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# Evidence from Extended School Days

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# ABSTRACT

# Parents' Effective Time Endowment and Divorce:
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Policies that extend the school day in elementary school provide an implicit childcare subsidy for families. As such, they can affect parents' time allocation and family dynamics. This paper examines how extending the school day affects families by focusing on marriage dissolution. We exploit the staggered adoption of a policy that extended the availability of full-time elementary schools across different municipalities in Mexico. Using administrative data on divorces, we find that the extension in the school day by 3.5 hours leads to a significant increase in divorce rates. Moreover, the effect grows with every year of municipalities' exposure to full-time schooling. Increased female employment due to the availability of childcare is likely to be one of the mechanisms that relaxed restrictions to marriage dissolution.

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# 1 Introduction

Childcare availability affects families' time allocation and family members' opportunities to conduct activities outside their households. Limited access to childcare coupled with gender norms limiting female labor force participation restricts women's opportunity to allocate their time to market activities. Because the opportunity to earn income tends to be correlated with intra-household bargaining power, this situation directly affects families' dynamics. This paper sheds light on the connection between childcare availability and divorce, an important and unexplored issue given that early family environments are strong predictors of short and long-run students' outcomes (Heckman, 2006). Specifically, we examine how extending the school day by 3.5 hours in public elementary schools in Mexico affects divorce. In doing so, we provide the first evidence on the effects of access to full-time schools (FTS hereafter) on marriage dissolution and, to our knowledge, the first causal evidence regarding the impact of the availability of subsidized childcare on divorce.

FTS affect families' decisions due to the implicit childcare subsidy they provide. A dimension in which FTS can affect families is their effect on family structure, specifically through marriage dissolution. Most of the literature on FTS has focused on examining the effect of extensions in the school day on students' learning, finding mixed results.[1] Recent studies have found that the availability of FTS has significant intra-household effects, increasing the labor force participation of mothers and extended family members (Contreras and Sepúlveda, 2017; Padilla-Romo and Cabrera-Hernández, 2019; Cabrera-Hernández and Padilla-Romo, 2020; Kozhaya and Martínez Flores, 2020).[2] However, little is known about how the increase in the availability of FTS affects other dimensions of household dynamics.

Considering a simple two-good model of labor supply decisions in which caregivers choose

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[1]See, for example, Cerdan-Infantes and Vermeersch (2007), Bellei (2009), Arzola González (2010), Pires and Urzua (2010), Dias Mendes (2011), Xerxenevsky (2012), Llambí (2013), Orkin (2013), Almeida et al. (2016), Hincapie (2016), Cabrera-Hernández (2020), Agüero et al. (2021), and Padilla-Romo (2022). For a discussion on the effects for Latin American and Caribbean countries, see Holland et al. (2015).

[2]Other important outcomes that have been studied are teenage pregnancy (Kruger and Berthelon, 2009) and child labor (Kozhaya and Martínez Flores, 2020).

between home time (that includes time spent caring for children) and consumption goods, extensions in the school day provide school-age children's main caregivers with a higher *effective time endowment*, by freeing time that was previously spent providing child care.[3] Given that children's main caregivers tend to be mothers or other female family members, this allows for a higher female labor force participation.[4] As intra-household bargaining models suggest, the position of women in bargaining can increase if the level of utility they can independently achieve increases. In this case, women's outside option can increase either because of a higher female individual income (for women who increase their labor supply) or due to the potential of increasing their income (for those women who do not adjust their labor supply but can do so because of their expanded time endowment).[5]

From a theoretical perspective, the direction of the expected effect of changes in bargaining power and female economic independence that may result from the extension in FTS on marriage stability is inconclusive. In line with the *independence effect hypothesis* (Ross et al., 1975), divorce can increase due to the potential increase in independent sources of income for women.[6] Moreover, female employment may lead to within household conflict or higher intimate partner violence (Eswaran and Malhotra, 2011; Heath, 2014; Krishnan et al.,

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[3]The simplifying assumption is that the consumption goods bundle does not include childcare services due (for example) to lack of availability. Suppose we assume that alternative paid sources of childcare are available. In that case, the school day extension can be interpreted as in line with a model in which the price of childcare services (included in the consumption bundle) declines.

[4]For the particular case of the FTS program implemented in Mexico, there is evidence that this policy significantly increased female employment (Padilla-Romo and Cabrera-Hernández, 2019; Kozhaya and Martínez Flores, 2020; Cabrera-Hernández and Padilla-Romo, 2020). The connection between childcare and female labor force participation (in countries different from Mexico) has been studied by Gelbach (2002), Berlinski and Galiani (2007), Lefebvre and Merrigan (2008), Goux and Maurin (2010), Bauernschuster and Schlotter (2015), Nollenberger and Rodriguez-Planas (2015), Carta and Rizzica (2018), Eckhoff Andresen and Havnes (2019), and Berthelon et al. (2022), among others.

[5]Increases in labor force participation (and improvements in the type of employment) have been identified as positively associated with women's bargaining power (Anderson and Eswaran, 2009; Heath and Jayachandran, 2018). Importantly, increasing women's labor force participation is not a necessary condition for higher bargaining power. For example, Majlesi (2016) shows that higher employment opportunities for women in Mexico lead to increases in women's bargaining power.

[6]In the same direction, Becker (1981) explains that the gains from marriage are reduced, and divorce becomes more attractive when there is an increase in female earnings. An important related dimension that may affect marriage dissolution is the relative earnings of the spouses. In their analysis for the United States, Bertrand et al. (2015) find that marriages in which the husband earns less than the wife are more likely to get a divorce. Using variation in manufacturing plant openings in Mexico, Estefan Dávila (2018) shows that a reduction in the male-female earnings inequality increases divorce rates.

2010; Luke and Munshi, 2011; Guarnieri and Rainer, 2018) which are likely to affect divorce rates.[7] Yet, the increase in labor force participation of women can lead to lower economic stress for the family and reduce marital conflict, or to lower intimate partner violence due to a reduction in exposure to the abusive partner (Chin, 2012).[8] Moreover, if childcare has a deterrence effect on divorce due to the burden it imposes on the parent receiving custody of children (Cherlin, 1977), the implicit reduction in childcare costs originated by this policy can affect divorce rates directly.[9] Given the complexity of the mechanisms in place and the lack of consensus on their expected effect on divorce, empirical evidence is crucial to shed light on the potential effects of access to subsidized childcare on divorce. In this paper, we fill this gap in the literature by studying a policy that provides a large implicit childcare subsidy by extending the school day from 4.5 to 8 hours for students enrolled in public elementary schools in Mexico.

