ECMA 31320 Final Project

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1 Introduction

1.a Motivation

In the 1960s and 1970s, many American states passed unilateral divorce laws, which allowed a single spouse to dissolve the marriage without requiring the consent of the other. During that same period, post-divorce property division rules were also amended to promote more equitable asset distribution. These widespread changes had significant implications and made an impact on marriage relations, intra-household decision-making, and female labor force participation. In this paper, we examine how mid-late twentieth century divorce legislation impacted household bargaining dynamics, incorporating recent developments in the difference-in-differences literature to provide a more precise analysis. Our particular focus is examining how household bargaining dynamics were affected by these new laws. Because household bargaining is very difficult to observe, we use the wife and husband's annual housework hours as as proxy for the bargaining power of each spouse, following the household bargaining literature (see Hersch and Stratton (1994)[9], Hersch and Stratton (1997)[7], and Hersch and Stratton (2002)[8]).

1.b Legislative Background

Prior to the passage of unilateral divorce laws, state regulation only allowed divorce under mutual consent or fault grounds. Mutual consent required both parties agree to file for divorce; fault grounds for divorce consisted of proving the opposing party had committed adultery or domestic violence. These laws were widely viewed as inadequate

for several reasons. First, filing for mutual consent divorce was a complicated processes and came with enormous financial and emotional transaction costs. Second, mutual consent laws ignored within-household power disparities that often placed one spouse in a position where they had to acquiesce to the decision of the other. Further, requiring fault grounds for divorce prevented spouses from dissolving a marriage that neither party wanted Gruber (2004)[6].

These concerns led to the unilateral divorce revolution, which allowed one spouse to obtain divorce without the consent of the other Voena (2015)[19]. The introduction of unilateral divorce was considered an attempt to rectify the inefficiencies of existing divorce laws, making divorce easier Gruber (2004)[6]. Figure 1 displays how states gradually adopted unilateral divorce. Although unilateral divorce laws were rare before the 1960s, by the mid-1970s, over 30 states had passed the law. By 1976, most states had unilateral divorce with few states newly adopting it in the period between 1976 and 1991. These laws have been a long-standing topic of interest for researchers who have examined curious about their impact on rising divorce rates, female labor supply and changes in intra-household decision-making.

1.c Property Distribution Laws

Voena (2015)[19] finds that the impact of unilateral divorce law depends on the divorce property distribution laws in place. Thus, in our study we consider the interaction between property division laws and unilateral divorce passage. During our period of analysis, from 1968 to 1989, states had one of three following types of post-divorce property allocation laws[1]:

- *Title-based laws*, where assets are divided based upon the title of ownership.

- *Community property laws*, where assets are divided equally between both spouses under the assumption of joint ownership.

- *Equitable distribution laws*, where divorce courts decide assets distribution in an equitable manner. This interpretation is intentionally flexible and for example, may favor the spouse who contributed more towards asset acquisition or to the spouse who has greater financial need.

Variation in property distribution law likely caused households to respond to unilateral divorce laws differently. We hypothesize that in title-based regimes, household

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[1]These definitions are taken from Voena (2015)[19].

bargaining power shifts away from the spouse who purchased fewer assets because in the event of a divorce, they will have access to a smaller percentage of assets, assuming that assets were divided evenly during marriage. In community property states, the spouse who has purchased fewer assets increases bargaining power because assets will always be split equally between both parties. Equitable distribution laws are harder to interpret, but their intent is to protect vulnerable parties by granting courts discretion in redistributing household assets. Thus, we hypothesize that equitable distribution improve the bargaining position of the spouse with less power prior to the passage of unilateral divorce laws.

Under the assumption that household assets are disproportionately held in the husband's name Gray (1998)[5], the adoption of unilateral-divorce laws implicitly redistributed assets towards the husband in title-based states and towards the wife in community-property and equitable distribution states. Indeed, Voena (2015)[19] finds that households who lived in community property and equitable distribution states adjusted their asset accumulation behavior in ways consistent with the expectation of these different distributions following the passage of unilateral divorce laws, while no effect is observed in titled-based states. As such, we anticipate that in states with community property or equitable distribution laws, unilateral divorce laws will cause a greater increase in household bargaining power for the wife. In our analysis, based off prior literature, since we expect both community property and equitable distribution to magnify the impact of unilateral divorce laws on household bargaining dynamics for wives, we do not distinguish between the two types of laws. We refer to both types of laws as equitable distribution throughout our paper.

1.d   Adoption Trends

Figure 1 shows trends in the adoption of unilateral divorce and equitable distribution laws. Prior to 1967, mutual consent was the dominant legal regime in the United States and only only 3 states permitted unilateral divorce. However, from 1967-1991, the number of states passing unilateral divorce grew to 36, with the bulk of adoption occurring in the late 1960s and early 1970s. By 1971, almost half of states had equitable distribution. By the end of our study period in 1989, all 50 states and D.C. had equitable distribution. Mississippi was the last state to pass equitable distribution in 1989.
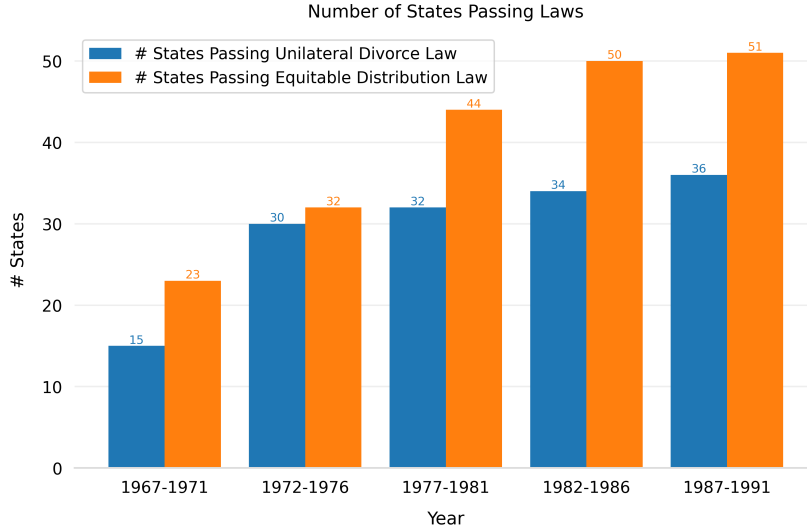
Figure 1: Passage of Divorce Laws Over Time

1.e Mechanism

Our empirical model is motivated by the household bargaining literature, especially Hersch and Stratton 1997[7]. In their bargaining model, each spouse's intra-household bargaining power is related to their next-best alternative, also known as the the "threat point". In this paper, we proxy for each wife's "threat point" through individual characteristics such as educational background and labor force participation, which define the credibility of their divorce threat. We believe that unilateral divorce will improve the bargaining position of all wives, but that working women, who we hypothesize have higher divorce threat credibility because they are less reliant on their marriage for financial support, can gain a larger increase in bargaining power than non-working women.

After the unilateral divorce reforms starting in the 1960s, spouses could credibly exercise their threat of divorce, as they are no longer reliant on obtaining the consent of the other party to terminate the marriage. Hence, our proposed mechanism is that the introduction of these laws will allow one spouse to threaten the other with divorce in order to negotiate changes in housework only after the passage of unilateral divorce. Thus, we examine the impact of divorce threat credibility on household bargaining by examining changes in the wife's annual housework time allocation.

Unilateral divorce law affects household bargaining because the credibility of divorce threat is also contingent on the difficulty of obtaining a divorce. As described earlier, prior to the passage of unilateral divorce laws, divorce could only be obtained under very specific conditions. With its introduction, spouses will be able to credibly

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threaten divorce. We anticipate that there will be heterogeneity in how it affects particular individuals. In particular, we believe that women who are higher educated or who have experience in the labor force prior to the introduction of unilateral divorce will be able to negotiate down their hours of housework more than who women who are less educated or do not have work experience. This is because women who are educated or have a stronger work history are considered to have a better outside option, so they can more credibly threaten divorce.

2 Data

Like many others who have studied unilateral divorce in the past, we use the Michigan Panel Study of Income Dynamics (PSID). We use annual panel household surveys from 1968 until 1989, which consists of 4803 households. We subset to only include married households and remove all households who first appear in the dataset after their state of resident passes unilateral divorce laws. These individuals are referred to as the always-treated group, and our empirical method requires that we remove all members of this group. This group also consists of all individuals who live in states that pass unilateral divorce laws before 1968. We also remove individuals who ever move to a different state. Finally, we exclude states that pass unilateral divorce after 1980 but before 1989, as Wolfers (2006)[20], citing Jacob (1988)[10], tells us that unilateral divorce law adoption timing was plausibly random only for states that adopted it between 1968 and 1980. This is a feature that we are interested in exploiting, so we exclude West Virginia, South Dakota and Utah from our analysis, who were the only states to pass unilateral divorce in the period between 1980 and 1989.

Our final dataset consists of 2764 households who are present on average for 12 years. To preserve the i.i.d nature of our dataset, we define our treatment group as specifically married, unseparated households who formed their household prior to the passage of unilateral divorce laws and who do not move between states while observed in our data. Of this group, 69% (a total of 1910) of households live in states that never pass unilateral divorce laws. The PSID also provides crucial household level information on labor force participation, income, age, education and home production for both spouses, which we use as covariates and outcomes in our empirical analysis.

