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The effects of divorce laws on labor supply: a reconsideration and new results

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Abstract

In this paper, I revisit the effects of unilateral divorce laws on female labor supply. I use a variety of models to check the robustness of the results and find that the estimated effects on female labor supply are remarkably robust. The main estimates that I use in this paper suggest that unilateral divorce laws increase female labor force participation rates by roughly 4–5 percentage points and that these effects strengthen over time. There are also strong, long-term effects on the weeks and hours of work and on participation in full-time work. In addition, this paper compares the dynamic participation responses of married mothers versus married non-mothers, high-education versus low-education women, young versus old women and white versus black women.

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1. Introduction

Female labor supply increased dramatically following World War II as measured by labor force participation (LFP) and working hours. This phenomenon is important for the women themselves, their families and society at large. But what has caused this? What are the effects? Answering these questions has been the subject of a voluminous literature.

During the 1970s, US divorce laws changed significantly. The new unilateral divorce laws allowed people to end a marriage without the consent of their spouse. In addition, many states removed fault as a consideration in property division. These law changes might affect bargaining power within the household and change people's expected value of a marriage. Therefore, they might also change women's returns to housework relative to other options.

The two above-mentioned phenomena prompted researchers to ask whether divorce laws affect female labor supply. If they do, how large and how long are the effects? As Gray (1998) discussed, according to the neoclassical model, the household head pools all family resources when determining the family's optimal behavior; therefore, this model predicts that unilateral divorce should only affect the time allocations of wives through its effect on divorce probabilities. However, cooperative bargaining models demonstrate that household behavior is sensitive to earning power within the family. A bargaining model assumes that husbands and wives cooperatively bargain over possible decisions to be made by the household. Changes to divorce laws alter wives' leverage in the bargaining process, which in turn is likely to alter married women's demand for leisure and their labor force participation behavior.

Early empirical studies such as Peters (1986) and Parkman (1992) used only single-year, cross-sectional data and showed that unilateral divorce laws are positively correlated with female labor force participation rates. However, Gray (1998) used a difference-in-difference (DID) approach to show these results are problematic, finding that "unilateral divorce has no significant impact on married women's labor-force participation unless the underlying marital-property laws in each state are considered" (p. 629). In a subsequent article, Stevenson (2008) carefully examined Gray's argument and showed that these results are not robust to alternative specifications and controls. She concluded that unilateral divorce laws increased female labor force participation, regardless of the pre-existing laws regarding property division. Genadek, Stock and Stoddard (2007) tried to distinguish the responses for married women with and without children. According to their results, new divorce laws increased the labor supply of married mothers relative to married non-mothers.

Though there are several papers investigating the effects of unilateral divorce laws on female labor supply, it is vital to check the robustness of the results by using different estimation methods and functional forms. For example, Wolfers (2006) examined the effects of the changes in divorce laws on a different, but related outcome: the rate of divorce itself. He investigated the dynamic effects with state-specific time trends. However, Lee and Solon (2011) explored the sensitivity of Wolfers' results to variation in estimation method and functional form. They found that the results are extremely fragile. Lee and Solon then concluded that the impact of unilateral divorce laws remains unclear. Moreover, they suggested that identification in difference-in-differences research becomes weaker in the presence of dynamics, casting doubt on all the estimates of the effects of unilateral divorce laws found by previous research. Because previous studies on female labor supply used the same identification strategy as that in Wolfers (2006), it is necessary to check the resulting effects on labor supply and assess whether they are also fragile, whether we can successfully measure the effects and what the real effects are.

In summary, my paper makes several contributions. First, I use a variety of models to estimate the effects of unilateral divorce laws on female labor supply. I get significant results across all these models, which implies that the effects on female labor supply are very strong. Specifically, I estimate dynamic effects and robustness of the evidence to the presence of state-specific time trends, which have not been used in previous research on labor supply. I also show that the estimates are quite robust to other estimation methods and functional forms. Second, rather than simply considering labor force participation rates, I also estimate the effects on weeks worked last year, usual hours worked per week last year and LFP for full-time jobs that have not been checked before.