Our empirical approach relies on leveraging plausibly exogenous variation from a large-scale expansion in FTS across Mexican municipalities from 2007 to 2016. To identify the dynamic effects of the extension in the school day on divorce rates over time, we combine administrative records from all divorces in Mexico with annual school-level census data on enrollment and participation in the FTS program. Our estimates are obtained using the estimation method for intertemporal treatment effects proposed by de Chaisemartin and D'Haultfœuille (2020) which produces estimates that are robust to heterogeneous and dynamic treatment effects across municipalities and years. We find that, as a result of an average increase in FTS availability of 24 percentage points, the divorce rate increased by 0.046 divorces per 1,000 individuals (a 15% increment relative to divorces in treated municipalities in the year prior to the opening of the first FTS). The effect rises over time, reaching an increase of 0.105 in the divorce rate after the municipalities provided extended

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[7]Domestic violence can also be affected by the gender wage gap. For the United States, Aizer (2010) finds that declines in the gender wage gap lead to the reduction of violence against women.

[8]See Heath and Jayachandran (2018) for an excellent discussion of the literature on female labor force participation in developing countries, its causes, and consequences.

[9]Cherlin (1977) describes this effect for families with children in preschool in the United States.

school days for seven years (when the expansion in FTS reached 32 percentage points).

To assess the validity of the estimated effects on divorce rates, we estimate placebo effects by comparing the evolution of divorce rates in municipalities not exposed to the FTS program and municipalities implementing this program before the roll-out took place. We find that the outcome variables for these two groups of municipalities evolved in a parallel way prior to the program's implementation, which supports our identification strategy. We also show that our results are robust to using the interaction-weighted estimator developed by Sun and Abraham (2021) and the imputation estimator proposed by Borusyak et al. (2021). Finally, we show that our estimates are robust to controlling for the state-level implementation of unilateral divorce laws, which allow for non-fault unilateral divorce and have been shown to increase divorce rates in Mexico (Aguirre, 2019; Hoehn-Velasco and Penglase, 2021).

The extension in the availability of childcare does not affect divorce homogeneously across areas. The increased number of hours elementary school students spend in school leads to a more pronounced rise in divorce rates in areas with higher initial divorce rates and higher baseline female employment. These results suggest that women's increased bargaining power is likely to result in marriage dissolution in areas where social norms were initially more favorable to divorce and women's economic independence. Consistent with the short-term labor supply responses to FTS in Mexico documented in Padilla-Romo and Cabrera-Hernández (2019), we find that childcare availability through extended school days leads to a rise in female employment in the longer run. This finding indicates that the higher effective time endowment women get due to the introduction of full-time schooling is used in the labor market, resulting in higher female economic independence.

We contribute to several strands of the literature. We add to the literature on female economic independence and divorce, which includes studies examining the connection between female non-labor income, labor income, and potential earnings and marriage stability (Ross et al., 1975; Becker et al., 1977; Hoffman and Duncan, 1995; Weiss and Willis, 1997; Jalovaara, 2003; Bobonis, 2011; Hankins and Hoekstra, 2011; Bergolo and Galván, 2018; Berniell et al.,

2020). We also contribute to the literature on the intra-household effects of (public) full-time schools (Contreras and Sepúlveda, 2017; Padilla-Romo and Cabrera-Hernández, 2019; Kozhaya and Martínez Flores, 2020; Cabrera-Hernández and Padilla-Romo, 2020; Berthelon et al., 2022) and, more generally, to the literature on the connection between public expenditure and family outcomes (Dickert-Conlin, 1999; Halla et al., 2016; Dahl et al., 2016; Bastian, 2017).

The paper proceeds as follows. Section 2 describes the institutional features related to divorce in Mexico and the FTS program we study. Section 3 presents the data. Section 4 discusses the identification strategy. Section 5 presents the estimated results. Section 6 examines the robustness of our estimates and Section 7 concludes.

## 2 Background

The FTS program expansion that we analyze extended the school day for students enrolled in public Mexican elementary schools from 4.5 to 8 hours. This federally funded program was launched starting in the 2007-2008 academic year, and it was aimed at improving education quality by expanding learning opportunities through an extended school schedule. Each state assigned schools to this program following broad guidelines established by the Secretariat of Public Education. These guidelines include the requirement that the schools operate only in one shift and provide services for vulnerable students.[10] The FTS program represents one of the largest investments in education in the past decades in Mexico. In Figure 1, we show the timing of implementation, considering the first year in which the first school in each municipality was incorporated in the FTS program. In 2007, the program only covered 500 schools. Over time, the coverage increased, reaching more than 25% of public elementary schools in more than 80% of the municipalities in Mexico.

The extension in the length of school days may affect marriage stability by impacting

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[10]See Padilla-Romo and Cabrera-Hernández (2019), Cabrera-Hernández (2020), Cabrera-Hernández and Padilla-Romo (2020), and Padilla-Romo (2022) for a detailed description of the program.

female employment. How female labor market decisions and women's independence more generally evolve depends on the gender norms prevailing in society. Social norms affect female labor market participation because of their influence in the intra-household distribution of time spent in childcare (Jayachandran, 2021). Like many developing countries, Mexico exhibits significant gender differences in the prevalence of unpaid work. By 2019, women spent 67% of their time carrying out unpaid work while men allocated 28% of theirs. Looking specifically at gender differences in the time allocated to domestic work, women spent (on average) 30.8 hours per week, and men spent 11.6 hours in the same activities. When focusing on the care of children, women spent 24.1 hours per week and men spent 11.5 per week taking care of children aged 14 years or younger.[11] The absence of affordable and high-quality formal childcare options can exacerbate this issue since it often results in relying on care by mothers and other female family members (Padilla-Romo and Cabrera-Hernández, 2019; Cabrera-Hernández and Padilla-Romo, 2020; Talamas, 2020).

Gender norms in terms of care are also reflected in individuals' views about female labor force participation. According to the World Values Survey 2017-2020 (Haerpfer et al., 2020), 31.5% (21.4%) of individuals in Mexico agree (strongly agree) with the statement "*When a mother works for pay, the children suffer.*" To put this information in context, the level of agreement (strong agreement) in the U.S., Canada, and the U.K. is 17.4%, 16.6%, and 19.9% (3.3%, 4.4%, and 3.4%) respectively. In terms of intra-household dynamics, 28.8% (24.2%) of individuals in Mexico agree (strongly agree) with the statement indicating "*If a woman earns more money than her husband, it's almost certain to cause problems.*" In the case of the United States, only 10% (0%) of agreement (strong agreement) with the same statement is reported. To examine how prevalent social norms interact with the impact on divorce, in Section 5.3, we separately examine how access to FTS affected divorces in areas with different degrees of baseline female employment.

Before the implementation of the FTS program, elementary school days of 4.5 hours

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[11]Data from the 2019 Mexican Time Use Survey.

imposed additional constraints to the possibility that primary caregivers participate in the labor market. While in Mexico, women's labor force participation remains relatively low, it has increased as a response to childcare subsidies (Padilla-Romo and Cabrera-Hernández, 2019; Calderon, 2014).[12] Given the relevance of the within household income distribution as a potential source of conflict, in Section 5.3, we analyze how the reform affected female employment in the long run.