Next, we reference Voena (2015)[19] for data on the timing of unilateral divorce and equitable distribution law passage across states. Figure 9 in the appendix includes a list of states and when they passed unilateral divorce or equitable distribution. This figure is taken directly from Voena (2015)[19]. Curiously, we do not observe any re-

gional trends in the passage of unilateral divorce laws. It is crucial that we expect states that do and don't pass unilateral divorce laws to have residents who are approximately all else equal. This is a concern because unilateral divorce laws are policies that state legislatures choose to pass. If legislatures passed unilateral divorce laws in states with more progressive attitudes, we could expect to see other covariates affecting our outcome, which could confound our analysis of the impact of unilateral divorce laws.

3 Literature review

Early foundational papers studied the impact of unilateral divorce on female labor force participation. Johnson and Skinner (1986)[11] found that it decreased while Peters (1986)[14] and Parkman (1992)[12] concluded that female labor force participation increased. We build on this literature by using female labor force participation as a pre-treatment covariate to examine heterogeneity in treatment effects along this dimension.

Our paper's primary focus is home production, a divergence from the focus on marital assets and investment that other papers in the unilateral divorce and household bargaining literature have focused on Stevenson (2007)[16] and Voena (2015)[19]. Parkman (1998)[13] found that women work more and spend less time in leisure in states with unilateral divorce.

Gray (1998)[5] found that whether women increase home production in response to unilateral divorce depends on the type of property distribution laws in place. Recently, Genadek (2018)[3] and Roff (2017)[15] have examined the impact of unilateral divorce laws on household work time allocation. They find that after unilateral divorce laws were passed, women spent fewer hours in home production activities and did a smaller share of housework within couples. Genadek (2018)[3] reports that unilateral divorce causes a decrease of between 1.45 and 1.63 less hours of daily housework. Roff (2017)[15] finds a much smaller decrease, around .29 hours of daily housework, although their results are also not statistically significant. Our contribution to this literature is both methodological and empirical. We use panel data to estimate the change in the impact of unilateral divorce laws over multiple time periods as opposed to identifying one pre and post estimator. This makes our estimates more precise and provides a more complete picture of the treatment effect. Moreover, we also estimate the impact of the differential timing of unilateral divorce laws, an empirical contribution to the literature.

Finally, the primary empirical method of choice for past empirical papers has been standard difference-in-differences, static two-way fixed effects and dynamic two-way fixed effects. Some of these methods might be potentially problematic to use given

our setting. Estimation using standard difference-in-differences often uses data from two time periods: one pre-period and one post-period. In our setting, because states pass unilateral divorce at different times, a pre-period and post-period that corresponds with the fact that states passed unilateral divorce over a 20+ year span would require a substantial year gap between the pre-period and post-period. Doing a difference-in-differences analysis over a such a large period incurs the risk of possibly confounding the treatment effect with other policies that may have passed between the pre and post-periods. Standard difference-in-differences also fails to account for the fact that different treatment groups have experienced the treatment for differential amounts of time, making treatment effect interpretation difficult.

Static two-way fixed effects, which allows for staggered policy adoption, is also biased unless one operates under very strict assumptions as outlined by Goodman-Bacon (2021)[4]. One of these assumptions is that the treatment effect does not evolve over time - which is problematic in this case because we expect household bargaining dynamics to change over time. Genadek (2018)[3] and Roff (2017)[15] use the methods of above to study a question similar to our own, so we hope to build upon their results using different methods.

We will show in section 4 that dynamic two-way fixed effects, while tempting to use in this context, will not be suitable. In section 5, we will use a method proposed in Callaway and Sant'Anna (2021)[1] to identify our results. Finally, in section 6 we summarize our results and identify areas of further work.

4 Dynamic Two-Way Fixed Effects

4.a Empirical Specification

When researchers are interested in how the impact of a policy changes over time and the policy's adoption is staggered, they have historically relied upon dynamic two-way fixed effects to estimate such results.

The methodological setup is as follows. Suppose we have data on units i between years $-K$ and L. Over the course of this period, some units will be treated and others not. Let us call the units that will be eventually treated our treatment group and those that do not receive the treatment our control group. If all units in our treatment group simultaneously become treated at time t^* and it can be reasonably assumed that, in the absence of treatment, on average, units in our treatment and control groups would follow parallel trends in the outcome variable, researchers often might

consider a year-by-year difference-in-differences regression specification, as displayed in equation 1 below,

$$Y_{it} = \alpha + \beta \text{Treat}_{it} + \sum_{\substack{j=-K \\ j \neq t^*-1}}^{L} \phi_t \mathbf{1}\{j = t\} + \sum_{\substack{j=-K \\ j \neq t^*-1}}^{L} \delta_t \mathbf{1}\{j = t\} \times \text{Treat}_{it} + \epsilon_{it} \qquad (1)$$

where Treat_{it} is an indicator if unit i has been treated in year t. Note that the year prior to treatment, $t^* - 1$, is excluded to avoid perfect multicollinearity between the year indicators and constant term. The sequence

$$\delta_t \ \ s.t. \ \ t \geq t^*$$

is then interpreted as a time varying average treatment effect on the treated and the sequence

$$\delta_t \ \ s.t. \ \ t < t^*$$

is used as a falsification test for the parallel trends assumptions. Assuming no anticipation, finding $\delta_{-K} = \delta_{-K+1} = \cdots = \delta_{t^*-2}$ would indicate that there are no pre-trends, which is often used as evidence to support the parallel trends assumption.

Often however, researchers are still interested in a time-varying treatment effect, but need to grapple with policies with staggered roll out in which units are not all treated at the same time. One solution to this problem is by using a generalization of the year-by-year difference-in-differences regression equation called the dynamic two-way fixed effects regression. This regression is used with the intention of identifying time-varying average treatment effects on the treated within *event-study* time rather than *calendar* time. The structure of the regression equation is,

$$Y_{it} = \tau_i + \sum_{j=-K}^{L} \lambda_t \mathbf{1}\{j = t\} + \sum_{\ell \in \mathcal{L}} \mu_\ell \mathbf{1}\{t - G_i = \ell\} + \nu_{i,t} \qquad (2)$$

Where τ_i are unit fixed effects, $\mathcal{L}$ is the set of event-study time periods under consideration, and G_i is defined as

$$G_i = \begin{cases} g & \text{unit } i \text{ is treated in year } g \text{ within our study period.} \\ 0 & \text{unit } i \text{ is not treated in our study period.} \end{cases}$$

Further, let $\mathcal{G} \equiv \text{supp}(G_i)$ and say unit i is in group g if $G_i = g$.

We use Tewari (2014)[18] as an example of how to interpret coefficients in this

regression specification. In her analysis of the effect of staggered bank deregulation on mortgage access, Tewari (2014)[18] uses the coefficients μ_ℓ in two ways. The sequence

$$\mu_\ell \ \ s.t. \ \ \ell < 0$$

are used to test her identifying assumption that absent treatment, units treated at time t and units treated at time $s > t$ would follow parallel trends in the outcome variable. Tewari (2014) argues that because her pre-treatment coefficients are noisy and approximately zero, her treatment and control groups do not experience differential pre-trends, which is suggestive that the parallel trends assumption may hold. Tewari (2014)[18] uses the sequence

$$\mu_\ell \ \ s.t. \ \ \ell > 0$$

to assess the dynamic effect of deregulation.

One additional comment is that covariates are sometimes included as controls in equation 2 if the identifying assumptions are only true conditional on covariates.

We will now briefly explain what this regression would look like given our institutional framework before describing the assumptions we would need to meet to interpret our coefficients in the way that they have been interpreted in other papers using dynamic two-way fixed effects regressions.

We have two regression specifications each of the form of equation 2. In each of the two specifications, our outcome variable, Y_{it}, is hours of housework done by the wife in household i in year t. In the first specification, G_i is equal to the year in which unilateral divorce was passed or equal to 0 if the household is located in a state does not pass unilateral divorce before 1989. In the second specification G_i is equal to the first year in which an individual's state had both unilateral divorce and either equitable distribution or community property. G_i is equal to 0 if the household is located in a state that does not pass unilateral divorce *and* equitable distribution or community property before 1989. The purpose of this second specification is to allow for the possibility that legislation regarding the distribution of assets post-divorce impacts household bargaining.

We run two regressions - one with the full sample and one with only "working women". We subset based off of work history because work history causes variation in the credibility of the threat of divorce. Women who have more experience in the labor force likely have a better outside option. They may have more human capital or a larger professional network. These assets would make them less financially dependent upon the marriage and thus make their threat of divorce more credible.

Defining a woman's work history is challenging in our setting for a few reasons. First, we do not have much data on women prior to the reform period in the 1970s, so it is difficult to get a sense of who has an extensive work history. Second, we want to define a definition of working that applies to both treatment and control groups. We are concerned that it is methodologically unsound to subset on whether an individual works in a post-treatment period because this could reflect particular individuals selecting into employment *in response to* treatment. Therefore, we want to subset on a covariate realized in a pre-treatment period. This becomes challenging when we have staggered treatment because we want to subset our population on covariates realized prior to treatment, but treatment dates differ and some individuals are never treated. One might be tempted to define "working women" as those working full-time in the year prior to treatment. However, applying such a definition to our control group is difficult because our control group does not have a "treatment year".

