.2
Zacatecas	X			11.7	88.1	0.2

NOTES AND SOURCES: Columns (1) to (3) show the default marital property regime. We follow the legal property regimes of states outlined in Ortega-Díaz (2020). We classify Conjugal Society and Legal Society (only Jalisco) as Community Property. Columns (4) to (6) presents the share of marriages selecting different property regimes. "Other" primarily includes marriages that follow a mixed property regime (i.e., a combination of community and separate), or is listed as unknown. The percent of marriages choosing each regime are from the INEGI 2011 marriage statistics.

Table A3: Probability of Leaving the Panel Early

	1(Leave Sample)			1(Leave Divorced)		
	(1)	(2)	(3)	(4)	(5)	(6)
Uni x Post	-0.0001	-0.0002	-0.0002	-0.0003	-0.0003	-0.0003
	(0.0020)	(0.0019)	(0.0019)	(0.0006)	(0.0006)	(0.0006)
Education		-0.0000	-0.0000		-0.0001**	-0.0001**
		(0.0002)	(0.0002)		(0.0000)	(0.0000)
Age		-0.0117***			-0.0007***	
		(0.0004)			(0.0001)	
Age Squared		0.0001***			0.0000***	
		(0.0000)			(0.0000)	
Number of Children		-0.0127***	-0.0126***		-0.0005***	-0.0006***
		(0.0008)	(0.0008)		(0.0001)	(0.0001)
1(Child <5)		-0.0428***	-0.0418***		-0.0031***	-0.0030***
		(0.0018)	(0.0017)		(0.0004)	(0.0004)
N	581,516	581,516	581,516	581,516	581,516	581,516
Adj R-sq	0.001	0.037	0.038	0.000	0.002	0.002
Mean Dep	0.032	0.032	0.032	0.004	0.004	0.004
Quarter-Year FE	X	X	X	X	X	X
State FE	X	X	X	X	X	X
Age FE			X			X

NOTES: OLS coefficients reported. Robust standard errors are clustered at the state level and are reported in parentheses. ***, **, * represent statistical significance at 1, 5 and 10 percent levels.
SOURCE: Individual-level data from the National Occupation and Employment Survey (ENOE) for the years 2007 Q1 through 2019 Q2.

Table A4: Labor Supply and the Quarter-Year Timing of the Reform

	Quarter-Year Defacto Divorce											
	(1) All	(2) All	(3) Married	(4) Single	(5) All	(6) All	(7) Married	(8) Single	(9) All	(10) All	(11) Married	(12) Single
1(Working)	-0.0774	0.1182	0.2451	0.2247								
	(0.2198)	(0.1942)	(0.3101)	(0.2674)								
Hours Worked					-0.0014	0.0030	0.0056	0.0064				
					(0.0040)	(0.0039)	(0.0064)	(0.0052)				
Hours Worked (Employed)									0.0002	0.0022	0.0028	0.0081
									(0.0059)	(0.0066)	(0.0081)	(0.0087)
N	113,399	113,348	12,928	4,498	113,399	113,348	12,928	4,498	56,431	56,406	5,447	3,167
Adj R-sq	0.000	0.010	0.010	0.014	0.000	0.010	0.010	0.014	-0.000	0.009	0.009	0.018
Controls		X	X	X		X	X	X		X	X	X

NOTES: Regression of the quarter year timing of the defato divorce reform on individual labor supply. Data sample includes the first quarter of the data used in the analysis, 2007Q1. Controls included indicators for age, number of children, education, and urban status. Robust standard errors are clustered at the state level and are reported in parentheses. ***, **, * represent statistical significance at 1, 5 and 10 percent levels. OLS coefficients reported.
SOURCE: Individual-level data from the National Occupation and Employment Survey (ENOE) for the years 2007 Q1 through 2019 Q2.

Table A5: Married Women's Labor Supply by Observed Property Regime

	1(Working)			Hours Worked			Hours Worked (Employed)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Uni x Post=1	0.0133**	0.0131**	0.0165*	0.5073**	0.4945**	0.3908	0.3303	0.3458	0.5505
	(0.0058)	(0.0057)	(0.0092)	(0.2323)	(0.2323)	(0.3652)	(0.3918)	(0.3920)	(0.7004)
Other Property × Uni x Post=1	-0.0254***	-0.0251***	-0.0279*	-0.6269*	-0.6101*	-0.7753	-0.0699	-0.0680	-0.7461
	(0.0092)	(0.0092)	(0.0166)	(0.3698)	(0.3695)	(0.6513)	(0.5649)	(0.5643)	(1.1399)
Community Property × Uni x Post=1	-0.0203***	-0.0202***	-0.0209*	-0.4720*	-0.4610*	-0.1401	0.2945	0.2828	0.2313
	(0.0060)	(0.0060)	(0.0112)	(0.2445)	(0.2444)	(0.4455)	(0.4078)	(0.4077)	(0.8334)
N	204,157	204,157	204,157	204,157	204,157	204,157	85,083	85,083	85,083
Adj R-sq	0.592	0.592	0.613	0.619	0.619	0.642	0.524	0.524	0.560
Mean Dep	0.413	0.413	0.413	14.212	14.212	14.212	34.392	34.392	34.392
State and Quarter-Year FE	X	X	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X	X	X
Age and Child FE		X	X		X	X		X	X
Individual Trend			X			X			X

NOTES: OLS coefficients reported. The sample includes all married individuals who are 22 to 65. The panel of individuals includes those who were married and experienced both the unilateral reform and the pre-reform period. Baseline fixed effects include state fixed effects, quarter-year fixed effects, individual fixed effects, indicators for the number of children, and age indicators. Robust standard errors are clustered at the individual level and are reported in parentheses. ***, **, * represent statistical significance at 1, 5 and 10 percent levels.
SOURCE: Individual-level data from the National Occupation and Employment Survey (ENOE) for the years 2007 Q1 through 2019 Q2.