Considering divorce regulation, Mexico has legalized divorce relatively earlier than other Latin American countries, allowing for mutual consent divorce and divorce with a cause in 1917 (Brazil, Argentina, and Chile legalized divorce in 1977, 1987, and 2004, respectively). Over time, the country implemented reforms that extended the possible causes for divorce and allowed for non-fault unilateral divorce.[13] In Section 6, we show that our estimates are robust to control for the state-level implementation of unilateral divorce laws.

# 3  Data

Data on divorce filings come from the National Institute of Statistics and Geography (IN-EGI). These administrative data contain individual divorce records and cover all divorces in Mexico. We calculate divorce rates per 1,000 people using municipality-level population projections from the National Population Council (CONAPO) for each academic year (August to July) and municipality.

To measure access to FTS, we rely on school-level census data on enrollment and participation in the FTS program from the Secretariat of Public Education. For each academic year, we match the availability of FTS in each municipality with information on the municipality of residence of the divorcing wife to capture exposure to the extension in the school day.[14] To examine long-run effects on female economic independence, we combine adminis-

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[12]By 2016, only 43.92% of women older than 15 were economically active in Mexico (ILOSTAT).

[13]For the case of Mexico, Aguirre (2019) and Hoehn-Velasco and Penglase (2021) describe the legal changes over time and analyze the effect of unilateral divorce laws on divorce.

[14] Information on the municipality in which marriages took place is also available. Given that the average

trative records on enrollment and school participation in the FTS program with census data on female employment from IPUMS-International for years 2000, 2010, and 2015 for women aged 15-65.

We restrict our sample to municipalities that have existed and maintained their geographic boundaries from 2000 to 2016. We also drop from the analysis municipalities that experienced the first FTS expansion in the 2007/2008 academic year because many of the 500 schools that adopted the program were already full-time before 2007.[15] Table 1 shows descriptive statistics for municipalities that by 2016 had implemented the FTS program (*Ever Treated*) and municipalities that had not implemented the program (*Never Treated*). Ever-treated municipalities had higher divorce rates than never-treated municipalities prior to the roll-out of the FTS program. On average 58.3% of the municipalities that implemented the FTS program by 2016 had pre-intervention divorce rates above the cross-municipalities median. The percentage of women employed in 2000 is not significantly different between ever-treated and never-treated municipalities. Still, the percentage of municipalities with high female employment in 2000 (above the cross-municipalities median) is larger among the ever-treated. Ever-treated municipalities had (on average) more population than never treated municipalities. Moreover, they are more likely to be located in a state that allows for unilateral divorce by 2016.

# 4    Identification Strategy

To identify the effects of FTS on divorce, we exploit municipality-level variation in the availability of elementary FTS. We estimate the following fixed effects model:

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length of marriages in our sample is 13 years (with a median of 11 years), the municipality of residence is likely to provide a more accurate measure of exposure to the program than the municipality of marriage. We provide evidence on the absence of endogenous migratory responses to the program in the population of individuals filing for divorce in Section 6.

[15]After imposing these restrictions, our analytical sample includes 2,178 out of the 2,456 existing municipalities in 2016.

$$DR_{mt} = \alpha_m + \gamma_t + \sum_{k=0}^{7} \delta_k FTS_{m,t-k} + \lambda_m t + u_{mt} \qquad (1)$$

where $DR_{mt}$ is the divorce rate in municipality $m$ in academic year $t$, $\alpha_m$ are municipality fixed effects, $\gamma_t$ are academic year fixed effects, and $u_{mt}$ is an error term that we allow to be correlated within municipalities. Our variable of interest, $FTS_{m,t-k}$ is an indicator equal to one $k$ years after the municipality opened its first FTS, and zero otherwise. To allow for the possibility of differential divorce trends across municipalities, we include municipality-specific linear time-trends as controls ($\lambda_m t$). The coefficients $\delta_k$ identify the average effect of $k+1$ years of implementation of the FTS program on municipalities' divorce rates.

Our empirical approach exploits the staggered adoption of the FTS program across municipalities. Estimation of Equation 1 using two-way-fixed-effects (TWFE) under ordinary least squares (OLS) including leads and lags of the implementation of the FTS policy produces estimates that are not robust to heterogeneous and dynamic effects over time and across groups (Goodman-Bacon, 2021; Sun, 2021; de Chaisemartin and D'Haultfœuille, 2020). To overcome this concern, we estimate the regression in Equation 1 following the methodology developed by de Chaisemartin and D'Haultfœuille (2020). This method produces estimates that are robust to heterogeneous and dynamic treatment effects across municipalities and years under standard common-trends assumptions. The estimates are computed by comparing the $t-k-1$ to $t$ evolution of divorce rates in municipalities implementing the FTS program for the first time in $t-k$ and municipalities that have not implemented the program yet in period $t$.[16]

Before presenting our main results, and to put our main estimates in context, we examine how the availability of FTS in a municipality evolves over time once the municipality starts implementing the program. To do so, we follow Padilla-Romo and Cabrera-Hernández (2019) and define our outcome variable as the predicted capacity of FTS in municipality $m$ and

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[16]Weighted averages of those difference-in-differences estimators across years are unbiased estimators for the average effect of having changed treatment for the first time $k$ periods ago (de Chaisemartin and D'Haultfœuille, 2020).

academic year $t$ using Equation 2:

$$FTS_{mt} = \frac{\sum_{s \in m} \bar{e}_s FT_{st}}{\sum_{s \in m} \bar{e}_s}$$

(2)

where $\bar{e}_s$ is the average enrollment at school $s$ from 2001 to 2006 (prior to program implementation) and $FT_{st}$ is equal to one if elementary school $s$ participates in the FTS program at academic year $t$ and zero otherwise.

# 5    Results

## 5.1    Main Estimates

Panel (a) of Figure 2 presents the evolution of the predicted capacity of FTS in a municipality during the first year in which the first FTS was opened ($k = 0$) and for subsequent years ($k \in [1, 7]$). On average, during the first year in which a municipality implements the program, the predicted FTS availability increases by 18 percentage points. The effect reaches its maximum of 32 percentage points six years after, when the municipality had implemented the program for seven years.

Panel (b) of Figure 2 shows the estimated results of FTS on the average divorce rate. In the first year of program implementation, the divorce rate is not affected by the (first) expansion of the FTS program relative to municipalities that did not implement it yet; the estimated effect is close to zero and statistically insignificant. However, one year after ($k = 1$) the divorce rate significantly increases by 0.021 per 1,000 individuals. The effects on divorce increase with every year of municipalities' exposure to the FTS program. This increasing effect reflects a combination of increased length of exposure (in years since the first opening of an FTS in a municipality) and the expansion in the capacity that occurs over time within municipalities implementing this program, as shown in Panel (a). After the municipalities have been providing extended school days for seven years and FTS availability increased (on

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average) by 32 percentage points, this large expansion in the availability of implicit childcare subsidies translates into an average increase in the divorce rate of 0.105 divorces per 1,000 individuals.