Thus, we propose the following as an imperfect proxy for being a "working woman", acknowledging that this may create control and treatment groups that are from slightly different distributions. For a woman whose state is eventually treated, we designate her as a "working woman" if she worked more than 1200 hours[2] in each of the years leading up until the year her state passed unilateral divorce or 1973, whichever came first. We choose 1973 as a reasonable end-point for defining working women because there is a recession in 1973 and an individual could still have a strong past work history even if they were laid off in a recession. For women in states that did not pass unilateral divorce in our study period, we designate a woman as being a "working woman" if she worked at least 1200 hours in each of the years up to 1973. We acknowledge that these two samples are not part of the same population, as some states passed unilateral divorce before 1973. However, we chose to impose this restriction that women worked up to the year of unilateral divorce in order to ensure that we were not subsetting our sample on the value of a covariate realized post-treatment. Providing a more apples-to-apples definition of "working women" will be an objective we hope to pursue in further work.

We anticipate that we will have stronger negative treatment effects for this subset of the population because women who live in states with unilateral divorce who do not have prior work experience will not necessarily be able to bargain down their housework hours. Therefore, if we remove non-working women from our sample, we may see expect

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[2]We chose 1200 hours as a benchmark for working because it corresponds with 30 weeks at 40 hours per week. Ideally, we would a larger minimum number of hours, but because we already lack observations, we chose a smaller value, so we may still estimate effects for this sub-population.

to see larger increases in the bargaining power of wives.

In order to interpret the coefficients μ_ℓ in the way that they have been in the prior literature, Sun and Abraham (2021)[17] note that we need to satisfy three assumptions. We will first define some notation[3] in order to express these assumptions cleanly.

Let $Y_{i,t}(g)$ denote the potential outcome that unit i would have at time t were they to be first treated at time g. Let $Y_{i,t}(0)$ denote the potential outcome that unit i would have at time t were they to be never treated. In our paper, if woman j lives in a state that passed unilateral divorce in 1975 and we want to express her number of housework hours worked in 1971 had her state passed unilateral divorce in 1970, we could write this as $Y_{j,1971}(1970)$ and her true observed housework hours in 1971 as $Y_{j,1971} = Y_{j,1971}(1975)$. The previous equality holds because the observed outcome is identical to the potential outcome because her state passed unilateral divorce in 1975.

Let the group-time average treatment effect be defined as,

$$\text{ATT}(g, t) = \mathbb{E}[Y_{i,t}(g) - Y_{i,t}(0)|G_g = 1] \tag{3}$$

where $G_g \equiv \mathbf{1}\{G_i = g\}$. This parameter is interpreted as the causal effect at a time t of being first treated at year g relative to the counterfactual of being never treated for units who are members of group g. Fixing a group g, this definition allows us to trace out the trajectories of treatment effects over time by estimating

$$\text{ATT}(g, t_1), \text{ATT}(g, t_2), \ldots, \text{ATT}(g, t_n)$$

Further, because these treatment effects are specific to treatment dates we can compare treatment effects k years after treatment for groups g and $g' \neq g$ by comparing $\text{ATT}(g, g + k)$ and $\text{ATT}(g', g' + k)$.

4.b   Assumptions

After defining our causal parameter, we will describe the three assumptions needed to interpret the coefficients μ_ℓ following Sun and Abraham (2021)[17]:

Assumption 1 *(Parallel trends in the outcome variable)*:
For all $s \neq t$, $\mathbb{E}[Y_{i,t}(0) - Y_{i,s}(0)|G_g = 1]$ is the same for all $g \in \mathcal{G}$. Absent treatment, units must follow parallel trends in the outcome variable regardless of group g. In par-

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[3]Sun and Abraham (2021)[17] define these assumptions in terms of their notation, but in order to maintain internal consistency within our paper, we use the notation of Callaway and Sant'Anna (2021)[1]. The assumptions do not change.

ticular, never-treated units ($g = 0$) and ever-treated units ($g > 0$) must follow parallel trends in the outcome variable.

Assumption 2 *(No Anticipatory Behavior Prior to Treatment)*:
$\text{ATT}(g, g - l) = 0$ for all $g \in \mathcal{G}$ and $l > 0$. There is no treatment effect in pre-treatment periods.

Assumption 3 *(Treatment Effect Homogeneity)*:
For each k, $\text{ATT}(g, g + k)$ need not depend on $g \in \mathcal{G}$. Units need to share the same treatment effect path regardless of when they are treated. Say unit i is treated in year g and unit j is treated in year $g' \neq g$. Then, despite receiving treatment at different times, these two units need to experience the *same* treatment effect k years post-treatment. For the specific example described above we would need that $\text{ATT}(g, g + k) = \text{ATT}(g', g' + k)$.

We have two different treatments for two populations, our full sample and "working women", so it would be best practice to go through each of these assumptions for each of the treatments and populations. Our arguments for these assumptions will extend for both treatments and populations, so we will not specifically reference a population or treatment except where necessary.

Assumption 1 *(Parallel trends in the outcome variable)*:
Under the assumption that treatment timing is plausibly exogenous during our study period, it is reasonable to assume that states that passed unilateral would have exhibited parallel trends in housework absent treatment if within that period states are not selecting into treatment. It is a little less clear that eventually-treated and never-treated states would have followed parallel trends. Figure 9 is a table of states and when they passed unilateral divorce compiled by Voena (2015)[19]. As was noted in section 2, the states that did and did not pass unilateral divorce do not exhibit regional differences. This is just one observable. There could potentially be other observables such as heterogeneous changes to access to labor-saving housework technologies that may have impacted housework hours differently for eventually-treated and never-treated states. It will be an object of further research to identify observables such as this that might confound our parallel trends assumption. We will tentatively posit that absent treatment we would observe parallel trends in housework for women in eventually-treated and never-treated states and that we would still see parallel trends if we only considered

"working women".

A potential threat to this identification strategy is if there are unobservable differences between eventually-treated and never-treated states. If a burgeoning feminist spirit or progressive values causes unilateral divorce in eventually-treated states then absent treatment we would *not* expect the parallel trends assumption to hold. Perhaps households in states with a burgeoning feminist sentiment would have faced evolving norms and even without unilateral divorce, women would reduce household hours. This alternative story is difficult to test, but as an avenue of further work, we might be interested in reading the sociological literature describing this period to see what was driving unilateral divorce and in particular, the forces that drove the adoption of unilateral divorce laws in states that adopted them during the 1970s.

Wolfers (2006)[20] provide a figure 11, which we include in our appendix, displaying trends in divorce rates for states that passed unilateral divorce before 1980 and states that did not from 1956 to 1998. In the period from 1956 to 1968, prior to the era of the bulk of unilateral divorce reform in the early 1970s, states that did and did not pass unilateral divorce in the 1970s followed parallel trends in divorce rates. While this figure is measuring divorce rates rather than housework hours worked by the wife, these parallel pre-trends might suggest that individuals in eventually-treated and never-treated states did not have diverging trends in how they were thinking about marriage and the family prior to the period of reform. It is crucial to note that parallel pre-trends do not imply parallel trends, but it is comforting to see that there was not a divergence in trends in divorce rates leading up to the reform period.

An additional complication comes from our second treatment in which we say units are treated only when their state has passed unilateral divorce and either equitable distribution or community property. We acknowledge that this is a crude estimator and in later sections outline future directions for combining an analysis of both unilateral divorce and equitable distribution.

Assumption 2 *(No Anticipatory Behavior Prior to Treatment)*:
As stated earlier, Jacob (1998)[10] and Wolfers (2006)[20] claim that the adoption of unilateral divorce by our treated states in the 1970s is plausibly exogenous so no anticipation exists.

One potential challenge to this assumption is that individuals would expect such legislation given the tide of feminist movements in the 1970s, which lead many other states to pass this policy. In addition, due to the gravity of this unilateral divorce legislation, it might generate noise during the legislative process. However, although individuals may have been aware of this legislation, we argue that women would not

adjust their behavior beforehand, fulfilling our "no anticipation" assumption. Without guarantee that unilateral divorce could actually be exercised, individuals might not be inclined to potentially create a rift in their marriage. Family relations are delicate matters, and individuals tend to be risk averse, especially when children or assets are at play. As such, we assume there is no anticipation given the discussion above, but given more time we would be interested in reading some of the sociological work from the time analyzing how women behaved in response to the introduction of unilateral divorce in neighboring states.

Assumption 3 *(Treatment Effect Homogeneity)*:
This assumption is difficult to defend in our context. In our study window, many states passed unilateral divorce beginning in 1970 through 1975. There was a recession in 1973. Under our assumption that the credibility of divorce is tied to one's outside option and in particular in our study the ability for women to get jobs, it is hard to imagine that we would have treatment effect homogeneity. Women who lived in states that passed unilateral divorce in 1970 faced a very different labor market than those who lived in states that passed unilateral divorce in 1973. For women whose state passed the law in 1973, there were likely fewer available jobs, so they might have been unable to bargain down housework hours in a similar way as those whose state passed the law in 1970. We explore heterogeneity in treatment across group in figure 8 using Callaway and Sant'Anna 2021[1]'s method, which shows that treatment effect homogeneity is unlikely.