2. The Robustness of Estimates of Divorce Laws on Labor Supply

Previous studies on the effects of unilateral divorce laws on labor supply included state fixed effects to control for unobserved factors varying across states but unchanging within a state over time. Year fixed effects were also included to control for evolving unobserved national factors. However, other factors may influence female labor supply differently across states over time. Therefore, we need to check the state-specific time trends that have not been used in previous research. As noted by Friedberg (1998), if we overlook these factors, the estimates will be biased if the divorce reform is endogenous. This means that there may be unobserved attributes that are correlated with the law changes across states and do not change at a national level uniformly (which can be picked up by the year fixed effects). Therefore, we need to check the results when adding state-specific time trends in the regression to test whether these factors do matter. Previous researchers have not added state-specific time trends to their models.

There is another important issue we need to consider. The impact of changes in divorce laws may not be immediate and constant, as individuals may learn about the new policy and then adjust their behavior gradually. Therefore, the state-specific trends may pick up the effects of a policy and not just preexisting trends. To solve this problem, we need to add variables that model the dynamic response of divorce explicitly. These variables should identify the entire response function allowing the estimated state-specific time trends to identify preexisting trends.[1] Stevenson (2008) examined the dynamic response of female labor force participation in Table 5 in her paper, but she did not check the results by adding state-specific time trends at the same time. Therefore, we are still not sure whether the models measure the accurate effects if the trends are different in each state.[2] To ensure this, I use specifications that include state-specific time trends while also allowing for dynamic responses.[3] Specifically, I focus on the following model:

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[1] If the state-specific trends are not linear, adding the state-specific trends and modeling the dynamic response of divorce would still not be enough to perfectly solve the problem.

[2] Stevenson included all women aged 14 years or greater in her sample. Since the change in divorce laws may have little effect on women who are too young (younger than 18), using these observations may attenuate the real effects on young and middle-aged adult women. I also checked the effects on women who are older than 50 and found that the change in divorce laws does not affect them. Therefore, in this paper I use a sample that only includes women between the age of 18 and 49.

[3] In all cases, I also estimate models without state-specific time trends, which yield similar results to models that include such trends. This similarity implies that the factors that influence female labor supply differently across states over time are not correlated with changes in divorce laws.

$$
\begin{aligned}
&Labor\ Force\ Participation_{s,t} \qquad (1)\\
&= \sum_{k\geq 1}\beta_k Divorce\ Law\ has\ been\ in\ effect\ for\ k\ periods_{s,t}\\
&+\sum_a \beta_a Age_{s,t} + \sum_r \beta_r Race_{s,t} + \sum_e \beta_e Education_{s,t}\\
&+\sum_s \beta_s State\ fixed\ effects_s + \sum_t \beta_t Time\ fixed\ effects_t\\
&+\sum_s \beta_{st} State_s * Time_t + \varepsilon_{s,t}
\end{aligned}
$$

In Equation (1), the dependent variable *Labor Force Participation* is women's state-level LFP rates in state s in year t. I follow Wolfers (2006) in adding variables meant to model the dynamic response of a change in divorce laws. These variables are dummy variables for one and two years before the new legal regime, first two years of the new legal regime, for three and four years, for five and six years, and so on. The *Age*, *Race* and *Education* variables indicate the share of each age, race, and education group, respectively, in each state and year. I control for state fixed effects, time fixed effects and state-specific time trends. In some specifications, I also control for quadratic time trends, and the results are very similar to those from regressions with only time fixed effects.

The data used in this paper comes from Current Population Survey (CPS), March Annual Demographic Files from 1977 to 2012.[4] I restrict the sample to married, spouse present women between the age of 18 and 49.[5] Table 1 shows some basic information about demographic and labor force participation for women in this sample. Specifically, women are on average 36 years old, and 88% of them are whites. Half of them attend college for at least 1 year. The number of children in the household is on average 1.68. In this sample, 68% of women are in the labor force, and around half of them have full-time jobs. I also present the basic information separately for the sample of women before and after the change in divorce laws. Before the law change, women earn less than they do after the law change. This is reasonable since personal income has increased gradually in the United States over the past thirty years.