Table A6: Unilateral Divorce Reform and Labor Supply, Varied Samples

PANEL A: 1(WORKING)

	All	Single	Cohabit-ating	Coupled	Divorced/ Separated	Married 1-3	Married None
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Uni x Post	-0.0020	-0.0018	-0.0086	-0.0002	-0.0067	-0.0016	-0.0293
	(0.0033)	(0.0073)	(0.0089)	(0.0042)	(0.0118)	(0.0055)	(0.0230)
N	407,394	80,203	57,488	235,773	31,614	138,455	7,149
Adj R-sq	0.627	0.608	0.577	0.624	0.601	0.630	0.651
Mean Dep	0.490	0.669	0.408	0.448	0.685	0.434	0.559
State and Quarter-Year FE	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X
Age and Child FE	X	X	X	X	X	X	X
Individual Trend	X	X	X	X	X	X	X

PANEL B: HOURS WORKED

	All	Single	Cohabit-ating	Coupled	Divorced/ Separated	Married 1-3	Married None
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Uni x Post	0.1526	0.4866	-0.0597	0.1397	-0.5900	0.0474	-0.7733
	(0.1371)	(0.3509)	(0.3572)	(0.1712)	(0.5697)	(0.2197)	(1.0105)
N	407,394	80,203	57,488	235,773	31,614	138,455	7,149
Adj R-sq	0.651	0.603	0.621	0.650	0.617	0.650	0.654
Mean Dep	17.961	26.684	14.540	15.736	26.247	15.009	20.306
State and Quarter-Year FE	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X
Age and Child FE	X	X	X	X	X	X	X
Individual Trend	X	X	X	X	X	X	X

PANEL C: EMPLOYED HOURS WORKED

	All	Single	Cohabit-ating	Coupled	Divorced/ Separated	Married 1-3	Married None
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Uni x Post	0.4589**	0.7094*	0.4784	0.4720*	-0.0185	0.3629	0.1003
	(0.2061)	(0.3685)	(0.6284)	(0.2850)	(0.6213)	(0.3756)	(1.2762)
N	201,944	53,507	24,833	106,909	21,824	60,822	4,149
Adj R-sq	0.546	0.488	0.579	0.555	0.525	0.557	0.564
Mean Dep	36.674	39.883	35.604	35.153	38.310	34.592	36.344
State and Quarter-Year FE	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X
Age and Child FE	X	X	X	X	X	X	X
Individual Trend	X	X	X	X	X	X	X

NOTES: OLS coefficients reported. Baseline fixed effects include state fixed effects, quarter-year fixed effects, individual fixed effects, indicators for the number of children, and age indicators. Column (1) shows all women. Column (2) presents single women, and Column (3) shows cohabitating women. Column (4) shows women who were either married, separated, or divorced pre-reform. Column (5) displays divorced or separated women. Column (6) shows married women with one to three kids, and Columns (7) presents married women without kids. Robust standard errors are clustered at the individual level and are reported in parentheses. ***, **, * represent statistical significance at 1, 5 and 10 percent levels.

SOURCE: Individual-level data from the National Occupation and Employment Survey (ENOE) for the years 2007 Q1 through 2019 Q2.

Table A7: Unilateral Divorce Reform and Married Men

	1(Working)	Hours Worked	Hours Worked (Employed)	Time Children Elderly	Time Household Production
	(1)	(2)	(3)	(4)	(5)
Uni x Post	-0.0040	0.0189	0.1749	-0.0796	0.2586*
	(0.0034)	(0.2454)	(0.2291)	(0.1498)	(0.1369)
N	189,136	189,136	169,066	108,242	118,206
Adj R-sq	0.480	0.459	0.393	0.257	0.160
Mean Dep	0.901	41.123	45.642	3.694	6.368
State and Quarter-Year FE	X	X	X	X	X
Individual FE	X	X	X	X	X
Age and Child FE	X	X	X	X	X
Individual Trend	X	X	X	X	X

NOTES: OLS coefficients reported. The sample includes all married individuals who are 22 to 65. The panel of individuals includes those who were married and experienced both the unilateral reform and the pre-reform period. Baseline fixed effects include state fixed effects, quarter-year fixed effects, individual fixed effects, indicators for the number of children, and age indicators. Robust standard errors are clustered at the individual level and are reported in parentheses. ***, **, * represent statistical significance at 1, 5 and 10 percent levels.
SOURCE: Individual-level data from the National Occupation and Employment Survey (ENOE) for the years 2007 Q1 through 2019 Q2.

Table A8: Unilateral Divorce Reform and Married Women's Labor Supply, Standard Errors Clustered at the State Level

	1(Working)			Hours Worked			Hours Worked Excluding Zeros		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Uni x Post	-0.0023	-0.0023	0.0006	0.1420	0.1380	0.2370	0.5165**	0.5245**	0.6310**
	(0.0041)	(0.0041)	(0.0045)	(0.1542)	(0.1514)	(0.1589)	(0.2182)	(0.2191)	(0.2904)
N	204,157	204,157	204,157	204,157	204,157	204,157	85,083	85,083	85,083
Adj R-sq	0.592	0.592	0.613	0.619	0.619	0.642	0.524	0.525	0.560
Mean Dep	0.413	0.413	0.413	14.212	14.212	14.212	34.392	34.392	34.392
Wild-Bootstrapped P-Value	0.617	0.611	.	0.405	0.418	.	0.058	0.050	.
State and Quarter-Year FE	X	X	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X	X	X
Age and Child FE		X	X		X	X		X	X
Individual Trend			X			X			X

NOTES: OLS coefficients reported. The sample includes all married individuals who are 22 to 65. The panel of individuals includes those who were married and experienced both the unilateral reform and the pre-reform period. Baseline fixed effects include state fixed effects, quarter-year fixed effects, individual fixed effects, indicators for the number of children, and age indicators. Robust standard errors are clustered at the state level and are reported in parentheses. ***, **, * represent statistical significance at 1, 5 and 10 percent levels.
SOURCE: Individual-level data from the National Occupation and Employment Survey (ENOE) for the years 2007 Q1 through 2019 Q2.