To examine the validity of the underlying assumptions for identification, to the left of zero, Figure 2 shows estimates of placebo effects. In Panel (a), estimates of placebo effects are zero by construction because they measure the effects on the predicted capacity of FTS in a municipality before the program rollout. Considering the estimates of the effects of the extension in the school day on divorce, in Panel (b), we find that placebo estimates are close to zero and statistically insignificant, which provides support to our identification strategy (we cannot reject the null hypothesis that all placebos are statistically insignificant).[17]

## 5.2  Marriage Dissolution and the Role of Social Norms

Our main estimates show that the divorce rate significantly increases due to the availability of implicit childcare subsidies. In this section, we analyze the extent to which lack of access to free childcare constitutes a restriction on marriage dissolution in areas with different social norms.

In particular, we examine how the effects on divorce vary by pre-intervention divorce rates (i.e., average divorce rates for 2000-2007), which we consider as a proxy for social norms about divorce. We group municipalities above and below the cross-municipality median of the divorce rate over 2000-2007. Table 2 shows that extensions in the school day did not result in an increase in divorces in areas with low initial divorce rates (point estimates are close to zero and statistically insignificant). In contrast, in areas where divorces were more common prior to the roll-out of the FTS program, the implicit childcare subsidy positively and significantly affects marriage dissolution. Our estimates suggest that while financial constraints can impose substantial restrictions on divorce in municipalities where marriage

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[17]We estimate long-difference placebos. While first-difference placebo estimates test if common trends hold over pairs of consecutive periods, long-difference placebos test for the presence of common trends along several periods, where (if present) divergent trends in divorce rates are more likely to be detected (de Chaisemartin and D'Haultfœuille, 2020).

dissolution is more acceptable to society, childcare subsidies are unlikely to affect marriage stability in areas where social norms against divorce are more prevalent.

A second dimension we analyze is how the effects vary with the prevalence of pre-intervention female employment (measured with 2000 census data from IPUMS). Intuitively, gender norms may affect the participation of women in the labor market, making it difficult for women to increase their labor supply once their effective time endowment has increased due to the availability of extended school days. In Table 3, we estimate our main regression for the sub-samples of municipalities where female employment was above and below the cross-municipality median before the implementation of the implicit childcare subsidy. The estimated results show that the overall increase in divorces is mainly explained by the availability of childcare in municipalities with higher baseline female employment. This indicates that gender roles and their potential effect on labor force participation (and earnings) shape how childcare subsidies affect marriage dissolution.[18]

## 5.3   Childcare Availability and Female Economic Independence

Availability of childcare subsidies can translate into increased bargaining power and economic independence of women, who are typically the primary caregivers for children. Increased female labor supply can ease couples' financial restrictions and facilitate divorce when partners cooperatively make decisions. Higher female labor force participation can also lead to divorce due to within household conflict. Importantly, higher divorce risk can also lead to an increase in the labor force participation of women (Bargain et al., 2012). This implies that increases in labor force participation can act as a mechanism that facilitates divorce and as a response to increases in the risk of divorce and divorce rates.

Using household survey data, previous studies have documented a short-run increase in

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[18]Even though baseline divorce and employment rates are likely to be related to how traditional social norms are, they exhibit a weak positive correlation in the data (the correlation coefficient between the indicator variables for being above the median of the baseline female employment rate and for being above the median of the baseline divorce rate is 0.0657), which suggests they are capturing different dimensions that shape how lack of childcare availability affects divorce rates.

labor supply as a response to the extension in FTS availability in Mexico (Padilla-Romo and Cabrera-Hernández, 2019; Cabrera-Hernández and Padilla-Romo, 2020). We complement this literature by providing evidence on longer-term effects on female employment relying on census data. Focusing on the long-term also allows us to examine the impacts on female employment as an outcome that accompanies (and in some instances can mediate) the estimated increase in divorce rates due to the extension in the school days.

We use census data on female employment for 2000, 2010, and 2015 from IPUMS. We proceed by estimating TWFE models. In Table 4, Column (1), we show that consistent with findings in other studies, longer school days lead to an increase in female employment (Contreras and Sepúlveda, 2017; Padilla-Romo and Cabrera-Hernández, 2019; Cabrera-Hernández and Padilla-Romo, 2020; Kozhaya and Martínez Flores, 2020; Berthelon et al., 2022). Our estimate indicates that the extension in the school day by 3.5 hours generated a long-run average increase of 1.8 percentage points in the share of employed women (6.8% relative to the baseline in the ever-treated municipalities). However, this estimate masks substantial heterogeneity across municipalities. The effects are close to zero and statistically insignificant when restricting the sample to municipalities with low baseline female employment rates (female employment rate below the cross-municipality median in 2000). In contrast, in municipalities where women were more likely to be employed prior to the intervention (female employment rate above the cross-municipality median in 2000), the effects are large and statistically significant, with an increase in female employment of 3.6 percentage points (10.7% relative to the baseline).

Goodman-Bacon (2021) shows that standard TWFE models recover a variance-weighted average of all two-by-two comparisons in the data. In our setting, in which implementation of the FTS was staggered, this implies that municipalities in which the policy starts being implemented in year $t$ are compared with other municipalities that adopted this program earlier, leading to potentially biased estimates. We evaluate the severity of this concern by implementing a decomposition suggested by Goodman-Bacon (2021). Figure 3 shows

the difference-in-differences (DD) estimates for different comparison groups (ever treated vs. never treated, early treated vs. later treated, and later treated vs. early treated) in the vertical axis and the associated weights in the horizontal axis. In Panel (a), we report estimates for the overall sample. In Panels (b) and (c), we show estimates for the sub-samples of municipalities with baseline female employment rates below and above the median, respectively. The results of the decomposition suggest that our main conclusions remain valid. When comparing ever-treated and never-treated municipalities, we find an effect close to zero for municipalities with low initial female employment and positive and large for the rest of the municipalities.

# 6    Robustness

Our main results rely on the estimation method proposed in de Chaisemartin and D'Haultfœuille (2020), which is one of several estimators available to address issues related to staggered treatments and heterogeneous effects. However, each method relies on different sources of variation and set of identifying assumptions. To examine the robustness of our estimates to alternative estimators, in Panels (a) through (d) of Figure 4, we compare the estimated effects of FTS availability on divorce rates using de Chaisemartin and D'Haultfœuille (2020) with other three estimation methods: the OLS-TWFE estimator, the interaction-weighted estimator (Sun and Abraham, 2021), and the imputation estimator (Borusyak et al., 2021). Overall, the estimated effects across all four methods increase with every year of exposure to FTS with point estimates of between 0.105 and 0.138 after seven years of implementation of the extension in the school day. Moreover, in all cases, the estimates for the years prior to treatment are close to zero and statistically insignificant, supporting the parallel trends assumption.