Table 1
Summary Statistics

Variable Name	Variable Description	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
		Full Sample		Before Law Change		After Law Change	
Demographic							
Age	In years	35.84	7.85	32.83	8.45	35.86	7.84
White	=1 if woman is white	0.88	0.32	0.96	0.19	0.88	0.33
Black	=1 if woman is black	0.06	0.24	0.002	0.04	0.06	0.24

[4] I do not use CPS data before 1977 because most states were grouped together from 1968 to 1976. In previous research, Parkman (1992) uses 1979 CPS data. Gray (1998) uses three different data sets: 1960, 1970 and 1980 Census data; 1968 and 1979 CPS data; and 1970 and 1980 PSID data. Genadek et al. (2007) use 1960–1990 Census data. Only Stevenson (2008) uses a similar data set, the 1968–1995 CPS data in Table 5 in her paper. She also uses 1970 and 1980 Census data in her paper.

[5] It is possible that the change to unilateral divorce law may affect selection into marriage. For instance, Rasul (2004) showed that the marriage rate declined by about 3 to 4 percent following the adoption of unilateral divorce laws. In addition, Gray (1988) shows that after taking into account the selection into marriage, the results on divorce rates are similar to those when using just married samples.

College	=1 if woman attended college for at least 1 year	0.50	0.50	0.39	0.49	0.50	0.50
Number of Child	Number of own children in household	1.68	1.26	2.13	1.63	1.68	1.26
Labor Force Participation							
Work	=1 if woman is in the labor force	0.69	0.46	0.58	0.49	0.69	0.46
Full Time Job	=1 if woman has full time job	0.52	0.50	0.41	0.49	0.53	0.50
Working Weeks[6]	Weeks worked last year	32.68	22.73	26.93	22.52	32.72	22.73
Working Hours	Hours worked last week	26.27	18.52	22.55	18.80	26.30	18.51
Salary Income	Wage and salary income	14606.95	23839.9	3812.53	5432.26	14698.22	23914.5
Other Income	Non-wage and salary income	1893.06	8330.93	553.48	2371.49	1904.39	8362.32
Household Income	Total household income	58821.93	58808.56	23071.09	14698.03	59124.22	58948.83

Notes: Sample is restricted to married, spouse present women between the age of 18 and 49 in CPS 1977-2012. The number of observation is 837726. Aggregate data used in this paper is constructed from this individual sample.

Given that the divorce law variation is at the state level, I aggregate all my data to the state-year level.[7] The state-year level data is constructed from the sample that I describe above from the 1977–2012 CPS. I construct the labor force participation rates and share of the observations for each age group, race group and education group by state and year.[8] The unilateral divorce laws specification used in this paper is based on Gruber (2004).[9]

Table 2
Dynamic Effects of Unilateral Divorce Laws on Labor Force Participation Rates
(Without state-specific time trends)

	(1)	(2)	(3)	(4)	(5)	(6)
Specification:	WLS	OLS	WLS, Cluster	OLS, Cluster	WLS, Log(LFP)	WLS, Logit
1-2 years before	0.024	0.032**	0.024**	0.032**	0.040*	0.012
	(0.021)	(0.014)	(0.010)	(0.013)	(0.011)	(0.016)
0-2 years later	0.029	0.035**	0.029**	0.035**	0.041*	0.021
	(0.021)	(0.014)	(0.012)	(0.014)	(0.014)	(0.016)
3-4 years later	0.058***	0.064***	0.058***	0.064***	0.072***	0.052***
	(0.018)	(0.012)	(0.016)	(0.016)	(0.012)	(0.015)
5-6 years later	0.052***	0.061***	0.052***	0.061***	0.060***	0.052***
	(0.017)	(0.011)	(0.015)	(0.017)	(0.011)	(0.013)

[6] The summary statistics of working hours, working weeks and salary income in Table 1 are conditional on working.
[7] Since the unilateral divorce laws change at the state level, using individual level data is the same as using aggregate-level data and controlling for the average value of each background variable.
[8] When constructing these variables in the regression, I use the CPS sampling weights.
[9] In Table 1 in Gruber (2004), he documents the availability of unilateral divorce in each state from 1910 to the present based on Friedberg (1998) and a careful state-by-state review of the actual divorce laws.