Table A9: Robustness Checks on Married Women's Labor Supply

PANEL A: 1(WORKING)

	Early Adopters	Late Adopters	Defacto <= Dejure	No Mexico City	Urban	Rural	Child 0-5	IPW for Attrition	Five Quarters	Out of School	De Jure	Placebo
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Uni x Post	-0.0029	-0.0005	-0.0017	0.0006	0.0023	-0.0014	0.0036	0.0013	-0.0002	0.0007	-0.0043	-0.0061
	(0.0095)	(0.0056)	(0.0047)	(0.0045)	(0.0051)	(0.0062)	(0.0097)	(0.0032)	(0.0046)	(0.0046)	(0.0065)	(0.0044)
N	78,381	125,776	192,399	199,169	121,361	82,795	42,065	204,157	197,999	199,655	154,745	205,225
Adj R-sq	0.622	0.605	0.611	0.611	0.644	0.588	0.620	0.637	0.613	0.612	0.610	0.627
Mean Dep	0.416	0.411	0.413	0.410	0.443	0.391	0.378	0.442	0.412	0.412	0.419	0.404
State and Quarter-Year FE	X	X	X	X	X	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X	X	X	X	X	X
Age and Child FE	X	X	X	X	X	X	X	X	X	X	X	X
Individual Trend	X	X	X	X	X	X	X	X	X	X	X	X

PANEL B: HOURS WORKED

	Early Adopters	Late Adopters	Defacto <= Dejure	No Mexico City	Urban	Rural	Child 0-5	IPW for Attrition	Five Quarters	Out of School	De Jure	Placebo
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Uni x Post	0.2302	0.2145	0.1944	0.2382	0.0828	0.2542	0.1212	0.1969	0.2119	0.2382	-0.1976	-0.1481
	(0.3905)	(0.2197)	(0.1867)	(0.1781)	(0.2083)	(0.2416)	(0.3705)	(0.1314)	(0.1797)	(0.1796)	(0.2569)	(0.1775)
N	78,381	125,776	192,399	199,169	121,361	82,795	42,065	204,157	197,999	199,655	154,745	205,225
Adj R-sq	0.642	0.642	0.641	0.643	0.651	0.633	0.648	0.661	0.642	0.642	0.632	0.652
Mean Dep	14.443	14.010	14.245	14.141	15.602	13.178	12.614	15.239	14.154	14.175	14.440	13.964
State and Quarter-Year FE	X	X	X	X	X	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X	X	X	X	X	X
Age and Child FE	X	X	X	X	X	X	X	X	X	X	X	X
Individual Trend	X	X	X	X	X	X	X	X	X	X	X	X

PANEL C: EMPLOYED HOURS WORKED

	Early Adopters	Late Adopters	Defacto <= Dejure	No Mexico City	Urban	Rural	Child 0-5	IPW for Attrition	Five Quarters	Out of School	De Jure	Placebo
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Uni x Post	1.0882*	0.5125	0.6747**	0.6245*	0.2277	0.8466*	0.6667	0.4167*	0.6280*	0.6311*	-0.0354	-0.0624
	(0.6599)	(0.3936)	(0.3375)	(0.3210)	(0.3397)	(0.4809)	(0.7049)	(0.2316)	(0.3241)	(0.3246)	(0.4792)	(0.3339)
N	32,136	52,947	80,038	82,988	54,643	30,438	16,139	85,083	82,543	82,862	65,085	85,319
Adj R-sq	0.563	0.558	0.557	0.563	0.545	0.560	0.563	0.550	0.562	0.561	0.554	0.574
Mean Dep	34.757	34.075	34.470	34.450	35.254	33.670	33.410	34.455	34.349	34.434	34.450	34.541
State and Quarter-Year FE	X	X	X	X	X	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X	X	X	X	X	X
Age and Child FE	X	X	X	X	X	X	X	X	X	X	X	X
Individual Trend	X	X	X	X	X	X	X	X	X	X	X	X

NOTES: Baseline fixed effects include state fixed effects, quarter-year fixed effects, individual fixed effects, indicators for the number of children, and age indicators. Column (1) removes states not treated before 2015. Column (2) includes states treated after 2014. Column (3) excludes states where the de jure date comes before the de facto dates. Column (4) excludes Mexico City. Columns (5) and (6) split the sample into rural and urban areas. Column (7) includes only individuals with children under age 5. Column (8) includes IPW for attrition (Wooldridge, 2010; Voena, 2015). Column (9) drops those who leave the panel, thus it is a 5-quarter balanced panel. Column (10) drops those who were in school in the first quarter observed in the sample. Column (11) shows the results with *de jure* dates. Column (12) tests a placebo test, where the reform is now coded a one year prior to the actual reform. OLS coefficients reported. The sample includes all married individuals who are 22 to 65. The panel of individuals includes those who were married and experienced both the unilateral reform and the pre-reform period. Baseline fixed effects include state fixed effects, quarter-year fixed effects, individual fixed effects, indicators for the number of children, and age indicators. Robust standard errors are clustered at the individual level and are reported in parentheses. ***, **, * represent statistical significance at 1, 5 and 10 percent levels.
SOURCE: Individual-level data from the National Occupation and Employment Survey (ENOE) for the years 2007 Q1 through 2019 Q2.

Table A10: Unilateral Divorce Reform and the Long-term State-level Effect of the Reform

	1(Working)			Hours Worked			Hours Worked (Employed)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Uni x Post	-0.0025	-0.0033	-0.0015	0.1287	0.1013	0.1341	0.5241**	0.5169**	0.4437*
	(0.0034)	(0.0036)	(0.0032)	(0.1304)	(0.1345)	(0.1293)	(0.2145)	(0.2137)	(0.2307)
N	581,516	581,515	581,515	581,516	581,515	581,515	255,221	255,221	255,221
Adj R-sq	0.010	0.039	0.040	0.006	0.032	0.032	0.007	0.014	0.014
Mean Dep	0.411	0.411	0.411	14.224	14.224	14.224	34.590	34.590	34.590
State and Quarter-Year FE	X	X	X	X	X	X	X	X	X
Age and Child FE		X	X		X	X		X	X
State-level Trend			X			X			X

NOTES: OLS coefficients reported. The sample includes all married individuals who are 22 to 65. The sample includes all married women over 2007Q1-2019Q2. We only include the first quarter the woman is in the ENOE sample. Fixed effects include the state, the quarter-year, and indicators for age and number of young children. Column (3) includes linear trends. Robust standard errors are clustered at the state level and are reported in parentheses. ***, **, * represent statistical significance at 1, 5 and 10 percent levels.
SOURCE: Individual-level data from the National Occupation and Employment Survey (ENOE) for the years 2007 Q1 through 2019 Q2.