Because Mexico implemented state-level reforms introducing unilateral divorce laws, we also examine the robustness of our results (that leverage municipality-level variation in the

extension of the FTS program) to controlling for state-level changes in divorce laws. Because controlling for additional treatments imposes empirical challenges, we start by studying this issue relying on a traditional fixed effects model, which provides more flexibility in terms of empirical implementation. In Panel (a) of Figure A.1, we show the estimated effects on divorce using a traditional OLS fixed-effects model adding an indicator for the implementation of unilateral divorce laws as control.[19] A caveat for these results is that in TWFE with multiple treatment indicators, the estimated coefficient in a particular treatment (i.e., availability of FTS in our context) is contaminated by a weighted sum of the effects of the other treatments in each municipality and year (de Chaisemartin and D'Haultfoeuille, 2020). Considering this concern, in Panel (b), we control for the implementation of unilateral divorce laws using the imputation estimator developed by Borusyak et al. (2021). The estimated results on divorce rates are robust to using this alternative approach.

Finally, because the average duration of marriages in the sample of divorcing individuals is 13 years (with a median duration of 11 years), we use the municipality of residence of the wife to capture exposure to the FTS program. A potential concern is that endogenous migratory responses induced by the program may affect our estimates. We examine the extent to which FTS explains the share of divorces in which the municipality of residence of the wife differs from the municipality of marriage in total divorces (in the municipality of residence). In Figure 5 we show that FTS expansion is not correlated with changes in this share, indicating that endogenous migration is unlikely to bias our estimates.

# 7  Conclusion

This study provides the first causal evidence on the effects of implicit childcare subsidies, through longer school days, on marriage dissolution. We find that the availability of FTS in a municipality increases the divorce rate per 1,000 people by 0.021 two years after the first

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[19]For most of the states, we rely on information from García-Ramos (2021) to identify the timing of the introduction of these laws. In addition, we combine information from Hoehn-Velasco and Penglase (2021) with information from alternative sources for the cases in which the date is missing in García-Ramos (2021).

FTS opened and that the effect increases over time, reaching 0.105 divorces per 1,000 people after seven years. Our main estimates suggest that the availability of FTS led to 23,314 additional divorces between 2009 and 2016, or approximately 5 percent of the total number of divorces filed during this time frame.[20]

Mothers and other female family members spend a disproportionate fraction of their time caring for children. Longer school days provided them with an increase in their effective time endowment that resulted in improved opportunities to acquire individual income. As a result, the intra-household bargaining positions are likely to be modified. Our results suggest that these changes in bargaining power translated into higher divorce rates in areas with social norms more favorable to divorce and to female employment, where lack of access to childcare was more likely to impose restrictions on marriage dissolution.

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[20]These numbers are calculated using the estimated effects in Column 1 of Table 2.

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**Figure 1:** Municipalities with and without Full-time Schools by Academic Year

**(a)** 2008/09

**(b)** 2010/11
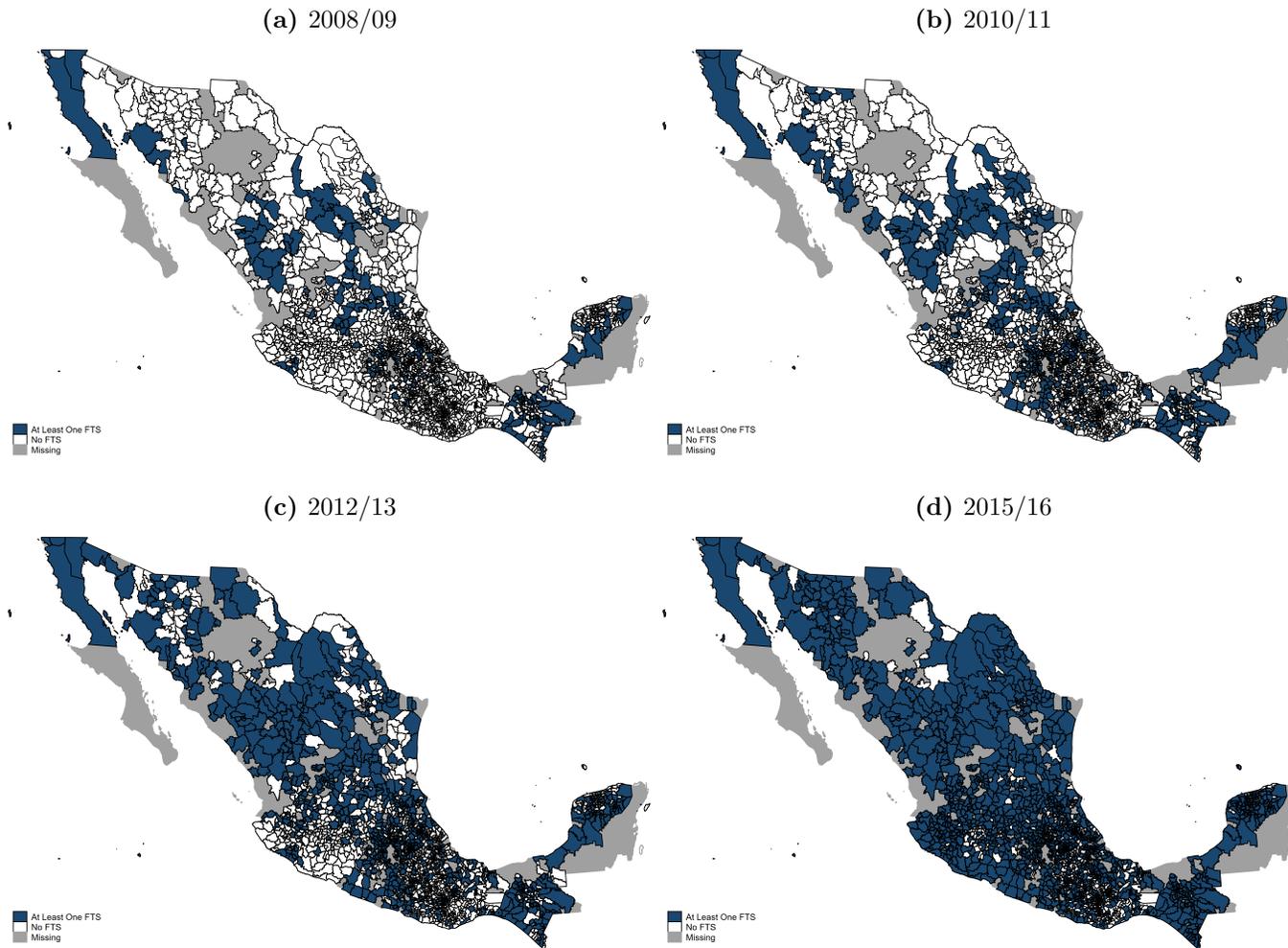
**(c)** 2012/13

**(d)** 2015/16



Notes: Each panel shows all 2,456 municipalities in Mexico and the staggered adoption of the Full-time Schools program. Overall, 220 municipalities had FTS in 2008/09, 495 in 2010/11, 891 in 2012/13, and 1527 in 2015/16.