7-8 years later	0.059***	0.055***	0.059***	0.055***	0.067***	0.060***
	(0.017)	(0.011)	(0.015)	(0.017)	(0.010)	(0.013)
9-10 years later	0.061***	0.058***	0.061***	0.058***	0.067***	0.063***
	(0.017)	(0.011)	(0.016)	(0.018)	(0.011)	(0.013)
11-12 years later	0.056***	0.053***	0.056***	0.053**	0.059***	0.060***
	(0.017)	(0.011)	(0.017)	(0.021)	(0.010)	(0.013)
13-14 years later	0.054***	0.053***	0.054***	0.053**	0.056***	0.060***
	(0.017)	(0.011)	(0.018)	(0.021)	(0.010)	(0.013)
>15 years later	0.045***	0.060***	0.045**	0.060***	0.045**	0.054***
	(0.017)	(0.010)	(0.020)	(0.022)	(0.010)	(0.013)
Observations	1,836	1,836	1,836	1,836	1,836	1,836
R-squared	0.876	0.860	0.876	0.860	0.876	0.865

Notes: Standard errors in parentheses. All regressions based on aggregate level data constructed from CPS 1977-2012. Sample restricted to married women age 18 to 49. Dependent variable is LFP rates. Control variables include: share of each age group in each state and year, share of each race group and education group in each state and year, state fixed effects and time fixed effects. The results in column (5) and column (6) are marginal effects and the standard errors come from Bootstrapping. ***significant at 1% **5% *10%.

Table 2 presents estimates of Equation (1) without state-specific time trends and quadratic time trends while Table 3 presents the estimates with state-specific time trends. Column (1) in both tables shows the results from a basic specification: weighted least squares (WLS), weighting each state and year observation by the state's population. The coefficients on the dynamic responses in Column (1), Table 2 imply that women's labor force participation rates do not significantly increase before the change in divorce laws and even in the first two years after the change. However, after 3 or more years of the change, women's labor force participation rates increase more than 5 percentage points, an effect that is roughly constant across all time periods. More strikingly, after adding state-specific time trends, we can find from Column (1), Table 3 that the effects on female labor force participation rates become even stronger in both the short term as well as the long term.[10] Specifically, the coefficient of 1–2 years before the change in divorce law is 0.061, which means that women's labor force participation rates increase significantly even before the change. As I have controlled for state-specific time trends in Table 3, the pre-law-change effects may not be endogenous trends if the state-specific time trends are linear. This could be the policy lead effects. People may change their LFP decisions even before the unilateral divorce laws have been changed, if they anticipate that this law would be passed in a few years.[11] The coefficient of *divorce law has been in effect for 0–2 years* is 0.074, and this effect increases to nearly 10 percentage points 3–4 or more years after the law change. This pattern suggests that some people react before the change, but many other people need time to adjust their labor supply based on the new divorce laws. As I have discussed before, the estimates from regressions with state-specific time trends

[10] I test the equality of the coefficients in column (1), Table 2 and those in column (1), Table 3. They are marginally significantly different in 10% level.

[11] Since I could not check the pre-trends due to lack of data, I could not completely rule out the possibility that the significant pre-trends in Table 2 and 3 are caused by mis-specification.

may be less biased than the estimates in Table 2 if state-specific time trends are linear. If some factors influence female labor supply differently across states over time, excluding state-specific time trends will induce biased estimates. The results of the basic specification imply that divorce laws have robustly positive effects on female labor force participation, even in the long term.[12] However, we need to examine several important issues before reaching a definitive conclusion.

