The Impact of Unilateral Divorce in Mexico: Bargaining Power and Labor Supply

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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 significantly change their employment patterns. Employed married women slightly increase their hours worked, but the effect is not large enough to be observed in the full sample of women. Past work has suggested that divorce laws impact labor supply through changes in women's marital bargaining power. We study this relationship using a structural, collective model and find that women experienced an economically insignificant decline in bargaining power following the reform.
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1 Introduction

Divorce is common in high-income countries, 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 social safety nets make 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 from 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 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 affected couples who *remain married* after the introduction of more liberal divorce laws.

We begin by analyzing how the legislation affected married women's labor supply. We use household-level, quarterly panel data from *The National Occupation and Employment Survey* (ENOE), which 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 change 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. To address the fact that asset division has been shown to affect how women respond to unilateral divorce laws (Gray, 1998; Voena, 2015), we then examine how the results differ by the state's marital property regime. 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] These results suggest that there may be heterogeneity within the sample that cancels out the aggregate effect.

Overall, our labor supply results differ slightly from existing work on divorce laws in developed countries, as we do not find a large increase in female employment (see Stevenson (2008) and Bargain et al. (2012)). This difference suggests that the setting matters. 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 there ability to adjust their labor supply (Silverio-Murillo, 2019; Hoehn-Velasco and Silverio-Murillo, 2020). This hypothesis is consistent with hours worked increasing only among women who already work. 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 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 measure allows us identify some heterogeneity by property regime, this may not be sufficient to fully disentangle the relationship.

To better understand our labor supply results, we then investigate one potential mechanism. Past work has hypothesized that unilateral divorce laws may increase women's labor supply as a result of changes in women's bargaining power (e.g., Bargain et al. (2012)). The spouse with the superior outside option has a higher threat point and therefore has more control over household decision making. This change in bargaining power manifests itself through changes in labor supply. The purpose of this exercise is to determine whether 1.) there was no change in bargaining power, which would partially explain the minimal change in female employment, or 2.) there was a change in bargaining power, but cultural norms or other labor market frictions

[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.

prevented women from working more.

A key challenge in answering this question is that bargaining power is unobservable. To address this issue, we study how the introduction of unilateral divorce affects married women's bargaining power using a structural model of intra-household consumption allocation. We use the collective household framework (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.[2] 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. Specifically, we exploit variation in how men's and women's clothing budget shares vary with household expenditure to identify resource shares (and therefore women's bargaining power).[3]

We estimate the model using consumption and expenditure data from the *Encuesta Nacional de Ingresos y Gastos de los Hogares* (ENIGH) survey. Within the framework of the structural model, we compare the resource shares of married couples across treated and untreated states, before and after the reforms. We find no change in women's bargaining power originating from the introduction of unilateral divorce. There is, however, some evidence that women with children experienced a small decline in bargaining power. We attribute this small effect to several factors. First, marital property laws partially determine the relationship between divorce laws and bargaining power. Depending on the unobservable marital property regime, either spouse could benefit from more liberalized divorce laws as the marital outside option is, in part, determined how property would be divided in a potential divorce. The net effect of differing property laws across marriages may result in the average effect being

[2]Resource shares are similar to Pareto weights, but do not depend on how utility is cardinalized. Resource shares are therefore a useful measure of bargaining power (Browning et al., 2013).

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

close to zero. Second, there is selection into which couples divorce as a result of these laws and which remain married. If all marriages had remained intact, we might have observed changes in bargaining power. However, since these couples divorce, the remaining sample of married women are, in effect, not treated. We analyze the magnitude of this problem by studying how our results vary by match quality.

This paper makes several contributions. First, we add to the ongoing debate 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). As discussed above, the findings of these studies appear context dependent and not generalizable to a middle-income context. Our work therefore contributes to the growing literature on the impact of divorce reforms in middle-income countries, particularly in Mexico. We add to this literature by directly linking the labor supply response to estimated changes in married women's bargaining power.

Second, we add additional context to our reduced-form results by structurally estimating the household bargaining effects of no-fault divorce. Most of the existing literature has attributed changes in labor supply to changes in bargaining power, without empirically testing this hypothesis. We use exogenous variation in divorce laws within a structural model to identify the causal effect of these laws on women's bargaining power. Our approach relates to prior work by Chiappori et al. (2002) and Voena (2015). We discuss how we differ from these studies in Section 2.

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 the Mexico. In Section 4 we summarize the ENOE and ENIGH surveys. In Section 5, we discuss the empirical strategy and results from our reduced-form analysis of the relationship between divorce laws and labor supply. In Section 6 we structurally analyze the relationship between divorce laws and bargaining power to better understand our reduced-form results. Section 7 concludes.

2 Literature Review

Unilateral divorce laws have become more widespread in recent years. These laws have been shown to increase divorce rates in a variety of different contexts including the United States (Friedberg, 1998; Wolfers, 2006), several countries in Europe (González and Viitanen, 2009; Kneip and Bauer, 2009), and more recently in Mexico (Hoehn-Velasco and Penglase, Forthcoming). How do divorce laws affect couples who remain married? Given that divorce is now a credible threat, the spouse who values exiting the marriage the most is likely to benefit from the new divorce regime. This shift in bargaining power within the marriage is likely to have a corresponding effect on household behavior.[4] A large literature has analyzed this hypothesis along several different dimensions, including labor supply, savings decisions, and investments in children. Our study relates primarily to work on the relationship between unilateral divorce laws and labor supply.

A variety of studies (Peters, 1986; Parkman, 1992; Genadek et al., 2007; Stevenson, 2008; Bargain et al., 2012) in several different contexts find that the introduction of unilateral divorce laws increased women's labor supply. These results are often attributed to women wanting to insure themselves against divorce. Gray (1998) studies the consequences of unilateral divorce laws in the United States, and somewhat counterintuitively, 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. More recent work by Heath and Tan (2014) in India finds a positive relationship between women's bargaining power and labor supply. In contrast, Chiappori et al. (2002) interpret higher labor supply as a decrease in bargaining power due to the decline in leisure, though they do not incorporate household production in their analysis. An alternative ex-

[4] See Chiappori et al. (2015) for a detailed model of divorce.

planation, discussed in Stevenson (2007) and Stevenson (2008), attributes changes in women's labor supply 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.

We contribute to this literature in two ways. First, our study addresses the conflicting evidence on the relationship between divorce laws, labor supply, and bargaining power by directly estimating changes in market work, household work, leisure, and bargaining power. Second, we add to this literature by analyzing the effects of unilateral divorce on labor supply in a middle-income economy. The wellbeing of women in a Mexico is an important policy issue and divorce laws appear to have different effects by gender. Existing work on the introduction of unilateral divorce in Mexico has focused on the effect on divorce rates and domestic violence (Lew and Beleche, 2008; Garcıa-Ramos, 2017; Hoehn-Velasco and Penglase, Forthcoming).

Our study also relates to recent work on the relationship between household bargaining power and divorce laws. Similar to our study, Chiappori et al. (2002) structurally estimate the relationship between divorce laws, bargaining power, and labor supply. The authors extend the collective labor supply model (Chiappori, 1988, 1992; Apps and Rees, 1988) to include distribution factors, such as divorce laws and the sex ratio.[5] Voena (2015) extends Chiappori et al. (2002) to a dynamic setting and incorporates asset accumulation and marriage into a life-cycle structural model. Like these studies, we examine the effects of unilateral divorce on both labor supply and bargaining power within the marriage.

We differ in two respects; First, instead of using a labor supply model, we infer bargaining power from a collective model of resource allocation (Browning et al., 2013; Dunbar et al., 2013). Because we focus on a middle-income country, female la-

[5]Distribution factors are variables that affect bargaining power within the household, but not preferences for goods.

bor supply in market work is uncommon, and wage data is unavailable for most of the sample. By contrast, we observe detailed consumption data. We add to the growing number of studies that have structurally estimated women's bargaining power in the developing world using this framework.[6] Our second difference is that we can conduct a more causal analysis. Our estimation allows for the inclusion of year and state-fixed effects in identifying the relationship between divorce laws and women's bargaining power, and thus our approach resembles a difference-in-difference identification strategy within a structural model. Voena (2015) is also able to incorporate this type of variation to identify the key model parameters using an indirect inference approach. A weakness of our paper relative to these existing studies is that we are unable to account for the property regime of the marriage, which may impact our results.

Finally, a different strand of research uses dynamic life-cycle models of marriage and divorce to better understand the evolution of married women's labor supply over time. These models estimate, for example, what share of increased labor force participation can be attributed to changes in divorce laws, relative to other potential factors, such as changes in female wages, the marriage market, or access to birth control. Important papers in this literature include Fernández and Wong (2014) and Eckstein et al. (2019) (as well as the previously cited Voena (2015)). Our study complements this literature by focusing on a single potential cause: changes in marital bargaining power.