**Figure 2:** Estimated Effects on Municipalities' Predicted Full-time Schools Availability and Divorce Rate

**(a)** Average Effects on Predicted Full-time Schools Availability
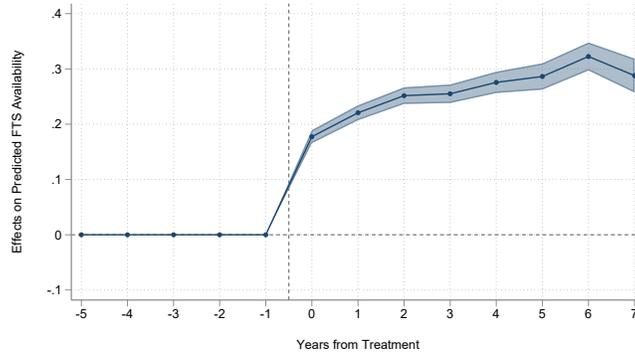


**(b)** Average Effects on Divorce Rate
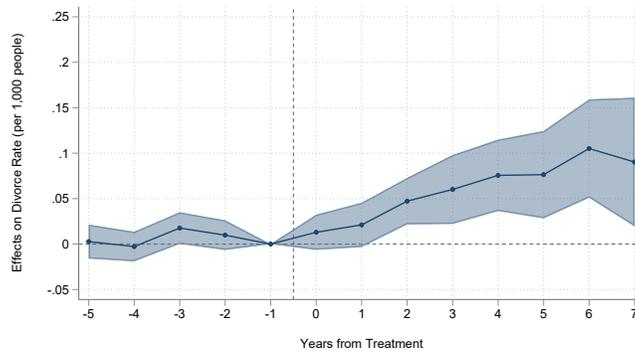


Notes: Panel (a) shows the evolution of the predicted share of Full-time Schools seats in a municipality after its first expansion of the Full-time Schools program. Panel (b) shows the estimated effects of the availability of Full-time Schools on divorce rates. All estimates are obtained using the de Chaisemartin and d'Haultfoeuille (2020)'s estimator and include municipality fixed effects, academic year fixed effects, and municipality-specific linear time trends. Standard errors for confidence intervals are clustered at the municipality level and bootstrapped based on 1,000 repetitions.

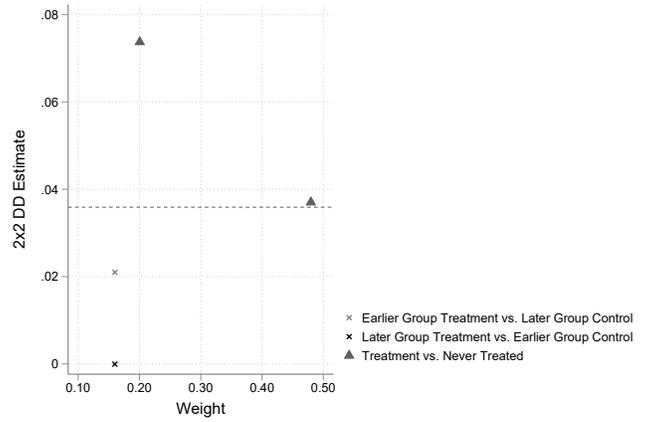**Figure 3:** Bacon Decomposition: Effects on Female Employment

**(a)** Overall
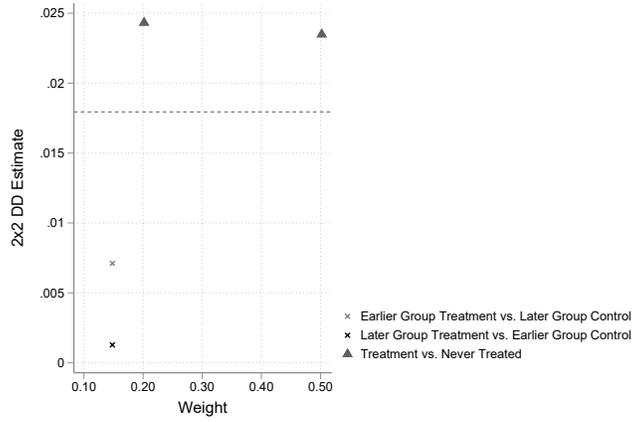


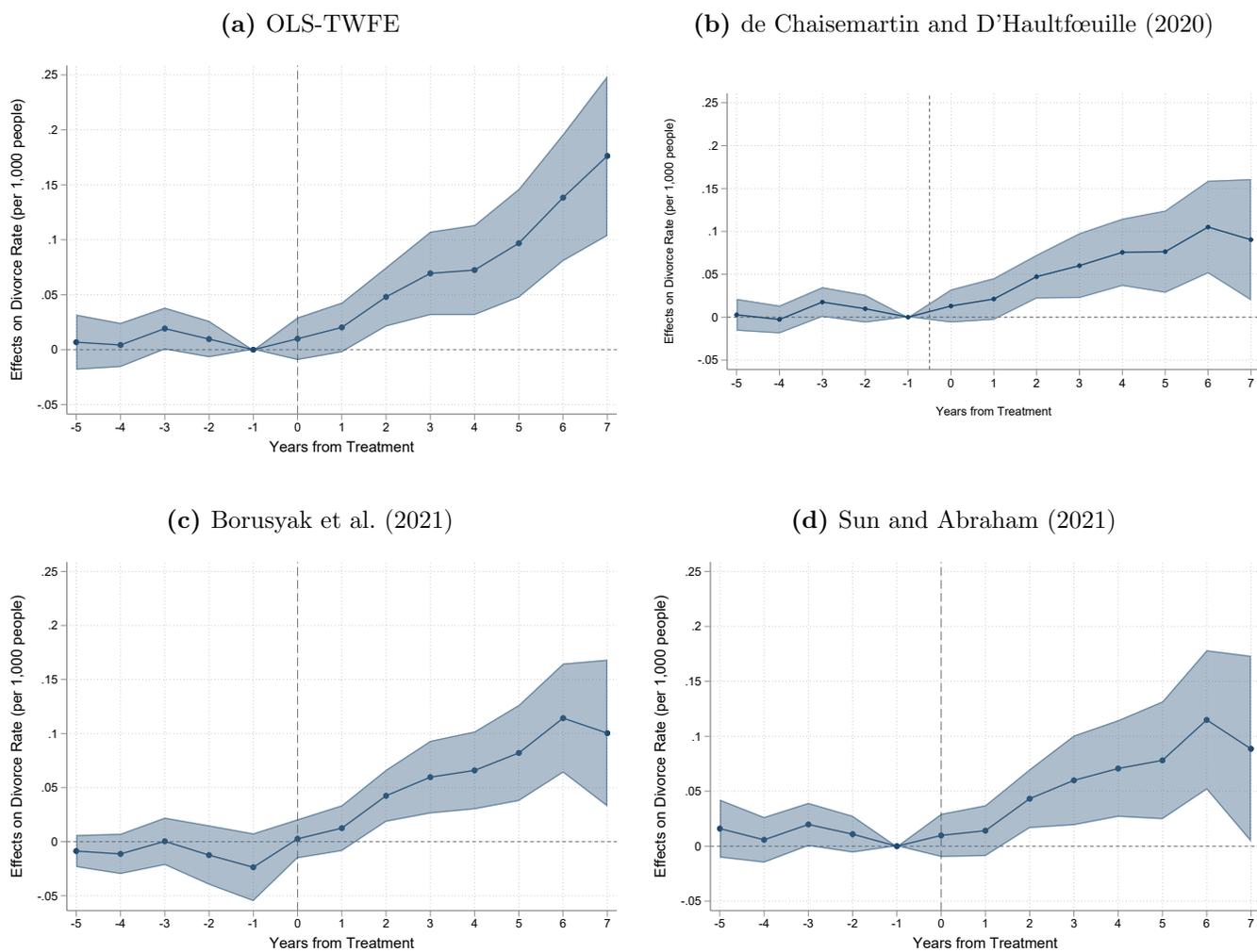**(b)** Municipalities with Low Female Employment
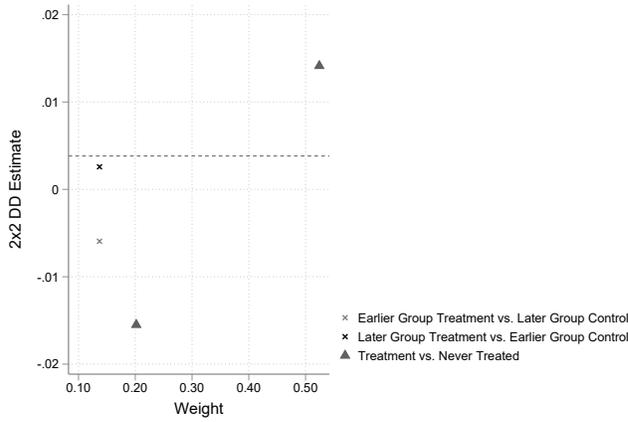