Table 3
Dynamic Effects of Unilateral Divorce Laws on Labor Force Participation Rates
(With state-specific time trends)

	(1)	(2)	(3)	(4)	(5)	(6)
			WLS,	OLS,	WLS,	WLS,
Specification:	WLS	OLS	Cluster	Cluster	Log(LFP)	Logit
1-2 years before	0.061***	0.055***	0.061***	0.055***	0.070***	0.056**
	(0.021)	(0.013)	(0.013)	(0.016)	(0.016)	(0.014)
0-2 years later	0.074***	0.063***	0.074***	0.063***	0.079***	0.070***
	(0.022)	(0.014)	(0.018)	(0.022)	(0.017)	(0.015)
3-4 years later	0.101***	0.095***	0.101***	0.095***	0.109***	0.095***
	(0.021)	(0.015)	(0.018)	(0.020)	(0.018)	(0.016)
5-6 years later	0.099***	0.098***	0.099***	0.098***	0.103***	0.097***
	(0.021)	(0.015)	(0.020)	(0.022)	(0.017)	(0.016)
7-8 years later	0.104***	0.095***	0.104***	0.095***	0.107***	0.103***
	(0.022)	(0.015)	(0.022)	(0.025)	(0.018)	(0.017)
9-10 years later	0.108***	0.098***	0.108***	0.098***	0.109***	0.106***
	(0.022)	(0.016)	(0.023)	(0.024)	(0.018)	(0.017)
11-12 years later	0.104***	0.097***	0.104***	0.097***	0.103***	0.105***
	(0.022)	(0.016)	(0.022)	(0.023)	(0.019)	(0.017)
13-14 years later	0.106***	0.099***	0.106***	0.099***	0.103***	0.108***
	(0.022)	(0.016)	(0.023)	(0.023)	(0.019)	(0.017)
>15 years later	0.107***	0.111***	0.107***	0.111***	0.103***	0.111***
	(0.023)	(0.017)	(0.025)	(0.025)	(0.020)	(0.018)
Observations	1,836	1,836	1,836	1,836	1,836	1,836
R-squared	0.911	0.900	0.911	0.900	0.916	0.901

Notes: See notes of Table 2. Control variables also include state-specific time trends. ***significant at 1% **5% *10%.

The basic specification in Column (1) assumes that the error term in each weighted regression is homoskedastic and serially uncorrelated. However, the residuals may have strong serial correlation, and ignoring these autocorrelations could lead to bias in the estimation of standard errors. I use Stata's *cluster* option to implement Arellano's (1987) method of correcting standard error estimates for both serial correlation and heteroskedasticity. The

[12] I also use regression with both state-specific time trends and quadratic time trends. The results do not change a lot when including higher-order state-specific time trends. They are quite similar with those in Table 3.

results in Column (3) in Tables 2 and 3 cluster at the state level. The standard errors in these specifications are similar to those in column (1), which means that autocorrelation is not a serious concern in this setting.

Secondly, only if the error terms for individuals within the state are homoskedastic and independent of each other, weighting by population leads to efficient coefficient estimation. However, error terms are not always homoskedastic. Based on the conclusion of Dickens (1990), it is likely that individual error terms are positively correlated. Then OLS applied to aggregate data may be more efficient than WLS.[13] To check this issue, I also use OLS and OLS *cluster* to run Regression (1), and the results are shown in Columns (2) and (4) of Tables 2 and 3. Now all the coefficients of dynamics response have the same pattern as those in Columns (1) and (3). WLS and OLS producing similar results is consistent with the models being correctly specified for measuring the effects on female labor supply in the light of DuMouchel and Ducan (1983). They emphasized that if the estimation model is correctly specified, both WLS and OLS are consistent.

The regression models discussed above are all linear. To analyze how the results are affected by alternative specifications, I also try nonlinear functional forms for the dependent variable. Specifically, since the LFP rate is a fractional variable and always positive, I use the model for the logarithm and the logit[14] of the labor force participation rate. The coefficients are shown in the last two columns in Tables 2 and 3.[15] All the effects on the LFP rate are still positive and statistically significant. This is also consistent with the results from other specifications.