3 The Mexican Context

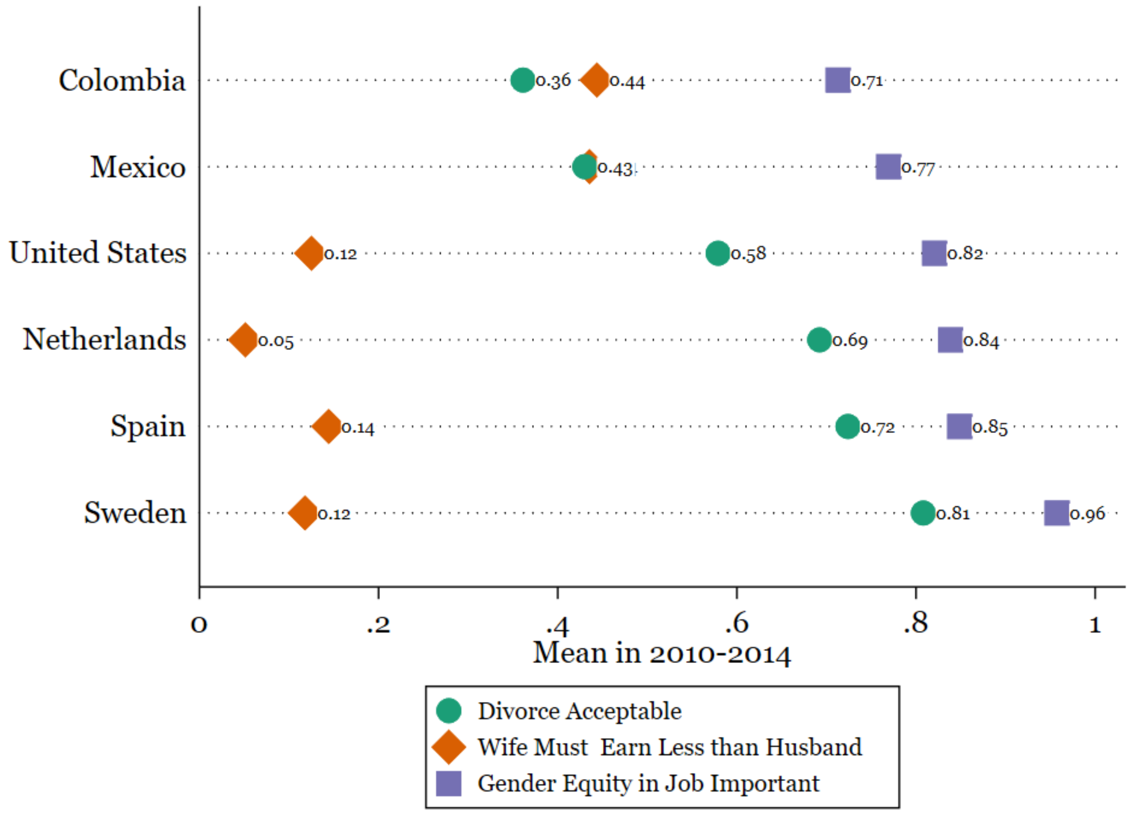
For most of its history, obtaining a divorce in Mexico has been an arduous process. The divorcing spouse had to prove cause (e.g., domestic violence or infidelity), or both spouses needed to (mutually) consent to the divorce. Legal reforms in the 1990s and early 2000s relaxed some of the hurdles, but it was not until 2008 when couples were able to divorce without legal grounds. At that time, Mexico City became the first state

[6] See, for example, Bargain et al. (2014); Calvi (Forthcoming); Calvi et al. (2017); Tommasi (2019).

to implement no-fault unilateral divorce, which eventually spread throughout Mexico over the next ten years. This legislation was a stark change from the existing divorce regime, as it allowed one spouse to dissolve the marriage without agreement across the married couple.

Existing research covering these reforms have highlighted the uniquen features of the Mexican context.[7] While these studies have demonstrated that divorce rates have increased as a result of the reforms, they have also shown that women are less able to escape abusive relationships as compared with high-income countries (Hoehn-Velasco and Silverio-Murillo, 2020).

Figure I: Cultural Differences between Countries, World Values Survey



NOTES: Figure reports how important individuals in each country view religion, the acceptance of divorce, and views on gender equality in the workplace. All three measures calculated on a scale from 0 to 1.
SOURCE: World Values Survey, Wave 6 (2010-2014).

To illustrate the distinct values across contexts, we use the most recent wave of

[7]See, for example, Lew and Beleche (2008), Garcıa-Ramos (2017), Hoehn-Velasco and Penglase (Forthcoming), and Silverio-Murillo (2019).

the *World Values Survey* to compare perspectives on divorce and gender equality in the workplace. Figure I presents cultural attitudes towards gender equality and divorce in two representative Latin American countries, three European countries, and the United States.[8] 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 each view divorce as acceptable and hold gender equality in higher importance. In the United States, fewer individuals emphasize that a wife must earn less than her husband, and the U.S. generally places a higher emphasis on gender equity.

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 outside of high-income countries. Due to differences in cultural values, we expect the findings in Mexico to differ in two main ways. First, due to rigid cultural interpretations of divorce, Mexican couples that remain married may be systematically different from U.S. households. This claim is supported by evidence in related work (García-Ramos, 2017; Hoehn-Velasco and Silverio-Murillo, 2020), which suggests that the most violent households did not dissolve after the introduction of unilateral divorce.[9] These cultural differences are relevant because they suggests divorce may be less of a credible threat in Mexico relative to 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.

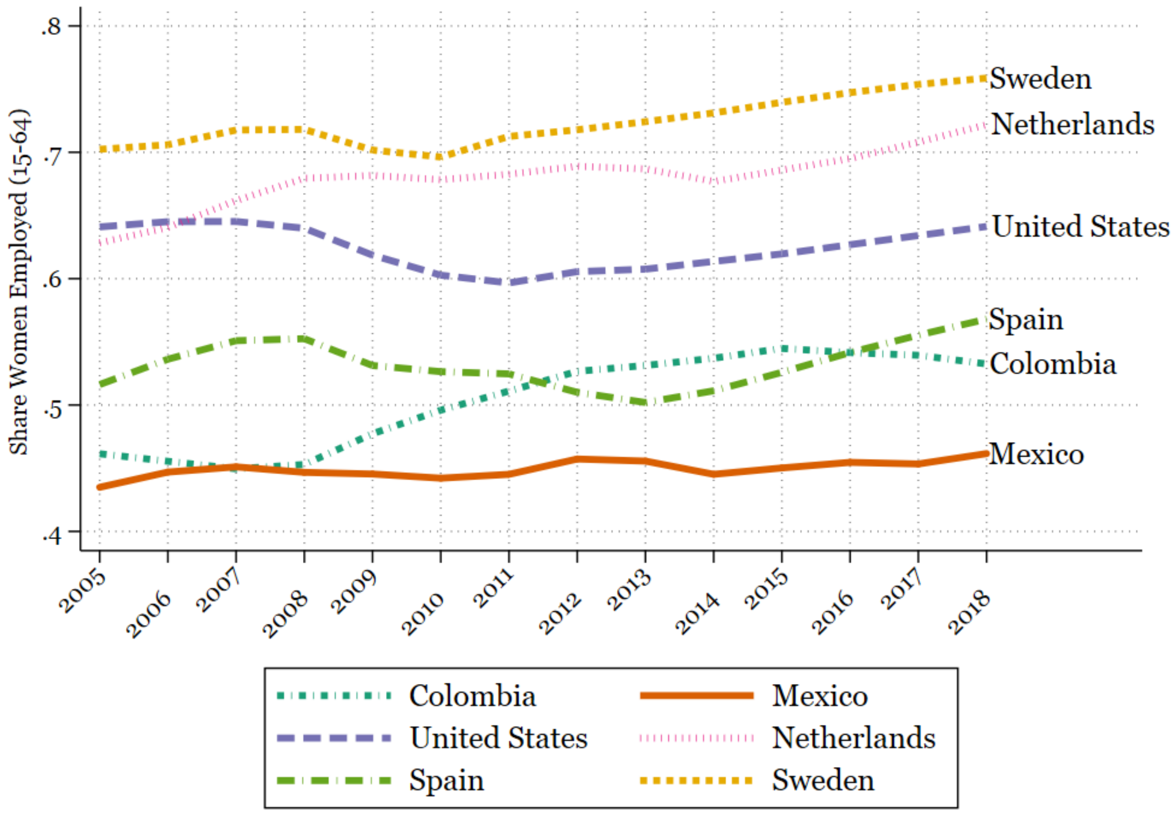
Second, social stigma may prevent women from entering the labor market. As

[8]We choose Spain because it has been studied closely in other contexts, but are unable to include Ireland as it is not reported as a separate country in the World Values survey.

[9]In Mexico, women appear less able to escape abusive relationships due to partner-imposed emotional and economic violence (Silverio-Murillo, 2019).

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). Figure II compares women's labor supply across Mexico and the countries shown in Figure I. We see that less than 50% of Mexican women participate in the labor force, which is lower than in Colombia. Based on the low employment, we hypothesize that Mexican women may prefer to increase their labor supply in the presence of unilateral divorce laws, but may be unable to due to this social stigma.

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.

4 Data

4.1 The National Occupation and Employment Survey

We take 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. The ENOE data is the largest continuous data project in Mexico.

Table 1 presents summary statistics. We restrict the sample to married women age 22 to 65 who are in the sample pre and post reform. In the main sample, 40 percent of married women are employed, and these women work 15 hours per week on average. They also spend roughly seven hours on household tasks and 16 hours caring for others. The average woman in our sample has three children and is 43.

Table 1: Summary Statistics for Married Women

	Observations	Mean	Median	St. Dev.
1(Working)	211,457	0.41	0.00	0.49
Hours Worked	211,280	15.19	0.00	20.86
Age	211,457	43.02	43.00	11.04
Urban	211,457	0.60	1.00	0.49
Number Children	211,447	2.90	3.00	1.82
Youngest Child	126,811	8.50	8.00	5.40
Time on House	211,457	7.36	3.00	10.42
Time on House/Children	211,457	16.38	10.00	17.60
Time with Children/Elderly/Sick	211,457	9.02	0.00	13.48
Leisure Time	211,457	121.03	133.00	42.45

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

The benefit of the ENOE data is that it provides a panel of households for five quarters. This allows us to track the employment choices of each family member for up to one year before and after the reform. Each quarter, one-fifth of the sample is replaced within an incoming cohort. We use this short panel to observe how individuals respond to the change in legislation. For each woman in the sample, we compare their labor supply after the reform to what it was before in the pre-treatment period.

While the panel structure of the ENOE is beneficial, there are several limitations to the data. First, the survey only tracks individuals for five quarters which prevents us from using this data to study the long-run labor supply dynamics. However, we do not believe this is of too great a concern, as there is no a priori reason to suspect the effect should vary over time.[10] An additional concern is that there is attrition due to changes in household structure that occur during the study. Individuals may exit the sample if they become divorced or separated. If the wife divorces her husband, one member of the couple (either the husband or the wife) will necessarily leave the data. Other times both will exit the data. 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 afterwards, regardless of their marital status. Since we cannot fully do this, we expect a slight downward bias in the estimates. To explore whether households systematically drop out of the data in predictable ways, Table A2 shows the probability of leaving the sample based on the reform, as well as other individual characteristics. The sample includes married women who are 22 to 65. The results in Columns (1) to (3) suggest that individuals are no more likely to leave the panel after the reform. They are more likely to drop out if they have fewer children, and are younger. For the probability of leaving the sample due to divorce, the results are similar, except for a higher drop out rate for women with more education.