If one were to treat dynamic two-way fixed effects as a generalization of difference-in-differences subject to the same assumptions as difference in differences, then one would only be concerned with the first assumption of parallel trends. We will proceed as if this is the only assumption that we need to meet to have consistent estimates of these dynamic treatment effects and we will produce and interpret estimates. We will then take a step back and consider what Sun and Abraham (2021)[17] say we have actually estimated using our dynamic two-way fixed effects regressions given we have not met all three assumptions.

Figure 2 displays our estimates of μ_ℓ from the dynamic two-way fixed effects regression with housework hours worked by the wife as the outcome variable. The orange ribbons are 95% confidence intervals with standard errors clustered at the state level. Each panel displays estimated coefficients from one of our two treatment specifications for either the full sample or "working women". Panel 2a shows estimates where we

Figure 2: Dynamic Two-Way Fixed Effects Estimates

(a) Unilateral Divorce

(b) Equitable Distribution
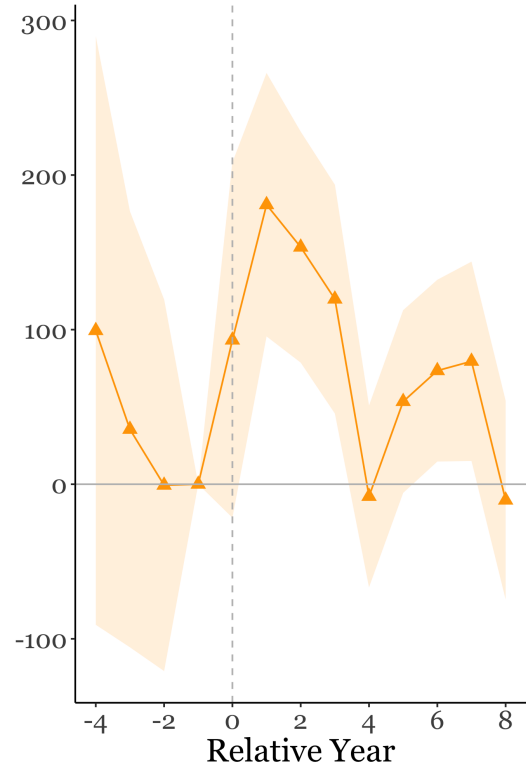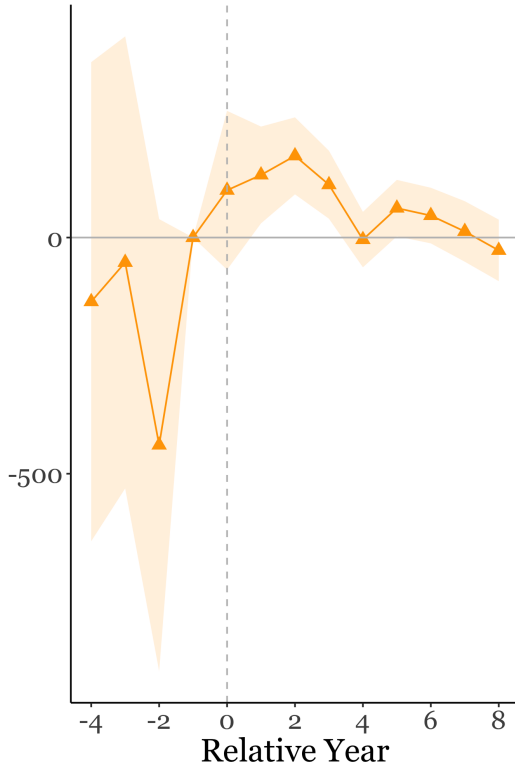
(c) Unilateral Divorce; Working Women

(d) Equitable Distribution; Working Women
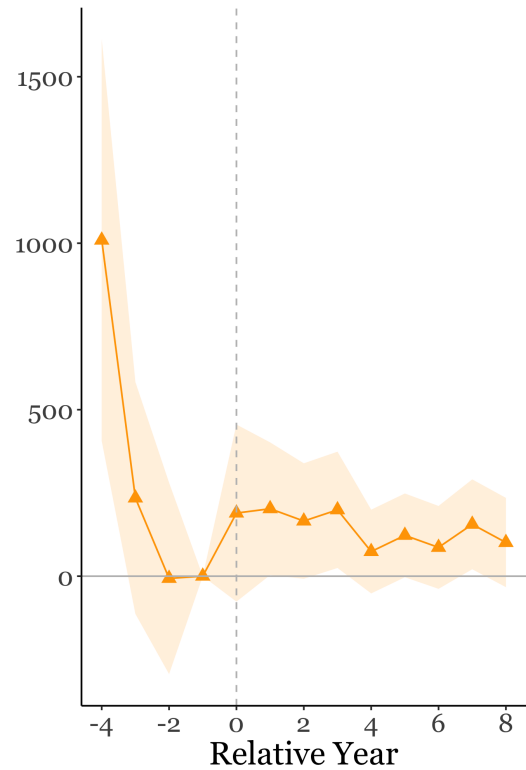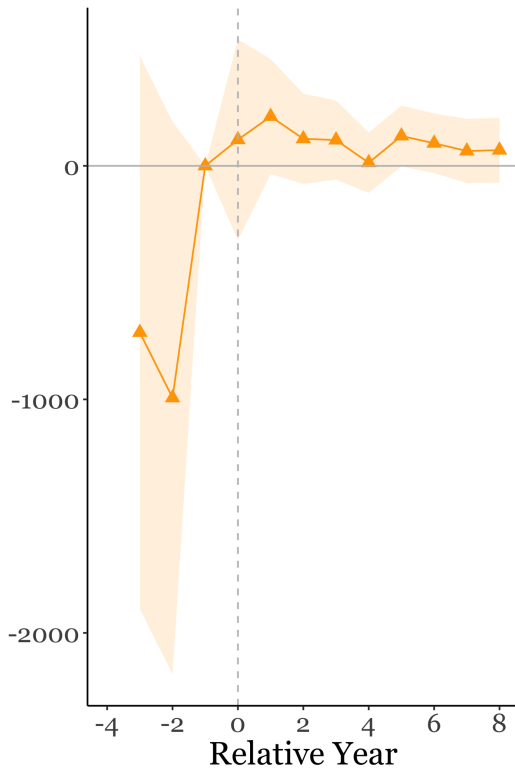
say units have become treated when their state passes unilateral divorce. Panel 2b provides estimates where we define units becoming treated when their state has both unilateral divorce and either equitable distribution or community property. Panel 2c includes estimates where we subset the population to "working women" and say unit i has become treated when their state passes unilateral divorce. It is important to note that these estimates are not valid because after subsetting the data to only include "working women" the matrix is extremely close to singular. Panel 2d displays estimates where we includes estimates where we subset the population to "working women" and say unit i have become treated when their state has passed unilateral divorce and has either equitable distribution or community property.

We will interpret our results as if we only needed the parallel trends assumption in the same way that others typically interpret the results of dynamic two-way fixed effects regressions. We will begin by examining the μ_ℓ $s.t.$ $\ell < 0$. These coefficients are typically used as a falsification test for the parallel trends assumptions. Looking at figure 2, we see that for both treatments and both the full sample and "working women" we have large imprecisely-estimated pre-trends. This would cause us to be skeptical of the parallel trends assumption because "treatment" and "control" groups are not following parallel trends in the pre-period.

We will now move to our estimates of the μ_ℓ $s.t.$ $\ell \geq 0$. In figure 2a and figure 2b it appears as if we have a positive and periodic effect. This is a bit concerning because it doesn't seem like it can be easily explained by bargaining dynamics. Perhaps this is suggestive that our estimates are confounded by another trend that is differentially effecting "treated" units. In figure 2c and figure 2d we see positive, but not statistically significant effects of the policy.

The effects we estimated are contrary to what we would have expected given our proposed mechanism. Parallel trends and no anticipation would have led us to believe that we would not observe any effect in any of the pre-periods. Our belief that unilateral divorce allows women to bargain for less housework caused us to imagine that we would expect a negative effect on housework hours. We would further expect following Voena (2015)[19] that equitable distribution and unilateral divorce would lead to even more negative effects. We would also have anticipated that because "working women" have a more credible threat of divorce that they would have an even greater negative effect than the full sample.

Note that our discussion effects above was contingent on a belief of parallel trends in the outcome variable and that we only need to match the parallel trends assumption. If we move to the Sun and Abraham (2021)[17] paradigm where this single assumption

is not enough we might wonder what we have estimated if we did not meet all three of their necessary assumptions.