**(c)** Municipalities with High Female Employment



Notes: Panel (a) shows the results of a decomposition of the effect of the availability of Full-time Schools on female employment considering all municipalities. Panel (b) and (c), present estimates for municipalities with female employment below and above the median in 2000, respectively. Estimates are obtained using the method developed by Goodman-Bacon (2021). In each panel, the dashed line shows the ATT from the TWFE estimation and each triangle corresponds to a different 2x2 TWFE estimate (treated in 2010 or 2015 versus never treated).

**Figure 4:** Alternative Estimation Methods for the Effects of Full-time Schools on Divorce Rate

**(a)** OLS-TWFE

**(b)** de Chaisemartin and D'Haultfœuille (2020)

**(c)** Borusyak et al. (2021)

**(d)** Sun and Abraham (2021)



Notes: All estimates include municipality fixed effects, academic year fixed effects, and municipality-specific time trends. Standard errors for confidence intervals are clustered at the municipality level. Coefficients and confidence intervals in panels (a) through (d) are estimated using the Stata commands *reghdfe*, *did_multiplegt*, *did_imputation*, and *eventstudyinteract*, respectively, and are plotted using the *event_plot* command.

**Figure 5:** Effects on Migration
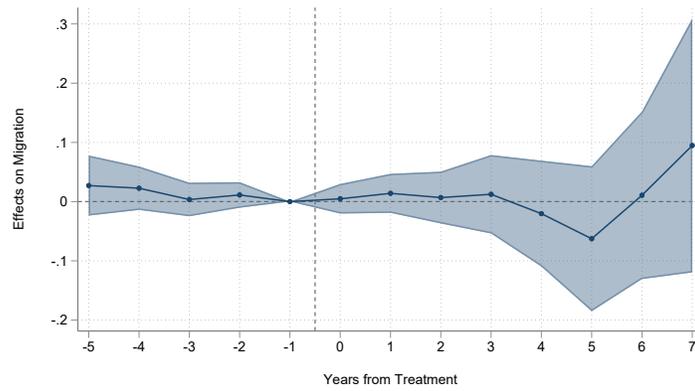


Notes: Estimated effects on the share of divorces in a municipality and academic year in which the municipality of marriage is different from the municipality of residence of the wife relative to the overall number of divorces in the municipality of residence. Estimates come from a single regression that includes municipality fixed effects, academic year fixed effects, and municipality-specific linear time trends, and are estimated using the de Chaisemartin and d'Haultfoeuille (2020)'s method. Standard errors for confidence intervals are clustered at the municipality level and bootstrapped based on 1,000 repetitions.

**Table 1:** Descriptive Statistics by Treatment Intensity

| | Ever Treated | | Never Treated | | Difference |
| | Mean | Std Dev | Mean | Std Dev | |
| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Divorce Rate (pre-intervention) | 0.258 | (0.359) | 0.123 | (0.262) | -0.135*** |
| Municipality with High (pre-2008) Divorce Rate | 0.583 | (0.493) | 0.306 | (0.461) | -0.277*** |
| Female Labor Force Participation (2000) | 0.265 | (0.108) | 0.266 | (0.133) | 0.000 |
| Municipality with High FLFP (2000) | 0.509 | (0.500) | 0.477 | (0.500) | -0.033*** |
| Population (in 1,000) | 41.788 | (112.352) | 7.964 | (11.185) | -33.825*** |
| Municipality with Unilateral Divorce Law | 0.454 | (0.498) | 0.344 | (0.475) | -0.110*** |

Notes: The table shows descriptive statistics at the municipality level. Divorce rates are reported for 2000-2007, prior implementation of the Full-time Schools Program. Municipality with High (pre-2008) Divorce Rate is an indicator equal to one if the municipality divorce rate in 2000-2007 is above the cross-municipalities median. Female employment is reported for 2000. Municipality with High female employment (2000) is an indicator equal to one if the municipality female employment in 2000 is above the cross-municipalities median. Municipality with Unilateral Divorce Law is an indicator equal to one if the municipality is located in a State that passed unilateral divorce laws by 2016. Never Treated are municipalities that were not covered by the FTS program by 2016 (N=651). Ever Treated are municipalities that implemented the program by 2016 (N=1,527). Data sources: divorce data from INEGI; Population from CONAPO; and female employment from 2000 Mexican Population Census (IPUMS). *, **, *** Significant at the 10%, 5%, and 1% levels, respectively.