B.1 Proofs

We first derive equilibrium conditions for labor supply as a function of the level of gender norms against working. We then use these conditions to illustrate how the introduction of a no-fault unilateral divorce law will alter labor supply given gender norms against female labor supply are either low (Prediction 1) or high (Prediction 2). Prediction 1 will follow immediately from Chiappori (1992), while the proof of Prediction 2 relates to Field et al. (2019).

Assume that $u(c, l)$ is continuous, twice differentiable, strictly increasing, strictly concave, and supermodular. Supermodularity requires that $\frac{\partial u}{\partial c \partial l} \geq 0$ is a common requirement to study comparative statics and will allow us to make unambiguous predictions regarding changes in labor supply due to divorce laws (Topkis, 1998).

We rewrite the wife's problem given by Equation (2) in terms of leisure as follows:

$$\max_{c_f, l_f} u_f[c_f, l_f] - \gamma_f(1 - l_f) - \frac{1 - \mu(z)}{\mu(z)}\gamma_m(1 - l_f) \quad \text{s.t.} \quad p_f c_f \leq w_f(1 - l_f) + \phi(z)$$

We normalize the price of the consumption good to one. Then $c_f = w_f(1 - l_f) + \phi(z)$. The wife's maximization problem is then:

$$\max_{l_f} u_f[w_f(1 - l_f) + \phi(z), l_f] - \gamma_f(1 - l_f) - \frac{1 - \mu(z)}{\mu(z)}\gamma_m(1 - l_f)$$

with first order conditions given by

$$u_1[w_f(1 - l_f^*) + \phi(z), l_f^*](-w_f) + u_2[w_f(1 - l_f^*) + \phi(z), l_f^*] + \gamma_f + \frac{1 - \mu(z)}{\mu(z)}\gamma_m = 0 \tag{A1}$$

We apply the implicit function theorem to Equation (A1) to determine how leisure (and therefore labor supply) will change as a result of a divorce law.

$$\frac{\partial l_f}{\partial z} = \frac{-[u_{11}(\cdot,\cdot)(-w_f)\frac{\partial c}{\partial \phi}\frac{\partial \phi}{\partial z} + u_{21}(\cdot,\cdot)\frac{\partial c}{\partial \phi}\frac{\partial \phi}{\partial z} - \frac{\partial \mu/\partial z}{\mu(z)^2}\gamma_m]}{NegativeTerm} \tag{A2}$$

Given that utility is assumed to be well-behaved, $u_{11} < 0$. Moreover, by supermodularity, $u_{21} \geq 0$. Furthermore, we assume (for now) that $\frac{\partial \phi}{\partial z}$ and $\frac{\partial \mu}{\partial z}$ are negative.[21] That is, more liberalized divorce laws result in lower female bargaining power, and therefore a smaller share of nonlabor income. As a result, the first term in the numerator is negative. By similar logic, the second term in the numerator is also negative. In the interest of conciseness, we do not write out the denominator. However, one can show it is negative by taking the second order condition of the wife's problem and assuming that the utility

[21] We confirm this assumption empirically in Section B.2.

function is, again, well-behaved.

Prediction 1

Suppose that γ_m is small enough such that:

$$\gamma_m < \frac{u_{11}(\cdot,\cdot)(-w_f)\frac{\partial c}{\partial \phi}\frac{\partial \phi}{\partial z} + u_{21}(\cdot,\cdot)\frac{\partial c}{\partial \phi}\frac{\partial \phi}{\partial z}}{\frac{\partial \mu/\partial z}{\mu(z)^2}}$$

Then the numerator of Equation (A2) is positive, and more liberal unilateral divorce laws will result in a decline in leisure, and therefore an increase in labor supply. This is a standard result. A more liberal unilateral divorce law decreases the wife's bargaining power, and thus her share of nonlabor income. Since leisure is a normal good, she consumes less of it and works more. This finding is easiest to see by setting $\gamma_m = 0$.

Prediction 2

Suppose that γ_m is large enough such that, such that:

$$\gamma_m > \frac{u_{11}(\cdot,\cdot)(-w_f)\frac{\partial c}{\partial \phi}\frac{\partial \phi}{\partial z} + u_{21}(\cdot,\cdot)\frac{\partial c}{\partial \phi}\frac{\partial \phi}{\partial z}}{\frac{\partial \mu/\partial z}{\mu(z)^2}}$$

Then the numerator of Equation (A2) is negative, and more liberal unilateral divorce laws will result in an increase in leisure, and therefore an increase in labor supply. This result is somewhat counterintuitive. Women are worse off due to the unilateral divorce law; they experience a decline in their share of nonlabor income and want to work more as a result. However, the divorce law also increases the weight they place on their husband's dis-utility from working, as $\partial\mu/\partial z < 0$. If γ_m is sufficiently large, this will prevent them from increasing their labor supply. As gender norms against women working increase, this mechanism is more salient.

B.2 What is the Effect of Unilateral Divorce Laws on Women's Bargaining Power?

In the main analysis, we take as given that the introduction of no-fault unilateral divorce resulted in a decline in women's bargaining power. This assumption is driven by the lack of social safety nets for divorced women, and the general absence of alimony or child support. We investigate this further in the main text by examining the results by the marital property regime.

In this section, we test the validity of this assumption. We follow Dunbar et al. (2013) to identify the share of household resources controlled by women, which we will use to infer bargaining power. The test has a narrow focus; we model changes in consumption patterns, but abstract from changes in labor supply and do not explicitly model gender norms against working (though they are likely present). One can view this analysis as a sub-problem to the framework discussed in Section 4, where consumption and labor decisions are separable.[22] The idea behind the test is that conditional on household expenditure, the spouse who consumes more following the introduction of no-fault unilateral divorce must have more bargaining power. Since individual consumption is not directly observable, we require a model of intra-household resource allocation to identify it.