Sun and Abraham (2021)[17] find that these μ_ℓ are a weighted average of $\text{ATT}(g,t)$'s where $w^\ell_{g^*,t^*}$ is the weight on $\text{ATT}(g^*,t^*)$ being averaged in coefficient μ^ℓ as in the equation below,

$$\mu_\ell = \sum_{g \in \mathcal{G}} \sum_t w^\ell_{g,t}\text{ATT}(g,t)$$

Unless all three assumptions are met, then μ_ℓ may be *contaminated* by $\text{ATT}(g,t)$'s from *different* relative periods. For example, consider the lead two periods before the treatment date, μ_{-2}. If this is a weighted average of leads and lags then it is uninformative. This could be extremely problematic if we use these μ_ℓ as a falsification test for parallel trends or as an effect. Analyzing them could result in false positive or negative results depending on the weights, so we cannot even determine if we are falsely accepting or rejecting possibly significant results.

Fortunately, Sun and Abraham (2021)[17] have produced a package that retrieves the weights on each of the $\text{ATT}(g,t)$'s that make up each μ_ℓ. We can use these results to get a sense of if our coefficients are contaminated by the contributions of $\text{ATT}(g,t)$'s from different relative time periods.

In figure 3, we produce plots for leads -4 and -2 and lags 1 and 5. On the x-axis, we have a list of all relative years. On the y-axis, we sum the weights on the $\text{ATT}(g,t)$'s that come from each of the relative periods. Let us call these values y^ℓ_k and define them according to the equation below,
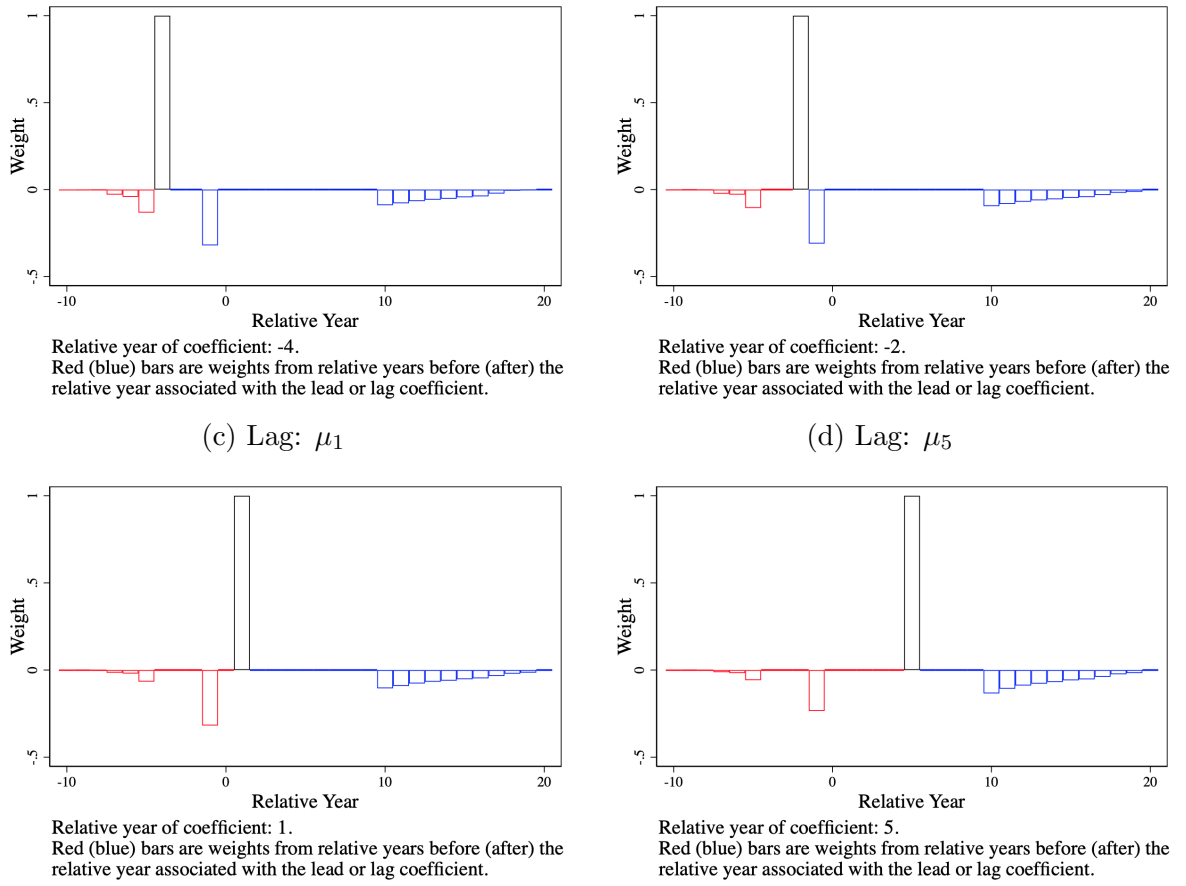
$$y^\ell_k = \sum_{g \in \mathcal{G}} w^\ell_{g,g+k}$$

This will give us a sense of how our leads and lags are contaminated with leads and lags from different periods.

In figure 3a and figure 3b we see that the leads incorporate negatively weighted treatment effects from outside of their relative year. This is problematic if we want use these leads as a falsification test for parallel trends because even if the the true treatment effect coefficient for that lead was zero, if there was a treatment effect in a much later period, we would reject parallel trends.

We also see that lags in figure 3c and figure 3d incorporate negatively-weighted leads and negatively-weighted lags. This is an issue because we are not estimating an effect local to that relative year. Our estimate is confounded by effects from other relative periods. Worse yet, these are negative weights, so this estimated coefficient

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Figure 3: Weights associated with coefficients μ_ℓ

(a) Lead: μ_{-4}

Relative year of coefficient: -4.
Red (blue) bars are weights from relative years before (after) the
relative year associated with the lead or lag coefficient.

(b) Lead: μ_{-2}

Relative year of coefficient: -2.
Red (blue) bars are weights from relative years before (after) the
relative year associated with the lead or lag coefficient.

(c) Lag: μ_1

Relative year of coefficient: 1.
Red (blue) bars are weights from relative years before (after) the
relative year associated with the lead or lag coefficient.

(d) Lag: μ_5

Relative year of coefficient: 5.
Red (blue) bars are weights from relative years before (after) the
relative year associated with the lead or lag coefficient.

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cannot be interpreted because it is some combination of positive effects from its period and negative effects from other periods.

Dynamic two-way fixed effects does not seem to be the right tool for the job given that we cannot meet the necessary assumptions and because our estimated coefficients do not have easy interpretations. Fortunately, there are a few other tools to produce estimates of a time-varying treatment effect given staggered adoption. We proceed in the next section using a tool developed by Callaway and Sant'Anna (2021)[1].

5 Callaway Sant'Anna Solution

The method proposed in Callaway and Sant'Anna (2021)[1] allows us to estimate the impact of unilateral divorce laws on household bargaining under a very flexible set of assumptions. Callaway and Sant'Anna (2021)[1] estimate essentially an identical group-time parameter as Sun and Abraham (2021)[17] that allows for flexible aggregation. That enables us to analyze both the impact of unilateral divorce laws in event-study time, as well as observe how differential timing in the law's adoption affected its impact. The benefit of this method over dynamic two-way fixed effects is that researchers can specify the weights of the group-time parameters rather than having to trust that the regression provides a suitable set of weights. In our previous section we show that without strong assumptions, we cannot trust that the regression will do so. Another benefit of this method over others that estimate group-time parameters is that it provides clear instruction as to the assumptions needed and causal parameter estimated if one includes covariates.

5.a Assumptions

Callaway and Sant'Anna (2021)[1] state that there are five assumptions necessary to estimate their group-time treatment parameter using their method:

Assumption 1 *(Treatment is irreversible)*: We fulfill this condition as the two treatments in our study - unilateral divorce or unilateral divorce and either equitable distribution or community property - are not repealed by states that pass them.

Assumption 2 *(Data is i.i.d)*: Since the PSID is a random sample, the dataset we use fulfills this condition. Although we subset our data, we also adjust the group we claim our study can generalized to when we do so, so within our target population

of interest, our subsetted sample is i.i.d.

Assumption 3 *(Limited treatment anticipation)*: Callaway and Sant'Anna 2021[1] do not prohibit treatment anticipation, but if anticipation exists, it must be known and be a small quantity. Our empirical specifications assume that anticipation does not exist, in line with the rest of the literature. The plausability of our no anticipation assumption was discussed earlier in section 4 when we discuss the assumptions needed to use dynamic two-way fixed effects.

Assumption 4 *(Conditional Parallel Trends)*: Fourth, we have conditional common trends based on a never-treated or not-yet treated group. In our particular specification, our controls are never-treated individuals, so we require common trends for a never-treated group. This assumption can be tested empirically by estimating pre-trend coefficients in CSA. The outline of our mechanism justifies why we believe parallel trends holds when we discuss assumptions 4.b. This assumption is also discussed further in section 4, where we describe why we believe parallel trends hold for our full data sample and address potential threats to this assumption.

The covariates we condition on are age, income, and annual hours worked, factors that affect housework hours irrespective of the presence or absence of unilateral divorce laws. These are time-varying covariates[4]. We expect, for example, wives may experience differential trends in home production during this time period depending on their income, but that once accounting for income-based differences, common trends hold.