Table 4
The Effects of Unilateral Divorce Laws on Labor Force Participation Rates:
Different Specifications

Specification:	(1) Stevenson, 2008 column (3), Table 5	(2) Stevenson, 2008 column (4), Table 5	(3) Stevenson, column (3), Table 5: Replication	(4) Add Controls	(5) Add Controls & State Time Trend	(6) Add Controls & State Time Trend, 18-49 Women	(7) Add Controls & State Time Trend, 18-49 Women, 1968-2012
1-3 years prior to change	-0.003	0.000	0.003	0.003	-0.003	0.009	0.026*
	(0.010)	(0.009)	(0.010)	(0.008)	(0.011)	(0.013)	(0.014)
Year of change	-0.002	0.017*	0.001	0.022**	0.015	0.031*	0.055**
	(0.013)	(0.010)	(0.011)	(0.010)	(0.015)	(0.017)	(0.023)
1-3 years later	0.001	0.017**	0.009	0.030***	0.023	0.041**	0.064***
	(0.010)	(0.008)	(0.009)	(0.008)	(0.015)	(0.019)	(0.018)
4-6 years later	0.010	0.027***	0.017*	0.041***	0.026	0.057**	0.086***
	(0.009)	(0.008)	(0.009)	(0.008)	(0.019)	(0.024)	(0.023)
7-9 years later	0.004	0.026***	0.009	0.038***	0.017	0.048*	0.082***

[13] If the individual-level error term is like: $v_{ij} = c_i + u_{ij}$, where c_i is unobserved group-level factors in common. Then the variance of the group-average error term v_i is: $Var(v_i) = \sigma_c^2 + (\sigma_u^2 / J_i)$. If σ_c^2 is substantial and the sample size J_i is sufficiently large, the variance of the group-average error term may be dominated by σ_c^2, which is homoskedastic. In this case, OLS is better than WLS.

[14] The dependent variable is log[p/(1-p)] where p is the labor force participation rate.

[15] The coefficients in these two columns are marginal effects, and the standard errors come from Bootstrapping. Therefore, they are comparable with results in other columns.

	(0.010)	(0.008)	(0.009)	(0.008)	(0.020)	(0.025)	(0.025)
10 years or more later	0.016	0.027***	0.012	0.043***	0.015	0.040	0.083***
	(0.008)	(0.009)	(0.010)	(0.009)	(0.021)	(0.026)	(0.025)
Observations	1116	1116	1,116	969	969	969	1,836
R-squared	/	/	0.903	0.896	0.922	0.912	0.898

Notes: Standard errors in parentheses. All regressions based on aggregate level data. Sample restricted to married women age 14 or older. Dependent variable is LFP rates for all columns. Standard errors are robust as those in Steven (2008)'s paper. The results in column (1) are in column (3), Table 5 of Stevenson, 2008 paper. In column (2) I use the same data that are 1968-1995 CPS data to replicate the results. In column (3), I add control variables that include: share of each age group in each state and year, share of each race group and education group in each state and year, state fixed effects and time fixed effects. In column (4) I add state-specific time trends. In column (5) I restrict the sample to 18-49 years old women and in column (5) I use observations from 1995 to 2012. ***significant at 1% **5% *10%.

Because Stevenson (2008) used 1968–1995 CPS data, which is similar to the data I use in this paper, in Table 5 in her paper, it is important to compare my results with hers. In Columns (1) and (2) of Table 4, I copy the results in Columns (3) and (4) of Table 5 in Stevenson's paper. The sample is restricted to married women who are 14 years or older. In Column (3), I use the same data, 1968–1995 CPS, to replicate the results in Column (1). I only add state fixed effects and year fixed effects, which are the same as the controls used in Column (1). The estimation coefficients in these two columns are insignificant and very similar to each other. In Column (4), I add control variables that include the share of each age group in each state and year, the share of each race group and education group in each state and year, state fixed effects and time fixed effects, which are the same as those in Tables 2 and 3 in my paper.[16] After adding control variables, the coefficients become larger and strongly significant. Both the results in Columns (2) and (4) with control variables are significant, and the estimates in Column (4) are even bigger. In Column (5), I add the state-specific time trend. Compared to Column (4), the coefficients become smaller, and the standard errors become larger. As a result, the estimates are not significant. In Column (6), I restrict the sample to married women between the ages of 18 and 49. In Column (7), I extend the sample from 1968–1995 CPS data to 1968–2012 CPS data. Both changes make the estimation coefficients bigger. As I discuss above, because the change in divorce laws may have little effect on women who are too young or too old, using these observations may attenuate the real effects on young and middle-aged adult women.