Model: The setup builds upon seminal work by Chiappori (1988, 1992); Apps and Rees (1988); Browning et al. (1994) and Browning and Chiappori (1998), and specifically on more recent work by Browning et al. (2013) and Dunbar et al. (2013). The main alteration is that we emphasize the role of unilateral divorce in the household decision making process. We omit details of the derivation of the model and only summarize the aspects relevant to our analysis. More detail can be found in Dunbar et al. (2013).

We model *nuclear* households, defined as households that consist of a married man and woman, where person types are indexed by the subscript $j \in \{m, f\}$. While the household may include children, men and women are the only decision makers within the household and they bargain over how to allocate the household budget. As this is a collective model, households are assumed to reach a Pareto efficient allocation of goods.[23] Bargaining power within the household is a function of each spouse's outside option, and is therefore in part determined by their state's divorce laws. The goal is to uncover how bargaining power changes as a result of these laws.

Following Browning et al. (2013), we allow for the existence of public, private, and partially shared goods through a consumption technology function A, which converts what the household purchases

[22] The lack of reliable wage data for informal work, and the high percentage of women who do not work complicate any estimation and identification of a collective model of labor supply.

[23] Pareto efficiency is a testable assumption. It has not been rejected in a variety of different settings: Browning and Chiappori (1998); Bobonis (2009); Attanasio and Lechene (2014); Calvi (Forthcoming); Brown et al. (2018). However, there are notable exceptions where Pareto efficiency is rejected. See, for example, Udry (1996).

x into goods that are consumed by individuals given by c_j. The man and woman each have their own utility function $U_j(c_j)$. We write the household's problem as follows:

$$\max_{c_m, c_f} \tilde{U}[\ U_m(c_m), U_f(c_f),\ p/y\ ] = \mu_m(p/y, Uni)U_m + \mu_f(p/y, Uni)U_f$$
$$\text{such that} \tag{A3}$$
$$y = x^{'}p \ \text{ and } \ x = A[c_m + c_f]$$

where $\tilde{U}$ exists by Pareto efficiency and μ_j are the Pareto weights. Household expenditure is given by y. We denote the state divorce regime with the variable Uni, which enters each spouse's Pareto weight, and therefore has a direct effect on their bargaining power within the marriage.

Solving this program results in bundles of private good equivalents. Pricing these goods at within household shadow prices $A^{'}p$ allows us to calculate resource shares η_m, defined as the share of the total household budget controlled by the man in the household. This includes consumption of private goods, public goods, and partially shared goods. It follows that $\eta_f = 1 - \eta_m$ of the household budget is controlled by the woman. As η_m increases, the husband has greater bargaining power, and therefore has more control of the budget.[24] While resource shares are a function of many household characteristics, we are primarily interested in the causal relationship between unilateral divorce laws and resource shares.

Identification: To identify resource shares (and therefore changes in bargaining power), we rely on private assignable goods. A good is private if it is not shared, and a good is assignable if the econometrician can determine who in the household consumed it. The only available good that fits this description in our data is clothing.[25] The key advantage of focusing on these goods is that the demand functions will only depend on the preferences and resource shares of a single household member. Following Dunbar et al. (2013), we derive household-level Engel curves (i.e., demand functions holding prices fixed) for the private assignable goods. We specify a PigLog indirect utility function which results in Engel curves that are linear in log expenditure:

$$W_m = \eta_m\ [\alpha_m + \beta_m \ln(\eta_m)\ + \beta \ln y]$$
$$W_f = (1 - \eta_m[\alpha_f + \beta_f \ln(1 - \eta_m) + \beta \ln y] \tag{A4}$$

[24] Because the household is Pareto efficient, we can alternatively use duality theory to redefine the household's problem as a two stage process: In the first stage, resources are optimally allocated between the husband and wife. That is, the wife is allocated $\eta_m y$ and the husband $(1 - \eta_m)y$. In the second stage, each individual maximizes their own utility subject to their within household budget constraint which is determined by their share of household resources η_j and the shadow price vector $A'p$.

[25] If one spouse derives utility from the clothes that the other spouse wears, that would violate the assumption that clothing is private. We follow Dunbar et al. (2013) and test this assumption by comparing clothing preferences for single and married individuals. We find that they are not statistically different, which provides supportive evidence that clothing is private. This test, however, is not conclusive, as it requires that preferences do not change when individuals marry.

where W_j is the budget share for men's or women's clothing, and α_j and β_j are clothing preference parameters. Dunbar et al. (2013) require several assumptions to identify resource shares, all of which have empirical support. First, resource shares are assumed to be independent of household expenditure.[26] Second, Dunbar et al. (2013) restrict preferences to be similar in a limited way across people. In words, men and women must have the marginal propensity to consume clothing (i.e., $\beta_j = \beta$).[27] Finally, clothing must be private. That is, men and women cannot derive utility from their spouse's clothing expenditures. Resource shares are identified by inverting these Engel curves and implicitly solving for η_m. In practice, the model is identified using an OLS-type regression of the household-level budget share W_j on log expenditure $\ln y$. This identifies the slope of the Engel curve $\kappa_j = \eta_j \beta$. Then since resource shares sum to one, we have that $\sum_j \kappa_j = \sum \eta_j \beta = \beta \sum_j \eta_j = \beta$. Solving for resource shares, we have $\eta_j = \kappa_j / \beta$.[28]

Estimation: We use five waves for the National Household Income and Expenditures Survey (ENIGH) spanning the years 2008 to 2018. The key data requirements necessary for the structural model are household-level expenditure on a private assignable good (clothing) for both men and women.[29] The ENIGH also includes detailed demographic information about the household. In the estimation, we separate households by those with children and those without, because household behavior may be systematically different across these household compositions. We therefore provide summary statistics for these two samples in Table A11. In conducting the test, we account for observable heterogeneity in education, age, employment status (as a proxy for wealth), and whether the household is located in an urban or rural area.