4.2 The National Household Income and Expenditures Survey

For the structural model, we use individual- and household-level data from the *National Household Income and Expenditures Survey* (*Encuesta Nacional de Ingresos y Gastos de Hogares or ENIGH*) over the years 2008, 2010, 2012, 2014, 2016, and 2018. The survey includes detailed information on income and consumption, as well as standard

[10] Nonetheless, we use a separate data set to study long-run effects in Section C.

demographic characteristics.

While the ENIGH has complete measures of household expenditures, which are necessary for estimating the structural model, the data is limited as it is a repeated cross section of households. Because we do not observe the marriage dates of individuals, we cannot limit our sample to only those who were married before the reform. Thus, the results here can only suggest whether states experienced changes in the labor supply of married women and not whether women married before the reform are responding to the legislation.

We do test whether our findings from the ENOE data are consistent across the ENIGH data in the Appendix. We repeat our labor supply results using the ENIGH data in Appendix Section C. We also show summary statistics related to labor supply in Table A10. However, because the empirical strategies differ, the results are only comparable across the full difference-in-difference results – without individual fixed effects – where we find no change in labor supply. The ENIGH shows the same lack of effect as the full sample difference-in-difference approach from the ENOE data.

4.3 Divorce Legislation Data

To measure the timing of the reform, we collect the quarter-year passage of unilateral no-fault divorce from the state-level civil and family laws.[11] Table A1 shows the year the legislation passed and the location of the divorce legislation in the state's legal code. A distinction is made between states that record divorce law in the civil codes versus family codes. There are two notable issues with the divorce reform data.

The first limitation is that the legal dates of the reform frequently differ from the observed dates of no-fault unilateral divorce in the INEGI data. We highlight this

[11] Note that 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) and Garcıa-Ramos (2017), who also study no-fault divorce in Mexico.

issue by separating *de jure* years from *de facto* years in Table A1.[12] Comparing across *de facto* years and *de jure* years, it is clear there are discrepancies between when a state passed the reform and when an individual could exercise the right 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 years).[13] A further seven states match years but the dates differ by more than one quarter.[14] Only four states exactly match between *de facto* dates and *de jure* dates, suggesting an immediate implementation of no-fault divorce in these states.[15] 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 put the legislation on the books. See Hoehn-Velasco and Penglase (2019) for a more detailed discussion.

5 Labor Supply

5.1 Empirical Strategy

To study the effect of unilateral divorce laws on labor supply and time use, we exploit state-level variation in the quarter-year adoption of the legislation. We follow individuals through the reform using the available panel of women in the ENOE. The rotating panel of individuals allows us to compare the labor supply of women for one year leading up to and one year following the reform. We choose the sample of women surrounding the reform as we want to ensure that women were married when

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

[13] These states include Aguascalientes, Baja California Sur, Colima, Mexico City, Nayarit, Sinaloa, an Zacatecas.

[14] States include Guerrero, Hidalgo, Morelos, Oaxaca, Sonora, Veracruz, and Yucatan.

[15] The four matching states include Coahuila, Mexico, Tlaxcala, and Puebla.

the reform took effect (Voena, 2015).[16]

Then, following a strategy similar to Voena (2015), we estimate the labor supply of married woman i in state s during quarter t as:

$$Y_{ist} = \alpha + \beta(\text{Uni}_s \times \text{Post}_t) + \mathbf{X}'_{it} + \pi_s + \tau_T + \alpha_i + \phi_i t + \epsilon_{ist} \tag{1}$$

where Y_{ist} is our labor supply outcome of interest, including employment and hours worked. $\text{Uni}_s \times \text{Post}_t$ indicates state-level adoption of the reform in at time t. X_{ist} is a vector of individual characteristics, which includes indicators for age. π_s are state fixed effects, and τ_T are the quarter-year fixed effects. α_i are individual-level fixed effects and $\phi_i t$ are individual trends for each quarter-year the person is in the sample. We cluster standard errors at the individual level (Bertrand et al., 2004).

The assumption underlying this specification is that women are changing their labor supply only as a response to the reform. Thus, if women are systematically increasing their labor supply over time, for reasons other than the reform itself, the estimates will be inflated. Individual-level trends help to address any linear changes in individual labor supply that are not due directly to the reform, such as women working more due to their children getting older.

Our empirical strategy addresses several potential threats to validity. First, Mexican states are far from identical, and have different cultural norms and legal histories related to divorce. For instance, 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 effect of these reforms from the effect of the 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,

[16]More specifically, we subset the sample to women who experience both pre-reform and post-reform periods.

individual, and year fixed effects to alleviate any concerns.

Another potential concern is about life cycle changes in labor supply unrelated to changes in divorce. For example, women may mechanically be increasing their labor supply due to leaving school and entering the labor force, or re-entering the labor force as their children enter primary school. To address concerns about women who are leaving school, we restrict the sample to women age 22 and over. To account for women working more as their children age, we include the previously discussed individual-level linear trends.

5.2 Labor Supply Results

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

	1(Working)			Hours Worked			Hours Worked (Working)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Uni x Post	-0.0018	-0.0018	0.0003	0.1143	0.1127	0.2005	0.5105**	0.5164**	0.6070**
	(0.0038)	(0.0038)	(0.0046)	(0.1499)	(0.1499)	(0.1766)	(0.2299)	(0.2299)	(0.3042)
N	211,069	211,069	211,069	210,891	210,891	210,891	80,785	80,785	80,785
Adj R-sq	0.553	0.553	0.572	0.619	0.619	0.643	0.580	0.581	0.630
Mean Dep	0.410	0.410	0.410	15.194	15.194	15.194	36.201	36.201	36.201
State and Time FE	X	X	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X	X	X
Age 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, 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.

Main Results. Table 2 presents the labor supply results from Equation 1 for married women. The sample includes women between 22 and 65 years old. In Columns (1)-(3), the dependent variable is an indicator for employment. In Columns (4)-(6), the dependent variable is weekly hours worked. Across both employment measures, married women are no more likely to be working in the post-reform periods, and they do not adjust their hours worked.

The failure of women to adjust their employment levels differs from previous findings in high-income countries. Bargain et al. (2012) shows that women increase their labor force participation in response to the legalization of divorce in Ireland, and this increase occurs mostly at the extensive margin. Stevenson (2008) also finds an increase in labor force participation in the United States' as a result of the introduction of unilateral divorce. The lack of effect following the reform in Mexico demonstrates that the existing findings may be context-dependent. There are two immediate explanations for the failure of married women to change their employment. First, a portion of the lack of response may be explained by employment entry barriers. Women may lack the opportunity to enter labor markets, especially women who were not working before the reform. Second, as discussed in the background section, women may suffer social stigma surrounding employment. Mexico has a lower share of employed women, less than 50% compared with more than 60% in the United States, and thus the context is quite distinct.

We explore whether these explanations are plausible in the final columns of Table 2. We show the hours worked for women who were already working. Over Columns (7)-(9), women who work appear to increase their hours worked by one-half of an hour. This result is robust to the included time trends in Column (9). The increase in hours worked for employed women following the reform, suggests that some women may not have access to employment or labor market opportunities.

Overall, the initial results reveal that women may slightly increase their hours worked (where possible), but are not moving into the labor market. To test whether our results are robust to alternative measures of time use and labor supply, we examine several different outcomes in Table 3. First, in Column (1) and (2) we attempt to parse out whether women are entering the labor force in either formal or informal work. Women in Mexico may disproportionately choose to work in informal work if labor market opportunities for women are scarce, and the distinction is especially relevant for the middle-income setting where women may not have access to the formal labor

Table 3: Alternative Measures of Married Women's Labor Supply and Time Use

	1(Informal Work)	1(Formal Work)	Informal Hours	Formal Hours	Log(Hours Worked+1)	Log(Hours Worked)	Time Kids	Time House	Time Leisure	Hours Kids (Working)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Uni x Post	-0.0020	0.0013	1.1810**	-0.0092	0.0089	0.0168*	-0.1964	0.1660*	-0.3321	-0.1646
	(0.0043)	(0.0015)	(0.5058)	(0.2235)	(0.0152)	(0.0085)	(0.1779)	(0.0942)	(0.3502)	(0.1938)
N	211,069	211,069	40,142	36,777	210,891	83,663	211,069	211,069	211,069	80,961
Adj R-sq	0.475	0.710	0.601	0.627	0.627	0.676	0.440	0.759	0.624	0.457
Mean Dep	0.243	0.188	31.568	41.680	1.462	3.411	9.019	7.360	121.032	7.038
State and Time FE	X	X	X	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X	X	X	X
Age FE	X	X	X	X	X	X	X	X	X	X
Individual Trend	X	X	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, 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.

market. Across the binary employment decision, there is no difference after the reform. Then we show the hours worked in each of these two groups, informal and formal work. Women increase their hours worked in the informal group but not in the formal group. The majority of the impact of the unilateral reform appears to be for women who had access to informal markets already.

Next, we show the results over the log of hours worked instead of linear hours worked. Column (5) shows the log of hours worked plus one (to include zeros), and Column (6) shows the log of hours worked (excluding zeros). The log of hours worked shows an increase, but not log of hours worked plus one (including zeros). This finding corroborates the baseline results. We conclude by testing women's time use along other dimensions in Columns (8)-(10). These findings suggest that women are not reorganizing their time allocation outside of the small increase in time spent on household work (but not childcare).

Subsamples of Men and Women. We conclude the main labor supply analysis by testing the effect of the reform over different subsamples of women in Table A3. The results are presented with individual fixed effects and time trends. Column (1) shows all women. Column (2) presents single women, and Column (3) shows cohabitating women. In Column (4), we present results for 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. Across the subsamples of women, employment patterns are similar before and after the reform. For completeness, we end by showing the reform effect on men in Appendix Table A4. When individual time trends are included, married men fail to change their labor supply or time use.