In summary, based on what I showed in Tables 2 and 3, the results are robust to variation in estimation methods and functional forms.[17] According to the results in Lee and Solon (2011), the effects on divorce rates are still unclear; however, that is not the case here. Based on the results in Table 3 that come from probably preferable specifications, changes in divorce laws have strong effects on female labor force participation both in the short term as well as in the long term.

[16] According to Stevenson's paper, column (2) also add control variables that include the maximum AFDC rate for a family of four; existence of the AFDC unemployed parent and food stamp programs; the natural log of state personal income per capita, the unemployment rate; age composition variables indicating the share of states' populations aged 14-19; and then ten-year cohorts beginning with age 20 up to a variable for 90+; the Donohue and Levitt Effective access; and the share of the state's population that is black, white and other.

[17] I also use models that include both linear and quadratic time trends. The results from these models, which I do not include in the paper, are nearly identical to the results from models that only include linear trends.

3. Working Week, Working Hour and the Full-Time Job

Most previous research only focuses on the effects on extensive margin of labor supply, i.e., labor force participation.[18] However, people may change their types of jobs or working hours even if they remain in the job market since completely exiting the labor market is a big decision. Therefore, in this paper, I investigate the effects on a special case of LFP, i.e., full-time jobs. Furthermore, we would naturally like to know the effects on the weeks and hours worked as well. This part of the paper focuses on a model that is similar to Equation (1). The dependent variable represents the average *weeks worked last year*, *usual hours worked per week last year* or *LFP for full-time job* for married, spouse present women between the ages of 18 and 49 in state s in year t. The dummy variables for dynamic response here are different from those in equation (1). Since the coefficients for these dummy variables are quite similar in Tables 2 and 3, it is better to use a more concise model. In this section, the dynamic response variables that I use are dummy variables for one to two years before the new legal regime, for first two years of the new legal regime, for three to four years and for 5 and more years. The definitions of independent variables *Age*, *Race*, *Education* and other variables are the same as those in Equation (1).

Table 5
Dynamic Effects of Unilateral Divorce Laws on
Weeks and Hours of Work and LFP of Full Time Job

Specification:	(1) OLS, Cluster Weeks Worked, Unconditional	(2) OLS, Cluster Weeks Worked, Conditional	(3) OLS, Cluster Hours Worked, Unconditional	(4) OLS, Cluster Hours Worked, Conditional	(5) OLS, Cluster LFP of Full Time Job
1-2 years before	1.482**	0.090	0.802	-0.445	0.006
	(0.659)	(0.216)	(0.630)	(0.298)	(0.020)
0-2 years later	2.400***	0.726	1.825***	0.598	0.027
	(0.820)	(0.696)	(0.538)	(0.493)	(0.020)
3-4 years later	2.938***	0.379	2.009***	0.003	0.037
	(1.080)	(0.701)	(0.659)	(0.367)	(0.026)
>5 years later	3.336***	0.352	2.433***	0.259	0.043
	(1.010)	(0.646)	(0.690)	(0.473)	(0.028)
Observations	1,836	1,836	1,836	1,836	1,836
R-squared	0.922	0.908	0.884	0.822	0.853

Notes: See notes of Table 2. Dependent variables are weeks worked last year unconditional on participation in the labor force in column (1), weeks worked last year conditional on participation in the labor force in column (2), usual hours worked per week last year unconditional on participation in the labor force in column in column (3), usual hours worked per week last year conditional on participation in the labor force in column in column (4) and LFP of full time job in column (5). Control variables also include state-specific time trends. ***significant at 1% **5% *10%.

[18] Genadek, Stock and Stoddard (2007) use OLS to get the effects on weeks worked last year and hours worked last week. However, they do not check dynamic effects and also do not add state-specific time trends.