Another outcome of interest for us is the husband's housework hours. We believe that the husband's housework hours are affected by the same household bargaining dynamics that affect the wife's housework hours. Thus our common trends assumption holds for our other outcome of interest.

Two other specifications we run involve subsetting data - the two splits we use divide up our sample based on whether an individual completed high school and whether they worked full time, to refine our analysis. Assuming our common trends assumptions hold for the whole sample, does not imply parallel trends for a subset of our data. We believe however that our argument for parallel trends in section 4 may also

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[4]One might be worried that our estimates will be confounded by ex-post changes in these covariates. Fortunately, CSA's package modifies covariates so we do not face this issue. They explain that "For time varying covariates, the did package sets the value of the covariate to be equal to the value of the covariate in the "base period" where, in post-treatment periods the base period is the period immediately before observations in a particular group become treated (when there is anticipation, it is before anticipation effects start too), and in pre-treatment periods the base period is the period right before the current period."

be applied to this subset. The benefit of subsetting is that we remove a potential source of variation that could contribute to differential common trends, for example, by specifying that all individuals in our control and treatment groups must have some common characteristic, like a high school diploma and limiting the impact of compositional differences in the educational backgrounds of individuals within treated and control states. Finally, our last specification combines unilateral divorce and equitable distribution laws to define the treatment, which has challenges that are mentioned in section 4.

Assumption 5 *(No Overlap)*: The fifth assumption is primarily a technical requirement - that at the time each of our treatment groups receives the treatment, a positive portion of the population becomes treated. This entails defining our treatment groups correctly.

5.b   Causal Parameter and Aggregation

Similarly to group-time treatment effect defined in Abraham and Sun (2021)[17], CSA's causal parameter is defined as the following

$$ATT(g,t) = \mathbb{E}[Y_t(g) - Y_t(0)|G_g = 1] \quad t = 1, \ldots, \mathcal{T}$$

where G_i is the year period household i first becomes treated and $G_g \equiv \mathbf{1}\{G_i = g\}$ is an indicator for whether individual i belongs to group g. We are interested in aggregating the group-time treatment parameter along both event study and treatment time. When discussing the two types of aggregation, we exclude most technical details. For further reference, one should see section 3 of Callaway and Sant'Anna (2021)[1]. Event study time aggregation of the $ATT(g,t)$'s allows us to evaluate the impact of the treatment over time by calculating a weighted average of the ATT for each group in each year before and after the treatment. This time varying treatment effect is the parameter that dynamic two-way fixed effects attempts to recover. Callaway and Sant'Anna (2021)[1] allows us to recover it with sensibly chosen weights.

The aggregation scheme for event time is for any year e after the treatment, Callaway and Sant'Anna (2021)[1] say we can find the event-time impact of the treatment $\theta_{es}^{bal}(e; e')$ through

$$\theta_{es}^{bal}(e; e') = \sum_{g \in \mathcal{G}} \mathbf{1}\{g + e' \leq \mathcal{T}\} ATT(g, g + e) P(G = g|G + e' \leq \mathcal{T})$$

$$\text{with } 0 \leq e \leq e' \leq \mathcal{T} - 2$$
$$\text{and } \mathcal{G} \equiv \text{supp}(G)$$

For any time e relative to the treatment date, we calculate $\theta_{es}^{bal}(e; e')$ by finding a weighted average of the $ATT(g, g+e)$, using each group $g \in \mathcal{G}$. The e' is the maximum number of years post-treatment we are interested in finding the impact of the treatment for. Including e' is important because we use e' to keep compositional balance constant so we can accurately compare $\theta_{es}^{bal}(e; e')$ to $\theta_{es}^{bal}(e^*; e')$ for any other e^*. This is important as due to the staggered nature of treatment adoption, individuals in each group are present for a different number of years after.

Aggregating group-time treatment effects to identify the overall treatment effect for each group is more straightforward. For any group $\tilde{g}$ that is treated for $\mathcal{T} - g + 1$ periods starting at time $t = \tilde{g}$ until time $\mathcal{T}$, we can find the overall treatment effect for $\tilde{g}$ as

$$\theta_{sel}(\tilde{g}) = \frac{1}{\mathcal{T} - g + 1} \sum_{t=\tilde{g}}^{\mathcal{T}} ATT(\tilde{g}, t)$$

an average of each group-time treatment effect for group $\tilde{g}$ in each of the $\mathcal{T} - g + 1$ time periods.

5.c Estimation Strategy

Next, we identify the impact of unilateral divorce on housework hours. The first specification we estimate consists of our full sample, using our standard unilateral divorce treatment and covariates. We do not allow for any anticipation and we cluster our standard errors at the individual level. Intuitively, we want to cluster at the state level because policy variation occurs at the state level, but Callaway and Sant'Anna (2021)[1] say that one should only cluster when they have a large number of groups, and for several of our pre-treatment and post-treatment years, we do not have a large number of groups. Our standard errors for all results are derived using their bootstraping procedure. Although we use never-treated individuals as our control group in the report estimates, our estimates and the associated confidence intervals do not change much when we use not-yet-treated individuals. Note that we use our full dataset to estimate the results, instead of only keeping individuals present for all 22 years from 1968 to 1989 due to data constraints.

We see in panel 4a of figure 4 that unilateral divorce has a persistently negative effect on the wives' housework hours that accumulates over time but plateaus out after

a period of around 5 years. Our pre-trends are statistically insignificant from zero, validating our parallel pre-trends assumption. Our estimates are that throughout the first 9 years after the treatment, annual housework hours were reduced by an average of 112 hours annually, the equivalent of .307 less hours of daily housework. These results are very similar to those in Roff (2017)[15], although our estimates are statistically significant while being significantly lower than those estimated in Genadek (2014)[3] (between 1.45 and 1.63 less hours daily). We also see that the treatment effect evolves over time - unilateral divorce has the largest impact on housework hours 7-9 years after the policy, although the magnitude of the estimates plateau after years 4-6 in event study time. Further, while not statistically significant, we see negative estimates of leads in the three years leading up to unilateral divorce passage. Possibly this might challenge our no anticipation assumption. Further analysis of this phenomenon is an area of future work given that the prior literature has also assumed no anticipation.

In panel 4b of figure 4, we define our outcome to be the husbands' housework hours and observe that there is a consistently positive trend over time, although the estimates are statistically insignificant. Our pre-trends are also statistically insignificant from zero, validating our earlier motivation for common trends. These figures together might imply that husbands are picking up a larger share of housework hours.

Figure 4: Unilateral Divorce

(a) Wife   (b) Husband
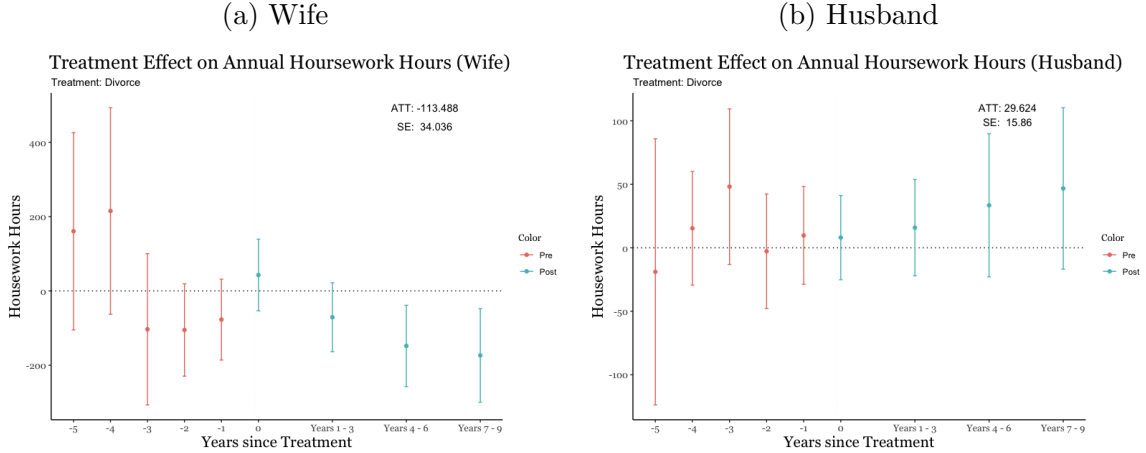
Next, we consider how variation in the credibility of divorce threats influence household bargaining dynamics. We first split our population by whether the wife had at least high school education or not. Our intuition is that more highly educated women are better positioned to use unilateral divorce laws to improve their household bargaining position. For example, wives more education have higher potential financial

earnings, so they would likely be less reliant on the family for financial support.

We find in figure 5 that for the subset of the population that had a high school education, the introduction of unilateral divorce laws substantially decrease housework hours, by around 138.8 hours annually, or .38 less hours of housework daily, and the effect increases linearly over time. This effect is much larger than what we observe in figure 4, validating our intuition. We also found statistically insignificant pre-trends in all but one pre-periods, validating our common trends assumption.

On the other hand, unilateral divorce had a statistically insignificant impact on housework hours for the individuals without a high school diploma. Thus, we observe that the majority of the individuals affected by unilateral divorce are educated.