5.3 Robustness

For robustness checks, we test several alternative specifications. First, we use an event-study design and consider the effect over time for the short panel available. Then, we show the effect separated by the default state property regime. Next, we conduct a balance test and then test several different subsamples of married women. Finally, in the Appendix, we also show the effect in an alternative dataset, the ENIGH (used for the structural model). This dataset considers a longer-time horizon and our specification in the ENIGH does not use individual fixed effects. Across all robustness checks, we fail to find an apparent effect for hours worked or employment. The only noticeable effect is hours worked for employed women.

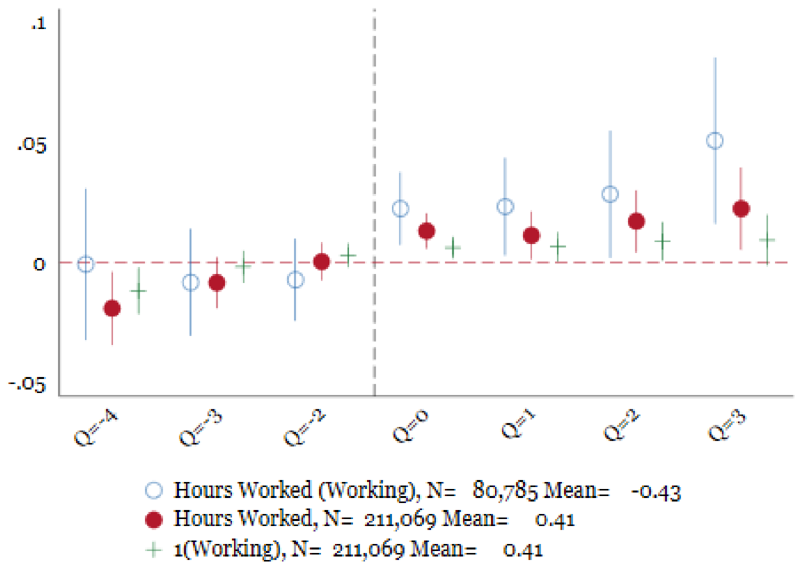
Event Study Analysis. We begin our robustness checks by testing whether women changed their labor supply in the periods leading up to the reform. In other words, we examine whether there are pre-trends. To accomplish this, we estimate a flexible event-study specification, for married woman i in state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

$$Y_{ist} = \alpha + \sum_{Q=-4}^{3} \beta_Q \text{ Unilateral}_{sQ} \mathbf{X}'_{it} + \pi_s + \tau_T + \alpha_i + \epsilon_{ist} \tag{2}$$

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 went into effect. We use the period just before the reform as we expect the reform to have an immediate effect on labor supply of women (Hoehn-Velasco and Penglase, Forthcoming). Thus, the pre-treatment period Q=-1 provides a better baseline measure of women's labor supply than the period the reform went into effect, $Q = 0$.

Figure III shows 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 period just before the reform (period -1), and the plotted coefficients reflect the labor supply effect relative to this 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.

Figure III: Labor Supply Response – Married Women



NOTES: Event study coefficients are reported for the reform quarter as well as four quarters post reform. Lines represent 95 percent confidence intervals. The excluded quarter is the quarter just before the reform (period -1). 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,and age indicators.
SOURCE: Individual-level data from the National Occupation and Employment Survey (ENOE) for the years 2007 Q1 through 2019 Q2.

Figure III reports the probability of working (in green), the hours worked (in red), and hours worked for employed women (in blue). To plot estimates on the same graph, we standardize hours to 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 be linearly increasing before the reform. There is a smaller increase in employment, but the response is also on a pre-trend. Both hours worked, and employment appears 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 more clearly shift upwards after the reform. The estimates for hours worked of employed women do not appear to be on a pre-trend and instead immediately increase after the passage of unilateral divorce. The event-study results suggest similar conclusions to the baseline results, employed women may be weakly increasing the labor supply through hours worked, but these women do not change their binary employment choices.

Results by Property Regime. The impact of no-fault unilateral divorce laws on women's bargaining power may depend on the divorce property regime. From the perspective of Nash bargaining, spouses who have a better outside option have a higher threat point, and therefore a better bargaining position within the marriage. The introduction of no-fault unilateral divorce interacts with the divorce property regime in such a way that significantly alters each spouses outside option.[17]

One of the drawbacks of this analysis compared to related work in the United States (Stevenson, 2008; Voena, 2015) is that we are unable to observe the property distribution upon divorce. Voena (2015) finds a decline in women's employment in states that had community property division and no corresponding change in states with title-based or equitable distribution of property. The results in Voena (2015) suggests that the treatment effect of the reform may vary by the property regime. However, Mexican property regimes operate quite differently than in the United States. Unlike the United States, where the property regime is decided at the state level, the prop-

[17]The importance of property rights in understanding the consequences of divorce laws has been studied most notably by Gray (1998) and Voena (2015). Both studies highlight the need to account how marital property is divided should the couple divorce in understanding the impact of no-fault divorce on labor supply, savings, and bargaining power. Stevenson (2008), however, finds evidence that the effect of unilateral divorce on women's labor supply does *not* depend on the property regime.

erty division in Mexico is decided at the marriage level. In Mexico, property division upon divorce is chosen within the initial marriage contract and is therefore endogenous. The couple can decide to operate under a *community* property regime, in which case all assets are shared by both spouses (56.9 percent of marriages). Alternatively, the couple could decide on a *separate* property regime, where each spouse individually owns certain assets (26.5 percent of marriages).[18]

We test whether the labor supply results are sensitive to the property division within each state. To establish the importance of property allocation, 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 appears:

$$\begin{aligned} Y_{ist} = \alpha + \beta_1(\text{Uni}_s \times \text{Post}_t) + \beta_2(\text{Uni}_s \times \text{Post}_t \times \text{Community Regime}_s) \\ +\beta_3(\text{Uni}_s \times \text{Post}_t \times \text{No Regime}_s) + \mathbf{X}'_{it} + \pi_s + \tau_T + \alpha_i + \phi_i t + \epsilon_{ist} \end{aligned} \tag{3}$$

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 is the separate property group.

Table 4 displays the property regime results. We show the reform indicator, the reform interacted with states that had no defined property regime, and the reform interacted with the community property regime. The results suggest that employment decreases in states with community property and no default property, but increases in states with separate property regimes. These findings partially align with findings in the United States. While Stevenson (2008) finds an increase in labor supply no matter the regime, Voena (2015) sees a decline in employment in states with community property. Here we see a similar effect to Voena (2015), and this finding suggests that if we could observe the actual property regime at the marriage level, our baseline findings

[18]The remaining marriages are unaware of what they decided.

might be more apparent. The heterogeneity in the property regimes produces opposite estimates, and the effect cancels out in aggregate.

Though the results do partially align with previous work, caution in making comparisons is warranted. In Mexico, the property regime is not truly observable at the state level. Our findings suggest that there may be competing effects of the property regime, where the effect cancels itself out across individual marriages, resulting in no difference *on average* in the estimation sample. Because we do not observe property rights within the union, we are unable to account for this type of heterogeneity directly. Alternatively, enforcement of the marriage contracts may be limited relative to implementation in the United States.

Table 4: Married Women's Labor Supply by Default Property Regime

	Working			Work Hours		
	(1)	(2)	(3)	(4)	(5)	(6)
Uni x Post=1	0.0087	0.0086	0.0154*	0.4462**	0.4377**	0.5176
	(0.0056)	(0.0056)	(0.0089)	(0.2178)	(0.2179)	(0.3387)
No Default × Uni x Post=1	-0.0146**	-0.0147**	-0.0293**	-0.4955*	-0.4985*	-0.7105
	(0.0073)	(0.0073)	(0.0137)	(0.2958)	(0.2957)	(0.5390)
Community Property × Uni x Post=1	-0.0152**	-0.0149**	-0.0202*	-0.4709**	-0.4577*	-0.4012
	(0.0061)	(0.0061)	(0.0110)	(0.2363)	(0.2364)	(0.4247)
N	211,069	211,069	211,069	210,891	210,891	210,891
Adj R-sq	0.553	0.553	0.572	0.619	0.619	0.643
Mean Dep	0.410	0.410	0.410	15.194	15.194	15.194
State and Time FE	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X
Age FE		X	X		X	X
Individual Trend			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, 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.

Remaining Robustness Checks. We examine how similar treatment and control states were prior to the adoption of unilateral divorce laws in Figure A1. To accomplish this, we conduct a balance test that is standard in the literature (e.g., Goodman-Bacon (2018)). The results suggest that there are some differences in states treated earlier in the sample. However, given that we conduct an individual fixed effects model, this is less concerning. This balance test, however, gives us caution in interpreting the results over the early-treated and later-treated, as treatment may vary over the groups.

Based on the balance test results, we turn to several robustness checks in Table A5. Throughout these checks, we focus on our primary outcomes of interest: quarterly hours worked and employment. With the balance test in mind, we first present the results for the early versus the later treated. The motivation behind this restriction is that early-adopting states appear to differ in observable ways from later-adopting states. Column (1) shows the results restricting the sample to states that passed the reform before 2015 and Column (2) shows the post-2015 group. In Columns (1)-(2), the estimates for both early and later-treated groups both fail to be significant.

In Column (3), 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.[19] 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 (3), the results again fail to show an effect.

In Columns (4)-(8), we examine additional subsamples of interest including, rural, urban, those with children under 5, the balanced panel of individuals, and those out of school in the first quarter of the sample. In Column (9) we test the legal *de jure* legislation dates instead of the *de facto* dates and Column (10), we test a placebo test, where the reform is now coded as a year in advance. All robustness tests fail to show an impact.

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

6 Unilateral Divorce and Bargaining Power: A Structural Analysis

The above results suggest that the introduction of no-fault unilateral divorce laws resulted in lno change in married women's labor supply along the extensive margin, and a small increase their weekly hours worked. In this section, we investigate this relationship in more detail; we analyze the causal relationship between the introduction of unilateral divorce laws on women's bargaining power within the household. As bargaining power is not observable, we set out a structural model of intrahousehold decision making to recover this parameter. We follow Dunbar et al. (2013) to identify the share of household resources controlled by women, which we will use to infer bargaining power. Section 6.1 presents a standard collective household model along with several model implications. The estimation and results are provided in Sections 6.2 and 6.3, respectively.