Table 5 reports the OLS *cluster* estimates,[19] using *weeks worked last year unconditional on participating in the labor force*, *weeks worked last year conditional on participating in the labor force*, *usual hours worked per week last year unconditional on participating in the labor force*, *usual hours worked per week last year conditional on participating in the labor force* and *LFP rates for full-time job* as dependent variables.[20] The results presented in Column (1) of Table 5 indicate that, unconditional on participation in the labor force, women work several more weeks per year even before the law change and the effects increase gradually. Specifically, one to two years before the law change, women work around 1.48 more weeks. Within two years after the change, women work nearly 2.4 more weeks. After that, the weeks that they work increase. In the long term, they work around 3.3 more weeks. This is a very large effect. As noted before, the reason that the effects within two years are smaller is probably because people need some time to change their expectations of their marriage and then adjust their labor supply behavior. In Column (3), we can find that, unconditional on participation in the labor force, women also increase their working hours per week after the law change. They work roughly 1.8 more hours per week immediately after the laws changed and continue gradually increasing hours worked. In the long term, women in reformed states work around 2.4 hours more per week than their counterparts in other states. Based on the results in Columns (2) and (4), it is clear that, conditional on participation in the labor force, there is not a significant difference in either weeks worked last year or hours worked per week last year. Lastly, in Column (5), a similar pattern could be seen. In the short run, the full-time job participation rate increases 2.7 percentage points. After 5 or more years of the law change, the participation rate increases still more, up to 4 percentage points. However, the effects on LFP of full-time jobs are not significant. It is also possible that the results I find above are affected by selection. These results may imply that women who are induced to enter the labor force prefer to work longer than the average level of weeks that women worked last year and hours worked per week last year unconditional on participation in the labor force.

4. Summary and Discussion

In this paper, I expand upon previous analyses of effects of unilateral divorce laws on female labor supply and test alternative specifications. To accurately measure the results, I first investigate the dynamic effects with state-specific time trends. In addition, I carefully check the sensitivity of the effects on female labor supply with other estimation methods and functional forms. Previous research on the effects of unilateral divorce laws on divorce rates have found extremely fragile results; therefore, the impact of changes in divorce laws on divorce rates remains unclear. In this paper, I find more robust results that suggest that there are strong effects on the female labor force participation rate even in the long term. The robustness of effects on female LFP is different from the fragile results of divorce rates in the previous literature. There are also strong and long-term effects on weeks worked per year and

[19] According to the results in Tables 2 and 3, we can find that the standard errors from WLS are bigger than those from OLS. Thus, Dickens' argument is correct in this case; OLS is more efficient. Therefore, in the rest of the paper, I use OLS instead of WLS.

[20] I also check the sensitivity of results for *weeks worked last year* and for *usual hours worked per week last year*. Based on these results, I find that the results for *weeks worked last year* and for *usual hours worked per week last year* are also very robust.

usual hours worked per week. Though I find robust and strong results by using different specifications in this paper, these results should still be treated with caution. First, I could not check and account for pre-trends because of the lack of data; therefore, the results might be biased if there are pre-trends. Second, the identification is tenuous by using differences in differences when there are complicated dynamics.

According to Lee and Solon (2011), "the DID research design with unit-specific time trends is essentially a type of regression discontinuity design, with time as the 'running variable'…When the shift in the dependent variable may vary with the length of time since the policy change, and especially when that complication is accompanied by other differences across states in time trend, the sharpness of the identification strategy suffers." Therefore, when using the same identification strategy by exploiting the change in divorce laws, the results of other outcomes may also be sensitive as those of divorce rates. However, the results on female labor supply suggest much more robust effects. After controlling for state-specific time trends, no matter which estimation method and functional form I use, the results show that female labor force participation rate increases substantially. Why is there such a difference between the sensitivity of results on divorce rates and that on female labor supply? Perhaps the unilateral divorce laws have little effect on divorce rates since any decision to divorce is relative to a small portion of people who are close to divorce. Because the effects are so small, it is hard to measure them precisely and find any robust answer. On the contrary, unilateral divorce laws may have strong effects on female labor supply by changing the expectation of marriage among all adults, but not through divorce. In other words, since all married women, not just the women close to divorce, need to reconsider their labor force participation decisions, the effects could be very large and easy to measure. As a result, the identification strategy is sharper than that of effects on divorce rates.

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