We select a subsample of households that contain a single married couple with zero to three children. We therefore exclude households that have multiple adult men or women. The reason for this exclusion is that it facilitates our interpretation of female bargaining power; since we only observe women's clothing, but not individual-level clothing, we can only identify total women's resource shares. Having multiple women in the household would complicate our interpretation of women's bargaining power. We drop households in the top or bottom percentile of total household expenditure

[26] Menon et al. (2012) show the assumption to be quite reasonable. Moreover, this assumption only has to hold at low levels of expenditure.

[27] Bargain et al. (2018) have tested several aspects of the collective model including this assumption. They use a unique data set in Bangladesh that contain *individual-level expenditure data for all private goods*, including assignable clothing. They estimate a collective household model as if they only observe assignable clothing, and compare their results to observed resource shares. Their results more generally provide empirical support for using clothing expenditures to infer how total resources are allocated.

[28] Identification works best when the Engel curves have nonzero slope as discussed in Tommasi and Wolf (2018), which is satisfied in our context. See Figure A2.

[29] One might be concerned that there are consumption externalities from clothing consumption (e.g., the husband gets utility from the wife wearing nice clothes) which would invalidate the model. To test this assumption, we follow Dunbar et al. (2013) and compare preferences for single men (women) to married men (women), and find that their clothing preferences are not statistically different, which provides suggestive evidence that clothing is private.

in each wave to eliminate outliers, as well as households with men or women under age 22 or over 65. Lastly, we exclude households with missing values for any of our covariates.

Since expenditure is potentially endogenous due to measurement error, we follow the literature (see e.g., Lewbel (1996)) and use income as an exogenous instrument and estimate the model via Hansen (1982)'s Generalized Method of Moments.[30] Let q_j be an $L \times 1$ vector of instruments. Then $E(\epsilon_j q_l) = 0$ for all j and l. For instruments, we interact our vector of household demographic characteristics X_j, log income, and log expenditure with X_j. We add an error term to the Engel curves given in Equation (A6), and can then write the moments as follows:

$$\begin{aligned} &E[(W_m - \ \eta_m \ [\alpha_m + \beta_m \ln(\eta_m) \ + \beta \ln y]) \ q_{ml}] = 0 \\ &E[(W_f - (1-\eta_m)[\alpha_f + \beta_f \ln(1-\eta_m) \ + \ \beta \ln y]) \ q_{wl}] = 0 \end{aligned} \tag{A5}$$

We introduce observable heterogeneity by allowing each parameter to be a function of household characteristics. This includes demographic characteristics such as the age and education of each household member, but also state and year fixed effects.[31] Moreover, we allow resource shares to depend on the divorce law regime in the household's state of residence. For $j \in \{m, f\}$:

$$\eta_j = \delta^{\eta_j} X_i + \gamma_t^{\eta_j} + \psi_s^{\eta_j} + \phi Uni_s \times Post_t \tag{A6}$$

where X_i is a vector of household demographic characteristics, γ_t are year fixed effects, ψ_s are a vector of state fixed effects, and $Uni_s \times Post_t$ is an indicator for whether state s in year t allows unilateral divorce. We model clothing preference parameters α_j or β, similarly, though we assume that the divorce regime only affects the household demand for assignable clothing through its affect on resource shares (i.e., we consider divorce laws a distribution factor as in Chiappori et al. (2002)).

Results: Table A12 presents the effect of the introduction of unilateral divorce on women's resource shares. The parameter of interest is $Uni \times Post$ which can be interpreted as the two-way fixed effect estimate of the implementation of unilateral divorce on women's bargaining power within the household. This parameter originates from Equation (A6) which is estimated within System (A5).[32]

In Column (1), we see that women's resource shares declined by 1.1 percentage points due to

[30] The use of income as an instrument for expenditure is common in the demand literature and dates to Liviatan (1961). The necessary assumption is that any measurement error in current income is uncorrelated with measurement error in household expenditure. More recent examples of its use include Banks et al. (1997) and Browning et al. (2013).

[31] In some sub-samples, we omit state-fixed effects in order to ensure the model converges.

[32] Table A13 reports summary statistics of the predicted resource shares. Women's resource shares are slightly higher than previous estimates in the Mexican context which find women control slightly less than half of the budget (Tommasi, 2019). However, these results use data from the late 1990s and early 2000s, and we believe it's likely women's empowerment has improved in recent years. We report other covariates from the resource share function in Table A14. Clothing preferences parameters are also estimated, but are also not necessarily of interest, so we omit them from our discussion.

Table A11: Descriptive Statistics

PANEL A: HOUSEHOLDS WITHOUT CHILDREN

	Mean	Median	Std. Dev.	Min.	Max.
Household Characteristics:					
Treat × Post	0.608	1	0.488	0	1
Women Working	0.491	0	0.500	0	1
Women Secondary Schooling	0.568	1	0.495	0	1
Men Secondary Schooling	0.585	1	0.493	0	1
Women's Age	48.885	52	11.463	22	65
Men's Age	51.268	55	11.517	22	65
Urban	0.413	0	0.492	0	1
Year:					
2008	0.106	0	0.308	0	1
2010	0.113	0	0.317	0	1
2012	0.037	0	0.189	0	1
2014	0.087	0	0.282	0	1
2016	0.315	0	0.464	0	1
2018	0.342	0	0.474	0	1
Household Expenditures:					
Women's Clothing Budget Shares	0.016	0.004	0.025	0	0.367
Men's Clothing Budget Shares	0.015	0	0.025	0	0.337
Total Expenditure (K)	22.393	15.926	19.492	2.345	140.205
N = 9,528					

PANEL B: HOUSEHOLDS WITH CHILDREN

	Mean	Median	Std. Dev.	Min.	Max.
Household Characteristics:					
Treat × Post	0.542	1	0.498	0	1
Women Working	0.489	0	0.500	0	1
Women Secondary Schooling	0.768	1	0.422	0	1
Men Secondary Schooling	0.756	1	0.429	0	1
Women's Age	35.498	35	7.773	22	65
Men's Age	38.243	37	8.356	22	65
Urban	0.380	0	0.485	0	1
Proportion Female Children	0.483	0.5	0.383	0	1
Average Children's Age	8.531	8.5	4.235	0	17
Number of Children	1.992	2	0.740	1	3
Year:					
2008	0.146	0	0.353	0	1
2010	0.131	0	0.337	0	1
2012	0.040	0	0.195	0	1
2014	0.088	0	0.284	0	1
2016	0.309	0	0.462	0	1
2018	0.286	0	0.452	0	1
Household Expenditures:					
Women's Clothing Budget Shares	0.009	0	0.016	0	0.324
Men's Clothing Budget Shares	0.008	0	0.017	0	0.359
Total Expenditure (K)	21.019	16.496	15.516	1.903	117.545
N = 36,562					

NOTE: The sample in Panel A includes married couples age 22 to 65 with no co-resident children. The sample in Panel B includes married couples age 22 to 65 with 1 to 3 children. Total Expenditure is quarterly nominal expenditure and is reported in thousands of pesos. SOURCE: National Household Income and Expenditures Survey (ENIGH) for the years 2008, 2010, 2012, 2014, 2016, and 2018.