6.1 Model

The model 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) (DLP). The main alteration is that we emphasize the role of unilateral divorce in the household decision making process.

We model *nuclear* households, defined as households that consist of a married man (m) and woman (f) with up to three children (c). The adults are decision makers within the household and they bargain over how to allocate the household budget. Households are assumed to reach a Pareto efficient allocation of goods.[20] Bargain-

[20]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);

ing 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 of the model is to uncover how bargaining power changes as a result of these laws. To accomplish this, we identify how consumption goods are allocated within the household to determine which spouse "controls" more of the budget. This measure will serve as a proxy for bargaining power.[21]

Consistent with the Browning et al. (2013) formulation of the collective model, the household purchases a k-vector of goods z at market prices p. Individuals consume a k-vector of *private good equivalents* x of the household-level quantities, which are given by $z = Ax$. The k-by-k matrix A accounts for the sharing of goods within the household, and transforms what the household purchases into what individuals actually consume using Barten scales (Barten, 1964). The model, therefore, accounts for the allocation of public goods.[22]

The man and woman each have their own utility function $U_j(x_j)$, $j \in \{m, f\}$. We write the household's problem as follows:

$$\begin{aligned}
&\max_{x_m, x_f} \tilde{U}[\ U_m(x_m), U_f(x_f),\ p/y\ ] = \mu_m(p/y, Uni)U_m + \mu_f(p/y, Uni)U_f \\
&\qquad \text{such that} \\
&\qquad y = z'p \ \text{ and } \ z = A[x_m + x_f]
\end{aligned} \tag{4}$$

where $\tilde{U}$ exists by Pareto efficiency and μ_j are the Pareto weights. We denote the state divorce regime with the variable Uni, which enters each spouses 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

Brown et al. (2018). However, there are notable exceptions where Pareto efficiency is rejected. See, for example, Udry (1996).

[21] Browning et al. (2013) show that resource shares have a monotonic relationship with the Pareto weights, which determines each household member's bargaining power.

[22] We will not identify the extent of consumption sharing, but our estimates of consumption allocation will account for the existence of it.

goods at within household shadow prices $A'p$ allows to calculate resource shares η_m, defined as the share of the total household budget controlled by the man. 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.[23] Resource shares are a function of many household characteristics; we are primarily interested in the causal relationship between unilateral divorce laws and resource shares. A priori, it is not obvious how divorce laws will impact bargaining power, as it depends on which spouses outside option improves as a consequence of the law. As a result, we let the data inform what impact more liberalized divorce laws in Mexico had on women's resource shares, and therefore bargaining power.

Private Assignable Goods: To identify resource shares, we rely on private assignable goods. A good is private if it is not shared. Examples of private assignable goods include food and clothing. A good is assignable if the econometrician can determine who in the household consumed the good. In our context, we can not determine food consumption for each individual household member, but we can assign clothing to men and women. Food is therefore not assignable, whereas clothing is. Following DLP, we derive household-level demand functions for the private assignable goods. 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.

DLP derive the following household-level budget share functions for the private assignable good k . Identification does not require price variation, so we use an Engel

[23]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 $A'p$.

curve framework (budget share functions holding prices fixed):

$$\begin{aligned} W_m(y, Uni) =& \eta_m(y, Uni)\ \omega_m(\eta_m(y, Uni)y) \\ W_f(y, Uni) =& (1 - \eta_m(y, Uni))\ \omega_f((1 - \eta_m(y, Uni))y) \end{aligned} \tag{5}$$

where W_j is the budget share for good for men's or women's clothing, and ω is the individual-level demand function. The parameter of interest is η_j which serves as our measure of bargaining power. The challenge in identifying η_j is that for each Engel curve there are two unknowns functions: ω_j and η_j. That is, there are two reasons the household can spend more on women's clothing; (1) women may like clothing, or (2) women may control a large share of the household budget. In what follows, we discuss how we can separately identify clothing preferences (1), from resources shares (2).

Identification We follow the DLP methodology of identifying resource shares using Engel curves for private assignable goods. We use assignable clothing for men and women. DLP impose two key identification assumptions. First, resource shares are assumed to be independent of household expenditure.[24] Second, DLP restrict preferences to be similar in a limited way across people. We discuss the validity of these assumption as we move through identification.

We assume individual preferences follow a PIGLOG indirect utility function which takes the following form: $V(p, y) = e^{b(p)}[\ln y - a(p)]$. By Roy's identify, we derive budget share equations that are linear in log expenditure.[25] Holding prices fixed, this results in the following Engel curves: $w(y)_j = \alpha_j + \beta_j \ln y$. Substituting this equation into Equation (5) results in a system of household-level Engel curves. We suppress

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

[25]More general functional forms are allowed, but Engel curves of this form seem to match the data.

observable heterogeneity for now for notational clarity:

$$\begin{aligned} W_m &= \eta_m \left[\alpha_m + \beta_m \ln(\eta_m) \ + \beta \ln y\right] \\ W_f &= (1-\eta_m)[\alpha_f + \beta_f \ln(1-\eta_m) + \beta \ln y] \end{aligned} \tag{6}$$

where α_j and β_j are clothing preference parameters.

As required by DLP, we impose the "Similar Across People" (SAP) restriction. This restricts the slope preference parameter for clothing β_j to not vary across people, that is, $\beta_m = \beta_f = \beta$. Intuitively, this assumption requires that for the man and woman, their marginal propensity to consume clothing is the same.[26]

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 $c_j = \eta_j\beta$. Then since resource shares sum to one, we have that $\sum_j c_j = \sum \eta_j\beta = \beta \sum_j \eta_j = \beta$. Solving for resource shares, we have $\eta_j = c_j/\beta$.[27] How are we inferring bargaining power from how clothing expenditures are allocated? If household expenditure increases, this change will affect household-level expenditure on both men's and women's clothing. If we see that men's clothing budget shares increased by more than the increase in women's clothing budget shares, we infer from that that the man in the household *controlled* more of that additional household expenditure. Placing this intuition within a structural model is what identifies resource shares, and ultimately bargaining power.[28]

[26] Bargain et al. (2018) have tested several aspects of the collective model including this assumption. Their results more generally provide empirical support for using clothing expenditures to infer how total resources are allocated.

[27] 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.

[28] It is important to note that the relative magnitude of clothing budget shares does not determine the relative magnitude of resource shares. It is entirely possible for women to consume more clotting, but still control a smaller share of the budget.

6.2 Estimation

As discussed earlier, 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. 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 A7. In estimating the structural model, 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 nuclear households, where a nuclear consists of a married couple with zero to three children. We therefore exclude a significant percentage of 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 in each wave to eliminate outliers, as well as households with men or women under age 22 or over 65.[29] Lastly, we exclude households with missing values for any of our covariates.

[29] One reason to focus on households in this age range is because our model is static. The behavior of couples may change at retirement, and for simplicity, we avoid developing a dynamic model that accounts for these changes. Moreover, individuals age 18-21 may still be in school.

We first add an error term to the Engel curves given in Equation (9).

$$\begin{aligned} W_m &= \eta_m\,[\alpha_m + \beta_m \ln(\eta_m)\ + \beta \ln y] + \epsilon_m \\ W_f &=(1-\eta_m)[\alpha_f + \beta_f \ln(1-\eta_m) + \beta \ln y] + \epsilon_f \end{aligned} \tag{7}$$

We first estimate the model using non-linear Seemingly Unrelated Regression. This estimation method is, in effect, maximum likelihood with multivariate normal errors. Since expenditure is potentially endogenous due to measurement error or unobservable preference heterogeneity (see, for example, Lewbel (1996)) we use income as an exogenous instrument and estimate the model via Hansen (1982)'s Generalized Method of Moments. Let q_j be an $L \times 1$ vector of instruments. Then $E(\epsilon_j q_j) = 0$ for all j, l. The moments can be written as follows:

$$\begin{aligned} &E[(W_m - \ \eta_m\,[\alpha_m + \beta_m \ln(\eta_m)\ + \beta \ln y])\ q_{ml}] = 0 \\ &E[(W_f - (1-\eta_m)[\alpha_f + \beta_f \ln(1-\eta_m)\ +\ \beta \ln y])\ q_{wl}] = 0 \end{aligned} \tag{8}$$

For instruments, we interact our vector of household demographic characteristics X_j, log income, and log expenditure with X_j.

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. Moreover, we allow resource shares to depend on the divorce law regime in the household's state of residence. For $j \epsilon \{m, f\}$:

$$\begin{aligned} \eta_j &=\delta^{\eta_j} X_i + \gamma_t^{\eta_j} + \psi_s^{\eta_j} + \phi Uni_s \times Post_t \\ \alpha_j &=\delta^{\alpha_j} X_i + \gamma_t^{\alpha_j} + \psi_s^{\alpha_j} \\ \beta &=\delta^{\beta} X_i + \gamma_t^{\beta} \end{aligned} \tag{9}$$

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 assume the divorce regime only affects the household demand for assignable clothing through its affect on resource shares, that is, Uni is a distribution factor and therefore does not enter either clothing preference parameter α_j or β. Divorce laws have previously been used in the literature as a distribution factor (Chiappori et al., 2002). Lastly, note that β does not vary across person types j as required by the DLP identification method.

With the panel structure of the data, we are then estimating a two-way fixed effects specification within the structural model of intra-household resource allocation. The spirit of this identification strategy is to combine the best features of reduced-form and structural techniques, as discussed in Lewbel (2018). In effect, we estimate a structural system of Engel curves to identify resource shares which are linear in household characteristics. Then within the resource share function, we use a two-way fixed effects model with the structural parameter as the outcome of interest.

6.3 Results

Main Results Table 5 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 (9) which is estimated within System (8). Clothing preferences parameters are estimated, but not necessarily of interest, so we omit them from our discussion.