Figure 5: Unilateral Divorce Split by Education

(a) High school diploma   (b) No high school diploma
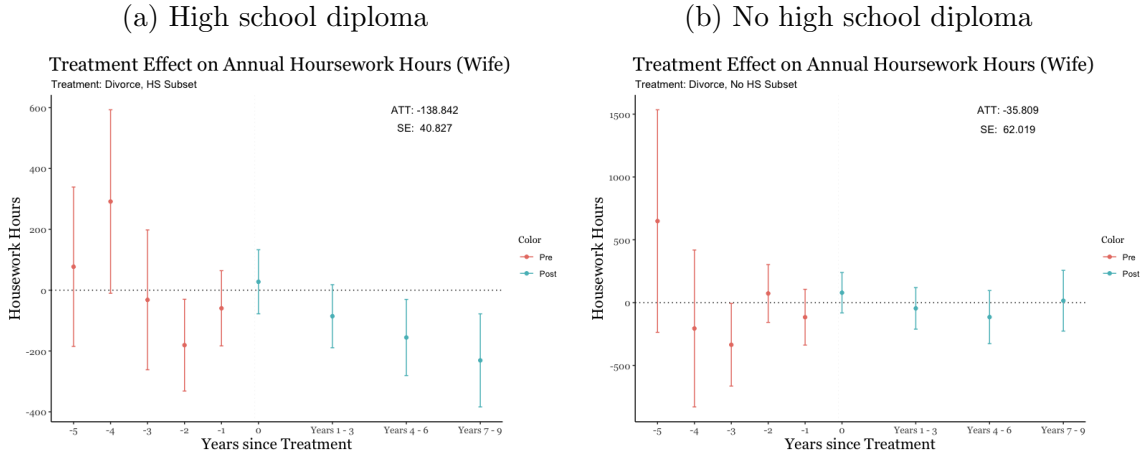
The second subset we considered was an individual's employment history, as we used in the dynamic two-way fixed effects specification in section 4. Our intuition was that working women would have more financial independence and be able to use that as greater leverage in household bargaining dynamics. We find in figure 6 that in fact, our results are the opposite of what we expect - for the subset of the population that worked, the policy actually had a statistically insignificant impact on housework hours while for the subset of the population that was not employed full-time, housework hours decrease substantially following the policy.

A potential explanation for this surprising result is that women who work full-time only have time to do highly-essential housework. Since such housework cannot be reduced, perhaps this is why we see a null effect.

Our next strategy was influenced by Voena (2015)[19] among other unilateral divorce papers, who found that property distribution was a key factor in determining

Figure 6: Unilateral Divorce Split by Work Experience
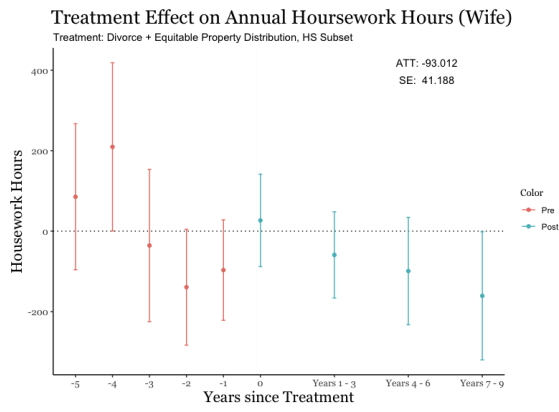
(a) Working

(b) Not working
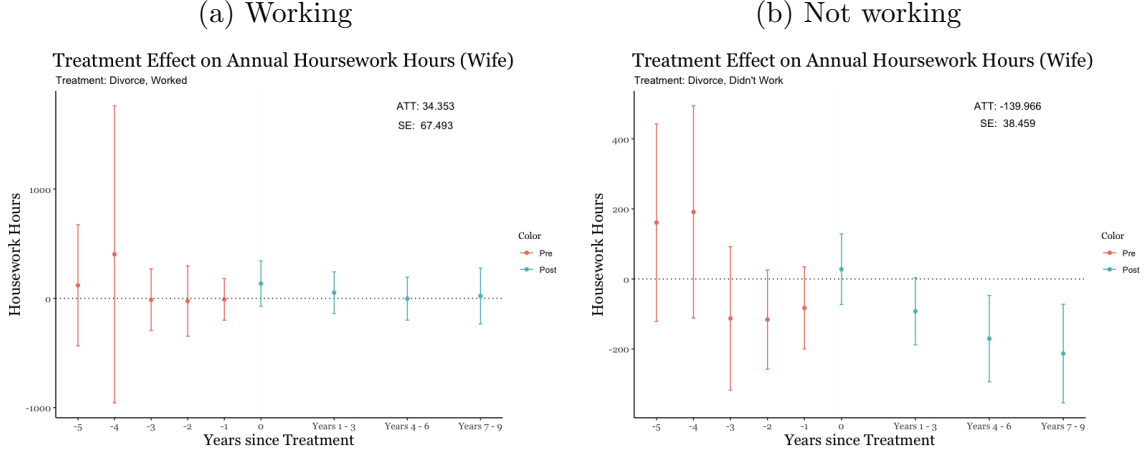
household bargaining dynamics. We say units are treated when their state has passed both unilateral divorce and either equitable distribution or community property. We acknowledge that this is a very preliminary first step in terms of combining our analysis of unilateral divorce with property distribution and outline future steps below.

In figure 7, we see that the combined effect of unilateral divorce and equitable property distribution reforms actually reduced the effect of our treatment. We also include run 3 other specifications, where we subset on just high school educated workers, and examine the treatment impact on working and non working women. Surprisingly, we find that the combined effect of unilateral divorce and equitable distribution laws did not further decrease home production hours - in fact, home production decreased less under our new treatment, compared to their equivalents in our earlier analysis. Moreover, once again, we find that women without a work history experienced a far stronger decline in housework hours than working women, who the policy and a statistically insignificant impact on. We do not have statistically significant leads, but the fact that in three of the four sub-populations there are negative leads in the three years leading up to treatment again emphasizes the need to examine the no anticipation assumption.
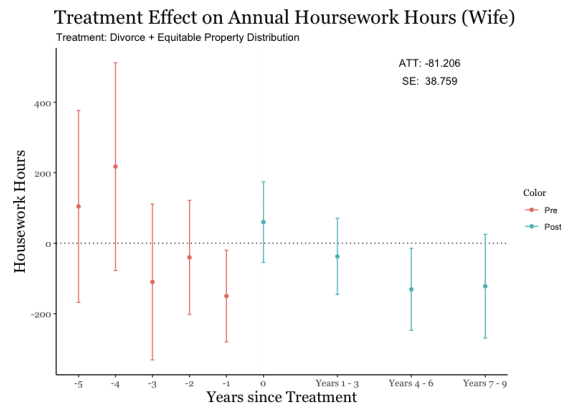
The results we see are striking, as we expected that a stronger policy shock would induce a larger decrease in home production hours by the wife. However, there are several factors that may be affecting our analysis that warrant further exploration in the future. First, each treatment is "different" in that in most states, unilateral divorce and equitable distribution are passed in different years. In some cases, the laws are passed in quick succession; in others, one law is passed before the other. Thus, any

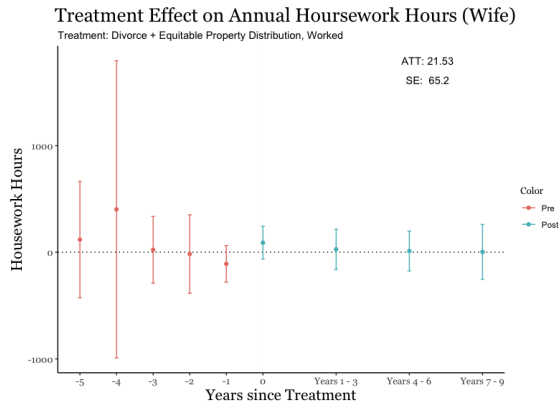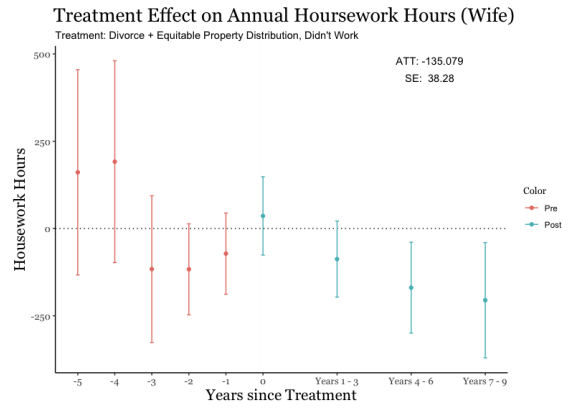Figure 7: Unilateral Divorce + Equitable Distribution

(a) High school diploma

Treatment Effect on Annual Hoursework Hours (Wife)
Treatment: Divorce + Equitable Property Distribution, HS Subset

ATT: -93.012
SE: 41.188

(b) Full sample

Treatment Effect on Annual Hoursework Hours (Wife)
Treatment: Divorce + Equitable Property Distribution

ATT: -81.206
SE: 38.759

(c) Working

Treatment Effect on Annual Hoursework Hours (Wife)
Treatment: Divorce + Equitable Property Distribution, Worked

ATT: 21.53
SE: 65.2

(d) Not working

Treatment Effect on Annual Hoursework Hours (Wife)
Treatment: Divorce + Equitable Property Distribution, Didn't Work

ATT: -135.079
SE: 38.28

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future analysis should accommodate for these timing differences. A first step would be to analyze the effect of equitable distribution on its own, independent of unilateral divorce.