**Table 2:** Estimated Effects on Divorce Rate by Pre-Intervention Divorce Rates

| | Overall | Low Pre-2008 Divorce Rates | High Pre-2008 Divorce Rates |
|---|---|---|---|
| | (1) | (2) | (3) |
| Initial Effect | 0.013 | 0.013 | 0.016 |
| | (0.010) | (0.010) | (0.016) |
| Years After = 1 | 0.021* | 0.009 | 0.040* |
| | (0.012) | (0.012) | (0.021) |
| Years After = 2 | 0.047*** | 0.022 | 0.075*** |
| | (0.013) | (0.015) | (0.022) |
| Years After = 3 | 0.060*** | 0.008 | 0.099*** |
| | (0.019) | (0.014) | (0.031) |
| Years After = 4 | 0.076*** | 0.024* | 0.122*** |
| | (0.020) | (0.014) | (0.035) |
| Years After = 5 | 0.076*** | 0.020 | 0.126*** |
| | (0.024) | (0.018) | (0.043) |
| Years After = 6 | 0.105*** | 0.025 | 0.177*** |
| | (0.028) | (0.022) | (0.048) |
| Years After = 7 | 0.090** | −0.016 | 0.172*** |
| | (0.036) | (0.028) | (0.061) |
| *Average Effect* | 0.046*** | 0.015 | 0.077*** |
| | (0.012) | (0.010) | (0.021) |

Notes: All estimates in each column come from the same regression that includes municipality fixed effects, academic year fixed effects, and municipality-specific linear time trends. All specifications are estimated using the de Chaisemartin and d'Haultfoeuille (2020)'s estimator. Standard errors in parentheses are clustered at the municipality level and bootstrapped based on 1,000 repetitions. Columns (2) and (3) show the estimated effects of the availability of Full Time Schools on divorce rates for municipalities with pre-intervention divorce rates below and above the cross-municipality median, respectively. *, **, *** Significant at the 10%, 5%, and 1% levels, respectively.

**Table 3:** Estimated Effects on Divorce Rate by Pre-Intervention Female Employment

|  | Overall (1) | Low Female Employment (2) | High Female Employment (3) |
|---|---|---|---|
| Initial Effect | 0.013 (0.010) | 0.012 (0.013) | 0.014 (0.015) |
| Years After = 1 | 0.021* (0.012) | 0.013 (0.017) | 0.028 (0.018) |
| Years After = 2 | 0.047*** (0.013) | 0.022 (0.018) | 0.071*** (0.019) |
| Years After = 3 | 0.060*** (0.019) | 0.011 (0.021) | 0.103*** (0.029) |
| Years After = 4 | 0.076*** (0.020) | 0.015 (0.026) | 0.128*** (0.029) |
| Years After = 5 | 0.076*** (0.024) | 0.032 (0.033) | 0.116*** (0.036) |
| Years After = 6 | 0.105*** (0.028) | 0.049 (0.033) | 0.151*** (0.042) |
| Years After = 7 | 0.090** (0.036) | 0.020 (0.047) | 0.154*** (0.055) |
| *Average Effect* | 0.046*** (0.012) | 0.018 (0.015) | 0.071*** (0.018) |

Notes: All estimates from each column come from the same regression that includes municipality fixed effects, academic year fixed effects, and municipality-specific linear time trends. All specifications are estimated using the de Chaisemartin and d'Haultfoeuille (2020)'s estimator. Standard errors in parentheses are clustered at the municipality level and bootstrapped based on 1,000 repetitions. Columns (2) and (3) show the estimated effects of the availability of Full Time Schools on divorce rates for municipalities with female employment below and above the median in 2000, respectively. *, **, *** Significant at the 10%, 5%, and 1% levels, respectively.

**Table 4:** Estimated Effects on Female Employment - Census Data

|  | (1) Overall | (2) Low Female Employment | (3) High Female Employment |
|---|---|---|---|
| Full Time Schools availability | 0.018*** | 0.004 | 0.036*** |
|  | (0.003) | (0.003) | (0.005) |
| N | 6,528 | 3,264 | 3,262 |
| Municipality FE | yes | yes | yes |
| Year FE | yes | yes | yes |

*Baseline Female Employment:*

| | |
|---|---|
| All Ever Treated Municipalities | 0.266 |
|  | (0.107) |
| Ever Treated Municipalities with Low Female Employment | 0.195 |
|  | (0.066) |
| Ever Treated Municipalities with High Female Employment | 0.336 |
|  | (0.092) |

Notes: Each column represents a different regression. All estimations include year and municipality fixed effects. FTS is an indicator for the presence of Full Time Schools in the municipality. Standard errors in parentheses are clustered at the municipality level. Information from IPUMS for years: 2000, 2010, and 2015. Baseline female employment is reported for Treated Municipalities in each group prior implementation of the Full Time Schools Program.
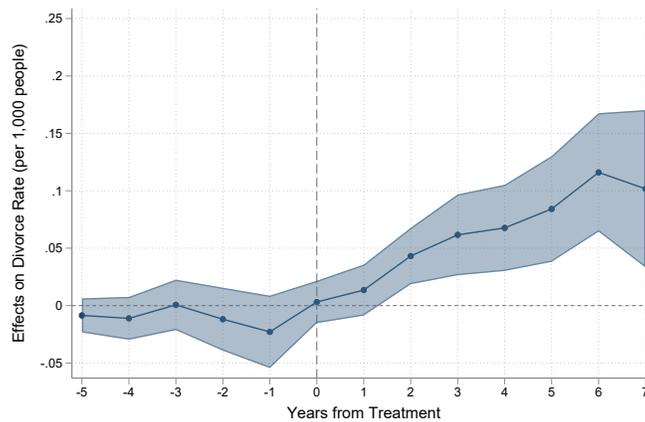*, **, *** Significant at the 10%, 5%, and 1% levels, respectively.

# Appendix A  Additional Results

**Figure A.1:** Estimated Effects on Divorce Rate- Controlling for Unilateral Divorce Laws

**(a)** OLS -TWFE controlling for unilateral divorce laws
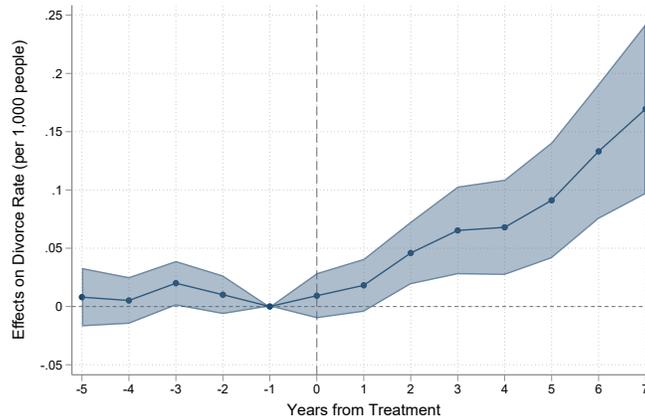


**(b)** Borusyak et al. (2021)'s estimator



Notes: Panel (a) shows the estimated effects using TWFE-OLS regression incorporating an indicator for the implementation of unilateral divorce laws. In Panel (b), we control for the implementation of unilateral divorce laws using the imputation estimator developed by Borusyak et al. (2021). All estimates from each panel come from the same regression that includes municipality fixed effects, academic year fixed effects, and municipality-specific linear time trends. Standard errors for confidence intervals are clustered at the municipality level and bootstrapped based on 1,000 repetitions.