For the main results, we limit the sample to married couples with zero to three children. These results are presented in Columns (1) and (2). In Column (1) we exclude state fixed effects from the resource share function (and instead include region fixed effects), while in Column (2) we include them. In both specifications, we find an insignificant decline women's bargaining power: The preferred results, given in

Table 5: Effect of Unilateral Divorce on Women's Bargaining Power

	MAIN RESULTS		ALTERNATIVE SAMPLES	
	All Couples		With Children	Without Children
	(1)	(2)	(3)	(4)
Uni × Post	-0.018	-0.017	-0.003	-0.042*
	(0.012)	(0.011)	(0.011)	(0.022)
N	46,090	46,090	36,562	9,528
Region FE	X			X
State FE		X	X	
Year FE	X	X	X	X

NOTES: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. The main results in Columns (1) and (2) include all households with 1 married couple, and 0 to 3 children. In Columns (3) and (4) we estimate the model separately for couples with and without children. 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. SOURCE: National Household Income and Expenditures Survey (ENIGH) for the years 2008, 2010, 2012, 2014, 2016, and 2018.

Column (2), suggest that unilateral divorce resulted in no change in women's control of the household budget. Since couples with, and without children may differ in unobservable ways (that we cannot account for), we estimate the model separately for those two samples. The results are similar in magnitude and significance for couples with children. Among couples without children, we see a 4.2 percentage point decline in women's resource shares, but this is only significant at the 10 percent level. These results are presented in Columns (3) and (4).

Next, Table A9 presents results using GMM estimation. These results, which are robust to measurement error in household expenditure, show a small negative effect; the introduction of unilateral divorces laws resulted in a 1.2 percentage point decline in married women's resource shares. Using this specification, we also see a 1.5 percentage point decline among married women.

We attribute this small effect to three reasons. First, our estimation may not be able to disentangle the relationship between unilateral divorce, labor supply and bargaining power. Unilateral divorce may cause women's bargaining power to decline, but if this in turn causes women to work more, that may offset the decline in bargaining power. Second, the marital property regime (community or separate) varies across marriages and his likely highly correlated with which spouse benefits from the reforms. Finally, we see a sharp increase in divorce rates. This suggests that couples may not be renegotiating the household allocation, but rather simply divorcing.

To examine the above explanations, we begin by incorporating labor supply into our resource share function. That is, we allow women's bargaining power to vary with the wife's employment status.[30] In Table A8, we report the covariates of the resource share function. Columns (1) to (3) display our baseline results that omit labor supply as a covariate. In Columns (4) to (6), we include an indicator for women's labor supply. This coefficient is only significant among married women with children. Among these women, working is associated with higher bargaining power. Thus, it is possible that unilateral divorce laws decreased women's bargaining power, causing them to work more. However, working more, in and of itself, *increased* women's bargaining power, offsetting some of this decline. We analyze this in a different way in Table 6. Here, we examine how the effect of unilateral divorce laws on bargaining power varies with labor supply. These results are presented in Column (1), but do not suggest much difference.

A second explanation is that we are missing heterogeneity due to the marital property regime. Thus, we examine whether the results vary by the default divorce property regime in the state (as discussed in Section 5.2).[31] Women's outside options are presumably better in community property states relative to separate property states. Therefore, we may expect unilateral divorce to decrease women's bargaining power

[30] In the baseline model, labor supply is omitted from the resource share function as it is a "bad control"; from our analysis of labor supply we know unilateral divorce alters women's employment.

[31] Recall that each state has a default property regime (community or separate), but the couple has the option of choosing the property regime if they desire.

more in separate property regimes relative to community property regimes. Our results are partially consistent with this story and the results are presented in Columns (2). There appears to be a (marginally significant) 2.5 percentage point decline in bargaining power among women living in separate property states. However, the results in community property states are not statistically different.

Lastly, we examine how heterogeneity in divorce stability may affect our results. We do this by first predicting divorce probabilities using our panel data (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. We may expect that couples with a low divorce probability are unaffected by more liberal divorce laws, while couples with a high divorce probability do respond. These results are presented in Column (3) of Table 6. We see no difference. This null result could be due to selection into divorce, which we are unable to capture without panel data.[32]

Finally, we present summary statistics of the predicted resource shares in Table 7 (we do so separately for couples with and without children). Bargaining power is largely equal across household compositions with women controlling a slightly smaller share of the household budget at 52.5 percent in married couples without children. Recall that these measures have a one-to-one relationship with the Pareto weights. Results are similar for couples with children. These figures 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.

[32]Changes in marriage rates could also be complicating this analysis. However, Hoehn-Velasco and Penglase (Forthcoming) show that the introduction of unilateral divorce laws did not affect divorce rates in Mexico. This differs from past work in other contexts (e.g., Rasul (2004)).

Table 6: Alternative Specifications

	Labor	Default Property Regime	Marital Stability
	(1)	(2)	(3)
Uni × Post	-0.021*	-0.025*	-0.019
	(0.012)	(0.014)	(0.013)
Uni × Post × Wife Employed	0.005		
	(0.011)		
Wife Employed	0.022		
	(0.015)		
Uni × Post ×Community		0.005	
		(0.012)	
Uni × Post × Other		0.025*	
		(0.014)	
Uni × Post × Stability			-0.097
			(1.049)
Stability			3.313***
			(0.960)
N	46,090	46,090	46,090
State FE	X	X	X
Year FE	X	X	X

NOTES: * p<0.1, ** p<0.05, *** p<0.01. The sample includes households with one married man and woman, both age 22 to 65. The results in Column (1) show differences in responses across woman who work and those who do not. In Column (2), we examine how the effect of unilateral divorce laws vary across states with different default property regimes. In Column (3), we examine heterogeneity by predicted marital stability. Marital stability is constructed using a regression to predict divorce using the ENOE panel data. Using the ENOE, we evaluate which demographic characteristics predict subsequent divorce including, husband and wife ages, education, employment, work hours, number of children, and relative wages. 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. SOURCE: National Household Income and Expenditures Survey (ENIGH) for the years 2008, 2010, 2012, 2014, 2016, and 2018.

Table 7: Predicted Resource Shares

		Mean	Median	Std. Dev.	Min.	Max.	Obs.
Married Couples with No Children	Women	0.525	0.526	0.074	0.201	0.789	9,528
	Men	0.475	0.473	0.074	0.211	0.798	
Married Couples with One to Three Children	Women	0.532	0.534	0.076	0.192	0.885	36,562
	Men	0.467	0.466	0.076	0.114	0.807	

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.

7 Conclusion

This paper analyzes the consequences of unilateral divorce laws in Mexico. We demonstrate that women living in states that legalized unilateral divorce did not increase their employment levels. However, women who were already working may have increased their hours worked following the reform. This result differs from the existing literature (Stevenson, 2008; Bargain et al., 2012; Voena, 2015) where women responded to the divorce reform by changing their employment levels. We find evidence that this difference may be due to lower labor market opportunities for women, or due to cultural norms against women working. We support these results using a structural model of intra-household resource allocation to identify changes in bargaining power as a result of unilateral divorce laws. Our findings indicate a small, near-negligible decline in women's bargaining power (on average).

Our results highlight the importance of understanding intra-household dynamics when studying the consequences of public policy. Unilateral divorce laws are present in at least nine countries and appear destined to spread throughout the world. Over the past 30 years in Latin American alone, Brazil, Argentina, Colombia, and Chile have all made efforts to ease and expedite their divorce processes. 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.

There are several limitations to our analysis that motivate future research. First, the relationship between labor supply decisions and bargaining power is difficult to disentangle within this context and may need to be jointly modeled. One complication that arises in the interpretation of our results is that working, in and of itself, increases female bargaining power (Anderson and Eswaran (2009); Atkin (2009)). That is, women may work more because of a decline in bargaining power, but the higher wage income relative to their husbands may offset some of this decline. A natural solution to this problem is to model labor supply in addition to consumption allocations as a way of inferring women's bargaining power. We would study both how consumption goods and leisure are allocated to estimate changes in bargaining power as a result of the divorce laws. Lise and Seitz (2011) conducts an analysis along these lines. However, there are several obstacles that prevent us from taking this route. First, Lise and Seitz (2011) focuses only on childless married couples who both are employed in market work and, this population is not common in Mexico as women often work, but not in the formal sector with observable wages. Moreover, modeling household production would involve strong assumptions about household production functions. Incorporating household production into a model of intrahousehold consumer demand is beyond the scope of this paper. While not entirely satisfying, we refrain from complicating our model and instead choose a simpler formulation that may fail to capture our desired outcomes of interest entirely.

Second, our model is static, and there are undoubtedly dynamic elements to the decision to get divorced, most obviously the possibility of remarriage. Third, there are several data limitations that are unique to the structural results using the ENIGH and limit our interpretation of the results. The ENIGH does not provide marital length, which limits our ability to subset the results into different groups such as newly married couples or established couples. We also do not have a prolonged time span following the reform, which limits our ability to examine whether individuals change their fertility decisions after the reform. Future work could consider the differential effects by marital length as well as whether divorce laws affect women's childbearing choices.