Another issue with this current analysis is that our control group actually consists of three subgroups - individuals who live in states with only one of unilateral divorce and equitable distribution, and individuals who live in states with neither. A more robust estimate that allows us to provide a more interpretable treatment effect would separately analyze our treatment using each of these three subgroups as a control.

Separately, we should also consider factor to consider is that in certain states, unilateral divorce was passed after equitable distribution laws. This could be a factor that is driving the negative point estimates of some of our pretends, notably in figure 4 and figure 6. The passage of equitable distribution laws after unilateral divorce could also be affecting our estimates of the impact of unilateral divorce on home production. To start, we would likely want to examine papers from the difference in differences literature on multiple treatments, such as Chaisemartin and D'Haultfœuille (2022)[2].

Finally, figure 8 demonstrates the variation in treatment effects, aggregated over a 9 year post period, depending on the treatment year. We observe that with the exception of 1978 and 1970, the treatment effects are persistently negative and the point estimates seem similar, suggesting that a large majority of unilateral divorce policies had negative effects of varying sizes on the population irrespective of the year passed. This is contrary to our expectation that there would be a more limited effect between 1973 and 1975 due to the paucity of available jobs and hence worse outside-option.
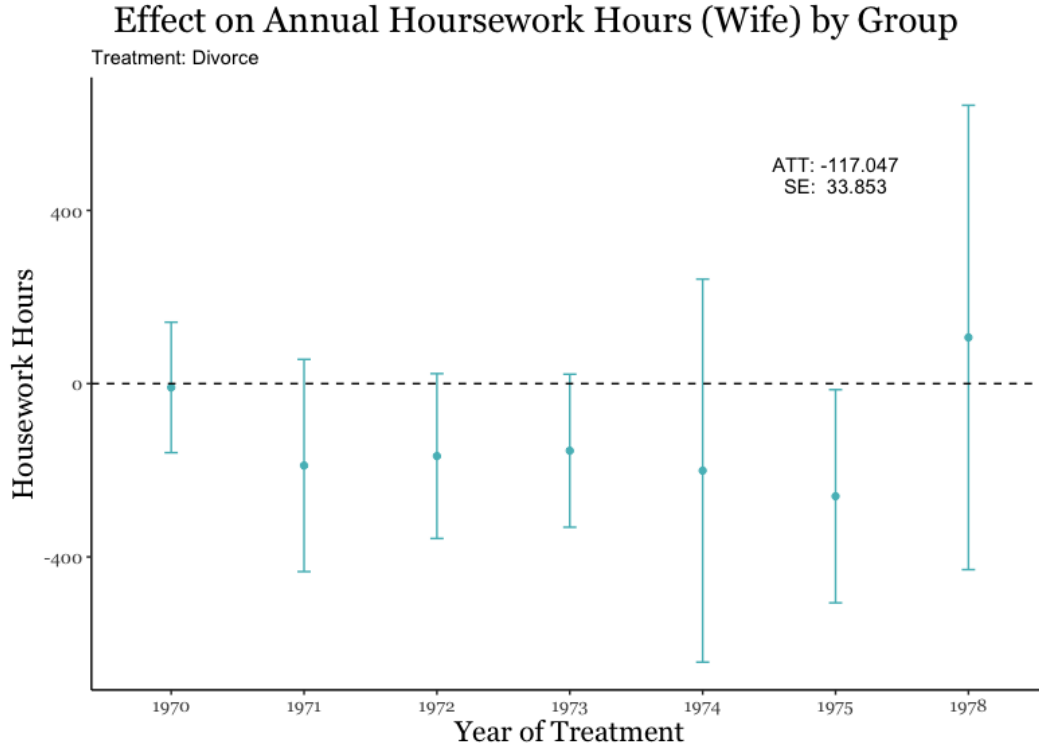
Effect on Annual Hoursework Hours (Wife) by Group

Treatment: Divorce

ATT: -117.047
SE: 33.853

Figure 8: Aggregate Treatment Effects by Group

6   Discussion

Our analysis found that unilateral divorce decreased housework hours performed by women, in line with the rest of the literature. Furthermore, we found that this effect was more pronounced for women with a high school education and women who were not working full-time. In the future, we'd like to improve our definition of working women. When estimating the combined impact of unilateral divorce and post-divorce property distribution laws, we surprisingly found a weaker effect of the policy. However, we acknowledge that this last analysis can be improved and outline in the relevant section suggested improvements.

Overall, we'd also like to research the historical circumstances regarding the passage of unilateral divorce laws further in order to better understand our assumptions of common trends and anticipation. In our analysis we did not include housework share as an outcome because of missing data with the household housework hours data. However, this would also be an area we would be interested in exploring in the future, because it provides a more relative measure of how household bargaining dynamics are shifting.

7 Appendix

State	Unilateral divorce	Equitable distribution	State	Unilateral divorce	Equitable distribution
Alabama	1971	1984	Montana	1973	1976
Alaska	pre-1967	pre-1967	Nebraska	1972	1972
Arizona	1973	community property	Nevada	1967	community property
Arkansas	no	1977	New Hampshire	1971	1977
California	1970	community property	New Jersey	no	1974
Colorado	1972	1972	New Mexico	pre-1967	community property
Connecticut	1973	1973	New York	no	1980
Delaware	1968	pre-1967	North Carolina	no	1981
District of Columbia	no	1977	North Dakota	1971	pre-1967
Florida	1971	1980	Ohio	1992	1981
Georgia	1973	1984	Oklahoma	pre-1967	1975
Hawaii	1972	pre-1967	Oregon	1971	1971
Idaho	1971	community property	Pennsylvania	no	1980
Illinois	no	1977	Rhode Island	1975	1981
Indiana	1973	pre-1967	South Carolina	no	1985
Iowa	1970	pre-1967	South Dakota	1985	pre-1967
Kansas	1969	pre-1967	Tennessee	no	pre-1967
Kentucky	1972	1976	Texas	1970	community property
Louisiana	no	community property	Utah	1987	pre-1967
Maine	1973	1972	Vermont	no	pre-1967
Maryland	no	1978	Virginia	no	1982
Massachusetts	1975	1974	Washington	1973	community property
Michigan	1972	pre-1967	West Virginia	1984	1985
Minnesota	1974	pre-1967	Wisconsin	1978	community property (1986)
Mississippi	no	1989	Wyoming	1977	pre-1967
Missouri	no	1977			

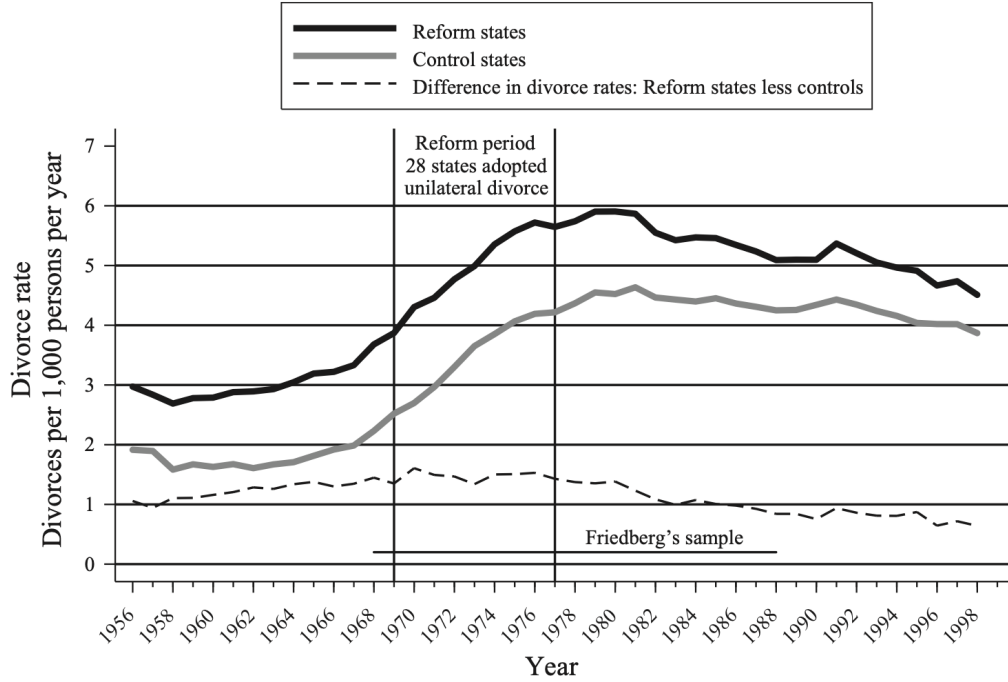Figure 9: Divorce Law Reforms

(a) From appendix of Voena (2015)[19]

Figure 11: Average Divorce Rate: Reform States and Controls

(a) Figure 1 Wolfers (2006)

[20]

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