Table A12: Effect of Unilateral Divorce on Women's Bargaining Power

	Main Results			Heterogenous Effects		
	All Households (1)	One to Three Children (2)	No Children (3)	Property Regime (4)	Labor Supply (5)	Marital Instability (6)
Uni × Post	-0.0116**	-0.00954	-0.0145**	-0.0213***	-0.0153**	-0.00886
	(0.00579)	(0.0109)	-0.00644	(0.00826)	(0.00690)	(0.00756)
Uni × Post × Comm.				0.0127*		
				(0.00739)		
Uni × Post × Other				0.01000		
				(0.0101)		
Wife Employed					0.0263***	
					(0.00936)	
Uni × Post × Wife Emp.					0.00494	
					(0.00698)	
Marital Instability						7.302***
						(1.830)
Uni × Post × Marital Inst.						0.368
						(0.724)
N	46,090	36,562	9,528	46,090	46,090	46,090
State FE	X	X		X	X	X
Year FE	X	X	X	X	X	X

NOTES: * p<0.1, ** p<0.05, *** p<0.01. All couples are between the ages of 22 and 65. Robust standard errors in parentheses. Controls include the age and education of the husband and wife, the number of children, average child age, proportion of female children, and whether the household resides in an urban area. *Community* (*Separate*) is an indicator equal to one if the individual lives in a state where the majority of marriages were community (separate) property in 2011. The *Other* category includes states which use a combination of community and separate property asset division. *Wife Emp* is an indicator equal to one if the wife is currently employed. *Marital Instability* is a measure of each women's predicted divorce probability. SOURCE: National Household Income and Expenditures Survey (ENIGH) for the years 2008, 2010, 2012, 2014, 2016, and 2018.

the implementation of no-fault unilateral divorce. This result provides support for our assumption in Section 4 that women are disproportionately harmed by more liberal divorce laws. In Columns (2) and (3), we separate the sample by the presence of children in the household. We only find a statistically significant decline in bargaining power for women without children, however, the coefficient on *Uni* × *Post* is not statistically different across samples. The lack of any difference is in some ways surprising. One might expect that women with children face more financial risk should the couple divorce, and thus may experience a greater decline in bargaining power to ensure their husband does not divorce them. We do not find evidence supporting this hypothesis. Instead, we see no difference, which may be due to a higher risk of divorce for childless couples, as fathers may not want to lose custody of their children.

We next investigate several potential sources of heterogeneity. First, the relationship between unilateral divorce and bargaining power may vary with the marital property regime. As discussed in

Section 7, women in community property marriages are unlikely to be as harmed by unilateral divorce as those in separate property marriages. Thus, we may expect unilateral divorce to decrease women's bargaining power more in separate property regimes. To account for this, we allow the results to vary with the state-level measures of the divorce property regime. If over 50 percent of 2011 marriages are governed by a community property regime, then we classify the state as community property. We do the same for separate property marriages.[33] Our results are somewhat consistent with this story. In Column (4) of Table A12 we find a 2.13 percentage point decline in bargaining power among women living in separate property states (the omitted category). Moreover, we find this decline in bargaining power is not as large in community property states, as the interaction of $Uni \times Post \times Community$ is positive.

We next investigate heterogeneity by labor supply. As discussed in Section 7, employed women increased their hours worked in response to the introduction of unilateral divorce. It's possible that an increase in labor income may offset any decline in women's bargaining power. To test this hypothesis, we allow the effect of unilateral divorce laws on bargaining power to vary with labor force participation. These results are presented in Column (5) of Table A12.[34] We find no evidence that the relationship between unilateral divorce and bargaining power varies with whether the woman is employed. This finding has several implications. First, it suggests that there are women who experienced a decline in bargaining power, but did not respond by entering the labor force. As discussed in detail Section 4, gender norms against women working is a potential explanation for this result. Second, it suggests that working more may be insufficient in compensating for the decline in bargaining power due to divorce laws. However, we hesitate to over-interpret these results, as we are not modeling the labor supply decision.

Finally, we examine heterogeneity by marriage stability. We may expect that couples with a low divorce probability are unaffected by more liberal divorce laws, while couples with a higher divorce probability do respond. To examine this, we begin by predicting divorce probabilities using the ENOE (recall the ENIGH is a repeated cross section). We then use these parameter estimates to predict the probability the couple will get divorced in the subsequent period. These results are presented in Column (6) of Table A12. We see no difference. The lack of any effect could be due to the coarse measure of instability that we are using, or that the couples most affected actually divorced, and we are working with a select sample.

In summary, we find evidence that women's bargaining power declined in response to the introduction of unilateral divorce. This validates the underlying assumption in the conceptual framework

[33] Some states did not report the property regime. For these states, we use the default property regime.

[34] We report results by the presence of children in Columns (4) to (6) in Table A14 (as well as the other parameters of the resource share function). There is some evidence that the negative consequences of unilateral divorce are lower for working women without children.