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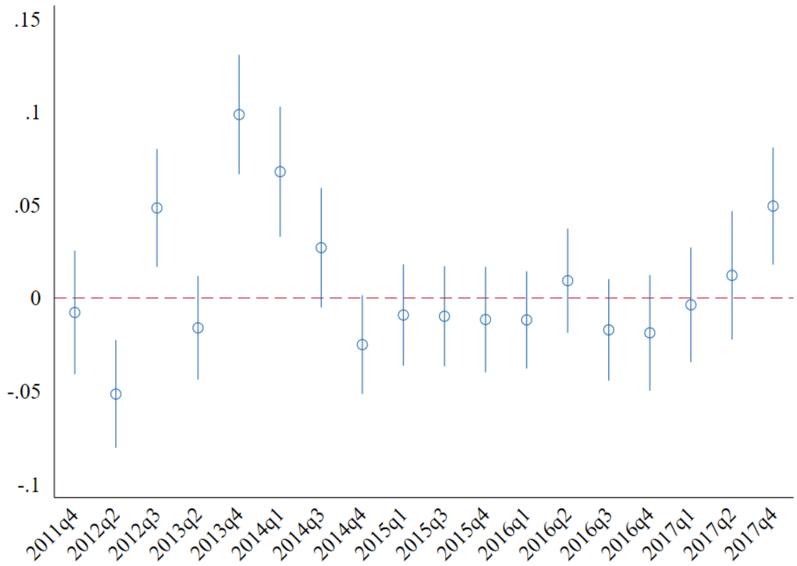
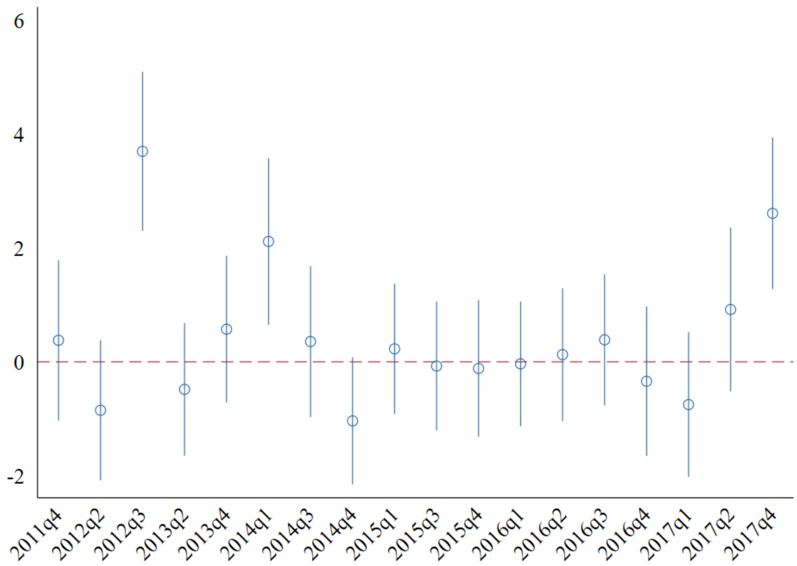
A Additional Figures

Figure A1: Balance Test – Married Women
Panel A: 1(Working)

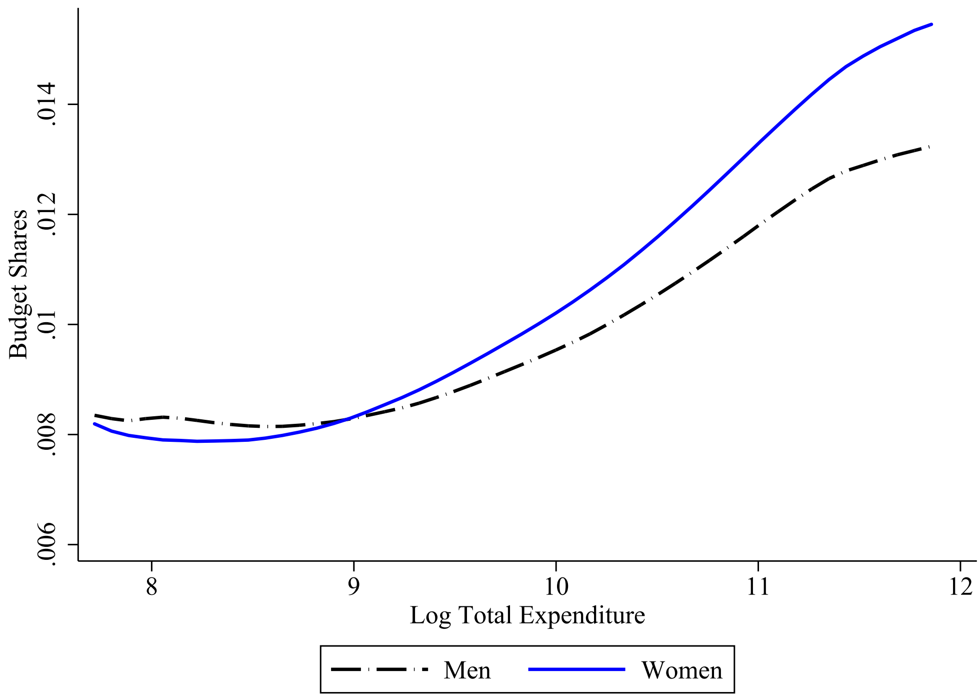


Panel B: Work Hours



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

Figure A2: Non-Parametric Clothing Engel Curves



SOURCE: ENIGH 2008-2018.

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: Probability of Leaving the Panel Early

	1(Leave Sample)			1(Leave Divorced)		
	(1)	(2)	(3)	(4)	(5)	(6)
Uni x Post	0.0001	-0.0000	0.0000	-0.0001	-0.0001	-0.0001
	(0.0015)	(0.0014)	(0.0014)	(0.0006)	(0.0006)	(0.0006)
Education		0.0000	0.0000		-0.0001***	-0.0001***
		(0.0001)	(0.0001)		(0.0000)	(0.0000)
Age		-0.0119***			-0.0007***	
		(0.0003)			(0.0001)	
Age Squared		0.0001***			0.0000***	
		(0.0000)			(0.0000)	
Number Children		-0.0128***	-0.0127***		-0.0005***	-0.0005***
		(0.0003)	(0.0004)		(0.0001)	(0.0001)
1(Child <5)		-0.0436***	-0.0425***		-0.0032***	-0.0031***
		(0.0011)	(0.0011)		(0.0005)	(0.0005)
N	581,516	581,516	581,516	581,516	581,516	581,516
Adj R-sq	0.001	0.038	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		X	X
a			X			X

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

Table A3: 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	0.0004	0.0065	-0.0035	-0.0006	-0.0080	-0.0015	-0.0057
	(0.0037)	(0.0098)	(0.0072)	(0.0049)	(0.0116)	(0.0045)	(0.0186)
N	407,390	84,355	61,088	244,647	33,578	138,455	7,149
Adj R-sq	0.590	0.561	0.548	0.584	0.553	0.585	0.584
Mean Dep	0.489	0.654	0.431	0.446	0.672	0.431	0.560
State and Time FE	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X
Age 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	0.1540	0.4933	0.0082	0.1111	-0.5670	0.0407	-0.5715
	(0.1266)	(0.4775)	(0.3282)	(0.1589)	(0.5260)	(0.1349)	(0.9329)
N	407,007	84,276	61,035	244,421	33,530	138,354	7,144
Adj R-sq	0.653	0.606	0.618	0.652	0.618	0.651	0.654
Mean Dep	19.007	27.161	16.621	16.813	26.972	15.982	22.189
State and Time FE	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X
Age 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, 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 A4: Unilateral Divorce Reform and Married Men

	1(Working)	Hours Worked	Time Kids	Time House	Time Leisure
	(1)	(2)	(3)	(4)	(5)
Uni x Post	-0.0016	0.0081	-0.0128	0.1481	-0.1912
	(0.0052)	(0.3119)	(0.0851)	(0.1191)	(0.6114)
N	196,252	195,387	196,252	196,252	196,252
Adj R-sq	0.403	0.457	0.267	0.276	0.433
Mean Dep	0.870	41.140	2.248	4.491	81.496
State and Time FE	X	X	X	X	X
Individual FE	X	X	X	X	X
Age 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, 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 A5: Robustness Checks on Married Women's Labor Supply

PANEL A: 1(WORKING)

	Early Adopters	Late Adopters	No Mexico City	Urban	Rural	Child 0-5	Five Quarters	Out of School	De Jure	Placebo
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Uni x Post	-0.0034	0.0003	0.0004	0.0025	-0.0034	-0.0005	-0.0003	0.0014	-0.0076	-0.0064*
	(0.0115)	(0.0064)	(0.0049)	(0.0035)	(0.0069)	(0.0100)	(0.0048)	(0.0055)	(0.0056)	(0.0035)
N	80,989	130,080	205,923	125,902	85,167	43,979	204,539	199,655	160,058	213,096
Adj R-sq	0.582	0.564	0.571	0.597	0.552	0.579	0.572	0.572	0.568	0.588
Mean Dep	0.406	0.412	0.410	0.441	0.364	0.382	0.409	0.408	0.414	0.406
State and Time FE	X	X	X	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X	X	X	X
Age FE	X	X	X	X	X	X	X	X	X	X
Individual Trend	X	X	X	X	X	X	X	X	X	X

PANEL B: HOURS WORKED

	Early Adopters	Late Adopters	No Mexico City	Urban	Rural	Child 0-5	Five Quarters	Out of School	De Jure	Placebo
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Uni x Post	0.2014	0.1754	0.2026	0.0783	0.1979	0.1008	0.1732	0.2420	-0.1615	-0.1053
	(0.3259)	(0.2082)	(0.1475)	(0.1380)	(0.2017)	(0.2631)	(0.1483)	(0.1686)	(0.2399)	(0.1369)
N	80,915	129,976	205,749	125,803	85,088	43,950	204,364	199,483	159,896	212,871
Adj R-sq	0.643	0.644	0.644	0.654	0.633	0.649	0.644	0.644	0.633	0.653
Mean Dep	14.904	15.375	15.203	16.557	13.181	13.790	15.140	15.112	15.277	14.891
State and Time FE	X	X	X	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X	X	X	X
Age FE	X	X	X	X	X	X	X	X	X	X
Individual Trend	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, and age indicators. Column (1) removes states not treated before 2015. Column (2) includes states treated after 2014. Column (3) excludes Mexico City. Columns (4) and (5) split the sample into rural and urban areas. Column (6) includes only individuals with children under age 5. Column (7) includes only the balanced panel. Column (8) drops those who were in school in the first quarter observed in the sample. Column (9) shows the results with *de jure* dates. Column (10) 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, 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: Robustness Checks on Single Women's Labor Supply

PANEL A: 1(WORKING)

	Early Adopters	Late Adopters	No Mexico City	Urban	Rural	Five Quarters	Out of School	De Jure	Placebo
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Uni x Post	-0.0219	0.0142	0.0066	0.0013	0.0133	0.0058	0.0013	-0.0076	0.0022
	(0.0141)	(0.0112)	(0.0098)	(0.0057)	(0.0149)	(0.0101)	(0.0099)	(0.0056)	(0.0096)
N	28,886	55,469	81,088	57,640	26,715	80,534	67,861	160,058	81,682
Adj R-sq	0.562	0.561	0.557	0.562	0.559	0.563	0.552	0.568	0.574
Mean Dep	0.654	0.654	0.655	0.668	0.625	0.653	0.694	0.414	0.663
State and Time FE	X	X	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X	X	X
Age FE	X	X	X	X	X	X	X	X	X
Individual Trend	X	X	X	X	X	X	X	X	X

PANEL B: HOURS WORKED

	Early Adopters	Late Adopters	No Mexico City	Urban	Rural	Five Quarters	Out of School	De Jure	Placebo
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Uni x Post	-0.4617	0.6668	0.4960	0.2485	0.5980	0.4791	0.3620	-0.1615	0.1405
	(0.3466)	(0.6360)	(0.4801)	(0.3629)	(0.6558)	(0.4726)	(0.5267)	(0.2399)	(0.3583)
N	28,866	55,410	81,013	57,594	26,682	80,466	67,796	159,896	81,597
Adj R-sq	0.599	0.612	0.602	0.603	0.609	0.606	0.595	0.633	0.624
Mean Dep	27.016	27.237	27.187	27.795	25.793	27.086	29.087	15.277	27.398
State and Time FE	X	X	X	X	X	X	X	X	X
Individual FE	X	X	X	X	X	X	X	X	X
Age FE	X	X	X	X	X	X	X	X	X
Individual Trend	X	X	X	X	X	X	X	X	X

NOTES: Baseline fixed effects include state fixed effects, quarter-year fixed effects, individual fixed effects, and age indicators. Column (1) removes states not treated before 2015. Column (2) includes states treated after 2014. Column (3) excludes Mexico City. Columns (4) and (5) split the sample into rural and urban areas. Column (6) includes only the balanced panel. Column (7) drops those who were in school in the first quarter observed in the sample. Column (8) shows the results with *de jure* dates. Column (9) tests a placebo test, where the reform is now coded a one year prior to the actual reform. The sample includes all single 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, 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 A7: 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 A8: Determinants of Women's Resource Shares

	Excluding Labor Supply Control			Excluding Labor Supply Control		
	All Couples	With Children	Without Children	All Couples	With Children	Without Children
	(1)	(2)	(3)	(4)	(5)	(6)
Uni × Post	-0.017	-0.003	-0.042*	-0.018	-0.004	-0.043*
	(0.011)	(0.011)	(0.022)	(0.011)	(0.011)	(0.022)
Employed Woman				0.023	0.042**	-0.004
				(0.014)	(0.017)	(0.027)
Woman's Education	0.037*	0.054**	0.108***	0.035	0.055**	0.100***
	(0.021)	(0.027)	(0.038)	(0.022)	(0.028)	(0.038)
Men's Education	0.010	0.051**	-0.108***	0.008	0.052**	-0.102***
	(0.020)	(0.025)	(0.039)	(0.020)	(0.025)	(0.039)
Woman's Age	0.659	2.439**	0.052	0.554	2.344**	-0.175
	(0.739)	(1.018)	(1.558)	(0.747)	(1.039)	(1.564)
Woman's Age^2	-1.253	-2.826**	0.225	-1.072	-2.746**	0.466
	(0.883)	(1.266)	(1.603)	(0.896)	(1.294)	(1.597)
Men's Age	-1.174	-2.610***	-1.785	-1.076	-2.304**	-1.696
	(0.745)	(1.005)	(1.679)	(0.754)	(1.004)	(1.701)
Men's Age^2	1.583*	2.961***	1.764	1.488*	2.596**	1.684
	(0.851)	(1.136)	(1.693)	(0.867)	(1.137)	(1.718)
Urban	-0.001	-0.023	-0.018	-0.002	-0.022	-0.019
	(0.017)	(0.018)	(0.033)	(0.017)	(0.018)	(0.034)
Children's Age	0.656***	0.473*		0.552**	0.389	
	(0.253)	(0.276)		(0.253)	(0.272)	
Proportion Female Children	-0.025	-0.013		-0.027	-0.021	
	(0.023)	(0.021)		(0.023)	(0.021)	
1 Child	-0.007			-0.010		
	(0.035)			(0.036)		
2 Children	0.013	0.008		0.011	0.012	
	(0.025)	(0.022)		(0.025)	(0.022)	
3 Children	0.010	-0.023		0.010	-0.020	
	(0.021)	(0.023)		(0.021)	(0.023)	
Intercept	0.665***	0.586***	0.574***	0.653***	0.563***	0.569***
	(0.071)	(0.053)	(0.054)	(0.073)	(0.054)	(0.057)
Sample Size	46,090	36,562	9,528	46,090	36,562	9,528
Region FE			X			X
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.

Table A9: Effect of Unilateral Divorce on Women's Bargaining Power: GMM Estimates

	MAIN RESULTS		ALTERNATIVE SAMPLES	
	All Couples		With Children	Without Children
	(1)	(2)	(3)	(4)
Uni $\times$ Post	-0.011*	-0.012**	-0.015**	-0.021
	(0.006)	(0.006)	(0.006)	(0.024)
N	46,090	46,090	36,562	9,528
Region FE	X			X
State FE		X	X	
Year FE	X	X	X	X

NOTES: * p<0.1, ** p<0.05, *** p<0.01. Columns (1) and(2) include households with married men and women with 0 to 3 children. Columns (3) and (4) restrict the sample to households with 1 to 3, and 0 children, respectively. 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. SOURCE: National Household Income and Expenditures Survey (ENIGH) for the years 2008, 2010, 2012, 2014, 2016 and 2018.

C ENIGH Labor Supply Results

Table A10 presents the summary statistics for the ENIGH data over the main estimation sample. We report the mean of each variable for the 2008-2018 sample years. The sample includes all married individuals who are 22 to 65. The primary outcomes and controls are shown in Table A10, and include labor supply, hours worked, and time use. Between 40 and 50 percent of the sample of women work, and these women work, on average, 17 hours per week. There is a significant increase in women working in 2012 which continues to 2018. Women clearly spend the majority of their weekly time on household chores, doing on average, 38 hours of household work per week. The average woman in our sample has one and a half children, is 43, and lives outside of urban areas.

Table A10: Summary Statistics for Women

	2008	2010	2012	2014	2016	2018	Total
Age	42.47	42.92	43.08	43.24	43.49	44.30	43.48
Income (K)	11.92	12.10	11.83	13.46	13.10	14.97	13.38
# Children	1.78	1.67	1.61	1.55	1.51	1.43	1.55
1(Literate)	0.92	0.91	0.91	0.94	0.95	0.95	0.94
1(Urban)	0.24	0.24	0.38	0.27	0.38	0.40	0.34
1(Child <5)	0.23	0.22	0.22	0.20	0.20	0.18	0.20
1(Working)	0.40	0.39	0.49	0.47	0.50	0.50	0.47
Work Hours	15.75	15.58	17.24	17.29	18.01	17.92	17.25
Household Work Hours	38.33	39.65	38.49	37.88	37.96	37.66	38.15
Leisure Hours	12.50	16.59	15.13	14.50	15.44	15.78	15.17
Reported Hours	64.58	71.58	70.58	69.53	71.31	71.23	70.16

NOTES: 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. Income is quarterly nominal income and is reported in pesos.
SOURCE: Individual-level data from the National Household Income and Expenditures Survey (ENIGH) for the years 2008, 2010, 2012, 2014, 2016, and 2018.

Table A11 Panel A presents the labor supply difference-in-differences results from Equation 1 for married women. The sample includes married women who are wives of the household head or household heads themselves, who are between 22 and 65 years old. We choose this range of ages as older or younger women are unlikely to be affected by the reform. Columns (1)-(3) show the probability of working and Columns (4)-(6) show the hours worked.

From the reported coefficients in Column (1)-(3), women are no more likely to be working in the post-reform periods. Moreover, in Columns (4)- (6), there is no change in hours worked. The failure of women to change their labor supply corroborates the results with the full sample of individuals, in Columns (1) and (5) of Table 2. The panel of individuals is more informative for the particular question of interested. We are interested in whether individuals, who were married before the reform, are changing their labor supply post reform. In the pooled cross section we are unable to observe whether this is the case. Instead, we are estimating whether married women generally are changing their labor supply.

Table A11: Unilateral Divorce Reform and Married Women's Labor Supply

PANEL A: LABOR SUPPLY

	1(Working)			Hours Worked		
	(1)	(2)	(3)	(4)	(5)	(6)
Uni x Post	-0.007	-0.008	-0.016	-0.337	-0.383	-0.641
	(0.025)	(0.025)	(0.022)	(0.803)	(0.784)	(0.779)
N	104,126	104,126	104,126	104,126	104,126	104,126
Adj. R-Sq.	0.02	0.06	0.06	0.01	0.05	0.05
Mean Dep	0.47	0.47	0.47	17.25	17.25	17.25
Controls		X	X		X	X
State x Trend			X			X

PANEL B: HOUSEHOLD WORK AND LEISURE

	Household Hours			Leisure Hours		
	(1)	(2)	(3)	(4)	(5)	(6)
Uni x Post	-0.001	-0.004	-0.261	1.876	1.888	1.733
	(2.027)	(2.071)	(2.291)	(1.227)	(1.217)	(1.374)
N	103,177	103,177	103,177	103,628	103,628	103,628
Adj. R-Sq.	0.02	0.04	0.05	0.04	0.05	0.05
Mean Dep	38.15	38.15	38.15	15.17	15.17	15.17
Controls		X	X		X	X
State x Trend			X			X

NOTES: Difference-in-difference estimation with individual fixed effects. OLS coefficients reported. Individual controls include age, age-squared, and indicators for education 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 Household Income and Expenditures Survey (ENIGH) for the years 2008, 2010, 2012, 2014, 2016, and 2018. Municipality-level variables from the INEGI divorce statistics.

For completeness, we next consider whether women spend more hours on household tasks or leisure activities in the post-reform period. These results appear in Panel B of Table A11. Columns (1)-(3) show household hours and Columns (4)-(6) show leisure hours. While the coefficients are positive, they are not significant and suggest there are few remarkable changes in married women's time allocation following the reform.