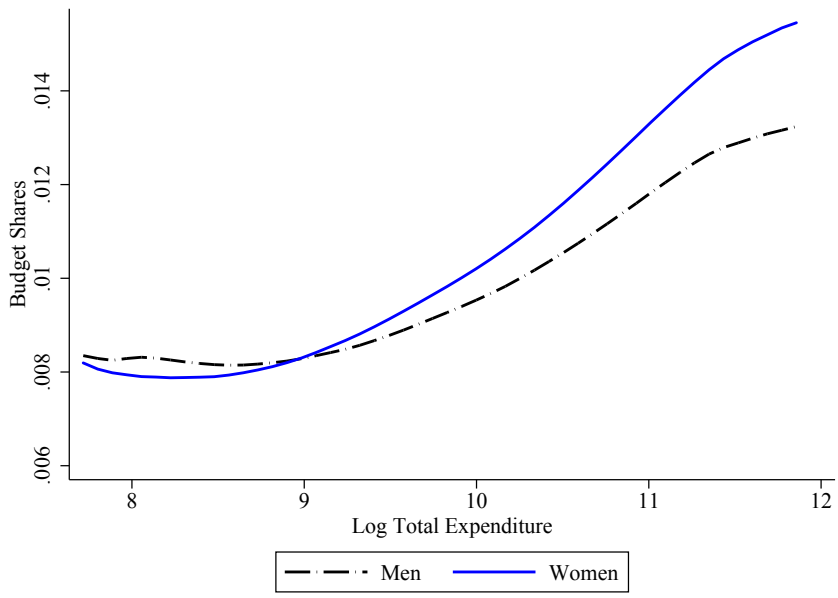
Table A13: Predicted Resource Shares

	Mean	Median	Std. Dev.	Min.	Max.	Obs.
Women	0.533	0.537	0.069	0.240	0.792	46,090
Men	0.467	0.463	0.069	0.208	0.760	

NOTES: Descriptive statistics for the predicted resource shares across the estimation sample. SOURCE: National Household Income and Expenditures Survey (ENIGH) for the years 2008, 2010, 2012, 2014, 2016, and 2018.

regarding this relationship. Nonetheless, there are several caveats to the present analysis that suggest caution should be taken in interpreting these results. First, while we have attempted to account for the marital property regime, we have only done so in a coarse way. It is possible we are missing heterogeneity in the marriage-level property regime that state-level default laws are unable to capture. Second, we are only examining the effect on bargaining power for women who remain married after the introduction of unilateral divorce. A more complete analysis would examine women who did divorce, as well as their subsequent remarriage rates. Given these caveats, we view this analysis as suggestive that the underlying assumptions regarding the relationship between bargaining power and unilateral divorce in Section 4 is valid. However, more work needs to be done, likely with better data, to examine the sensitivity of these results.

Figure A2: Non-Parametric Clothing Engel Curves



SOURCE: National Household Income and Expenditures Survey (ENIGH) for the years 2008, 2010, 2012, 2014, 2016, and 2018. NOTES: Non-parametric clothing Engel curves for men and women.

Table A14: Determinants of Women's Resource Shares

	Excluding Labor Supply			Labor Supply Interaction		
	All Couples	With Children	Without Children	All Couples	With Children	Without Children
	(1)	(2)	(3)	(4)	(5)	(6)
Uni × Post	-0.0116**	-0.0145**	-0.00954	-0.0153**	-0.0152**	-0.0232*
	(0.00579)	-0.00644	(0.0109)	(0.00690)	(0.00775)	(0.0130)
Uni × Post × Wife Employed				0.00494	-0.00236	0.0234*
				(0.00698)	(0.00789)	(0.0135)
Wife Employed				0.0263***	0.0134	0.0139
				(0.00936)	(0.0108)	(0.0173)
Age Woman	0.619	0.996	-0.325	0.470	0.916	-0.436
	(0.471)	-0.646	(0.776)	(0.469)	(0.639)	(0.787)
Age Man	-0.0934	0.176	-1.447*	-0.0334	0.367	-1.541*
	(0.469)	-0.611	(0.787)	(0.467)	(0.600)	(0.813)
Age Woman^2	-1.135**	-1.387*	1.226	-0.931*	-1.271	1.133
	(0.554)	-0.807	(0.850)	(0.553)	(0.800)	(0.860)
Age Man^2	0.203	0.0237	0.780	0.163	-0.166	1.154
	(0.527)	-0.701	(0.824)	(0.527)	(0.689)	(0.849)
Education Man	0.0355***	0.0477***	-0.0767***	0.0349***	0.0472***	-0.0751***
	(0.0127)	-0.0162	(0.0205)	(0.0126)	(0.0157)	(0.0208)
Education Woman	0.0470***	0.0427***	0.125***	0.0459***	0.0392**	0.114***
	(0.0127)	-0.016	(0.0245)	(0.0127)	(0.0157)	(0.0245)
1(Urban)	-0.00257	0.00269	-0.00160	-0.00476	0.00313	-0.000886
	(0.00971)	-0.0118	(0.0156)	(0.00970)	(0.0116)	(0.0157)
Age Child	0.708***	0.566***		0.656***	0.498***	
	(0.148)	-0.166		(0.149)	(0.165)	
Proportion Girls	-0.0247*	-0.0193		-0.0246*	-0.0180	
	(0.0140)	-0.0135		(0.0139)	(0.0133)	
No Children	-0.0138			-0.0177		
	(0.0219)			(0.0219)		
One Child	0.0110			0.00375		
	(0.0155)			(0.0155)		
Two Children	0.0115	-0.0206		0.00727	-0.0202	
	(0.0139)	-0.0132		(0.0138)	(0.0129)	
Three Children		-0.0383**			-0.0377**	
		-0.0152			(0.0150)	
Constant	0.564***	0.614***	0.583***	0.559***	0.623***	0.569***
	(0.0366)	-0.0489	(0.0302)	(0.0363)	(0.0473)	(0.0311)
Sample Size	46,090	36,562	9,528	46,090	36,562	9,528
State FE	X	X		X	X	
Year FE	X	X	X	X	X	X

NOTES: * p<0.1, ** p<0.05, *** p<0.01. The full sample includes all households with 1 married couple, and 0 to 3 children. Robust standard errors in parentheses. Age variables are divided by 100 to ease computation. SOURCE: National Household Income and Expenditures Survey (ENIGH) for the years 2008, 2010, 2012, 2014, 2016, and 2018.