Divorce and the birth control pill

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Abstract:

This paper explores the role of the birth control pill on divorce. To identify its effect, we use a quasi experiment exploiting the differences in the language of the Comstock anti-obscenity statutes approved in the 1800s and early 1900s in the US. Results suggest that banning the sales of oral contraceptive methods has a negative impact on divorce. These findings are robust to alternative specifications and controls for observed (such as female labour force participation, or changes in the early legal access to the birth control pill) and unobserved state-specific factors, and time-varying factors at the state level. Additional analysis, developed to examine whether the impact of subsequent divorce law reforms on divorce is modified after controlling for the birth control pill effect, shows that, although sales bans matter, the impact of divorce law reforms on divorce rate does not vary.

Keywords: Divorce rate, birth control pill, sales bans, unilateral divorce

JEL: J12, J13, J18, K36

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I. Introduction

Much recent research on divorce has concentrated on the impact of divorce law reforms on the evolution of the US divorce rate, in the main finding a positive relationship between liberalization of the laws and the divorce rate, (Friedberg 1998; Wolfers 2006; González-Val and Marcén 2011). Although the introduction of reforms may, at least in part, explain the acceleration of the US divorce rate since the late 1960s, such reforms have little to do with the rise in the divorce rate since the late 1950s. Researchers have looked at alternative explanations of this increase in divorce: the effect of major wars, (South 1985; Anderson and Little 1999), economic changes (Nunley 2010), and variations in female economic empowerment and in female labour force participation (Bremmer and Kesselring 2004; Nunley and Kietz 2009), among others. This paper presents evidence suggesting that the advent of the birth control pill also played an important role.

We are not the first to empirically analyze the impact of the birth control pill on divorce but, there is no existing literature that has examined this issue by considering sales bans of the birth control pill. Prior research papers have identified the impact of oral contraception on divorce by focusing on legal variations in early access to the birth control pill only among young, unmarried women, using US data (Goldin and Katz 2002; Nunley and Kietz 2009). Goldin and Katz (2002) find that access to the pill has a negative effect on the share of college women currently divorced (using data from the 1970, 1980 and 1990 US Census), and they find that access to the birth control pill increases the quality of the match by raising the age at first marriage, which allows individuals to sort themselves better at marriage. Variations in the law reforms granting early access to the pill were also utilized by Nunley and Kietz (2009) to construct an index of pill access among young women in order to analyse the effect of the pill on the

US divorce rate from 1929 to 2006. In this case, they find a positive relationship between pill access and the divorce rate, but this effect is difficult to interpret since they do not distinguish in their sample between those couples married prior to or after the changes in state laws regarding contraceptive access. This is relevant because it is not clear whether their results are driven by a sub-population, those married before the reforms, who were not affected by changes in the law. Also using US time-series data, but only from 1920 to 1974, Michael (1998) suggests that the diffusion of the pill and intrauterine devices can help to explain the growth in the US divorce rate since the 1960s. Similarly, Smith (1997) shows that the diffusion of the pill accounts for a significant portion of divorce actions in Great Britain.

Why do sales bans of the birth control pill matter? Bans on sales slow the diffusion of oral contraception (Bailey, 2010), then, for women (and not only young women) living in states without legal restrictions, the situation of divorce is more attractive, since the pill allows them to more easily decide whether and when to have children and maintain their attachment to the labour force (Smith 1997; Goldin and Katz 2000 and 2002).

In our empirical analysis, we use state-level data on divorce rates from 1950 to 2000. To identify the role of the birth control pill, we exploit the differences in the legal language of Comstock statutes that regulate sales bans of contraceptives, and the timing of the Griswold decision, which allowed states to repeal those sales bans (see, Bailey 2010, for a review). We find that divorce rates in states with sales bans are significantly lower than those without sales bans, after the introduction of the birth control pill, pointing to an important role of the birth control pill in the evolution of the divorce rate since the late 1950s. This is consistent with the use of different measures of divorce rates; the two main standard measures used are annual divorces per 1,000 married individuals, and annual divorces per 1,000 individuals.

Our findings contribute to the growing literature on the impact of oral contraception on several socio-economic variables. Several papers have pointed to access to the birth control pill as an important determinant of the decline in post-1960 US fertility (Bailey 2006, 2009, 2010; Guldi, 2008). Other researchers have studied the effect of the birth control pill on female labour force participation (Goldin and Katz, 2000 and 2002), female education (Ananat and Hungerman 2011; Hock 2007; Pezzini 2005), marriage (Edlund and Machado 2011; Christensen 2012), cohabitation (Christensen 2012), welfare (Pezzini 2005) and even on children's outcomes (Ananat and Hungerman 2011; Pantano 2007). We add to the existing literature by providing supplemental evidence suggesting that our results are not driven by unobserved state-specific factors, time-varying factors at the state level, variations in early legal access to the pill, the liberalization of divorce laws, or reforms in the aftermath of divorce.

We include controls for unobserved determinants at the state level that may be correlated with divorce, such as slow-moving demographic trends and/or changing social norms. In addition, we include observable factors that appear to be related to divorce, such as female labour force participation. It is also arguable that the coefficients capturing the effect of sales bans may be measuring the response of divorce rates to changes in early legal access to the birth control pill, in addition to - or instead of - the effect of the sales bans on divorce. To examine this issue, we incorporate in our main specification controls for legislative variations, across states, on the timing of early legal access to the birth control pill using information from Bailey (2006). After introducing these controls, the estimated coefficients picking up the impact of the birth control pill change very little.

As an additional check that variations in the sales bans of birth control pill are driving the evolution of divorce rates since the late 1950s, we analyze the differential evolution of divorce rates between states with and without sales bans. Since one interpretation of our results is that the increase in the divorce rates in states without sales bans is simply due to factors unrelated to differences in the Comstock statutes; if this were the case, then differences in divorce rates might be maintained, even as states revised their statutes in the aftermath of the Griswold decision that led to the lifting of the ban on the use of contraceptives. Our findings show that, five years after the Griswold decision, the difference in divorce rates between those states with and without sales bans decreases, indicating that our results are not driven by forces other than the variations of the Comstock anti-obscenity statutes.

In the final section, we examine whether taking into account the birth control pill effects reduces the impact of divorce law reforms on divorce rates. The liberalization of the divorce law reforms began some years after the introduction of the birth control pill, and it is possible to argue that prior analysis of the effect of divorce law reforms may be measuring the response of divorce rates to both divorce law reforms and the introduction of the birth control pill. To analyse this issue, we introduce controls for divorce laws that regulate how to get a divorce, in addition to controls for laws that govern living arrangements in the period subsequent to divorce, which also have an impact on the evolution of divorce rates in the subsequent decades (Nixon 1997; Brinig and Buckley 1998; González-Val and Marcén 2011). We find that the effects of divorce law reforms do not vary.

The remainder of the paper is organized as follows: Section II presents the empirical strategy, Section III describes the data; baseline results and robustness checks are discussed in Section IV, in Section V we analyse whether the effects of divorce law reforms are maintained, and Section VI sets out our main conclusions.

II. Empirical Strategy

Our empirical approach makes use of the differences in the language of the Comstock statutes, enacted in 48 states in the US during the late 1800s and early 1900s, to identify the effect of the birth control pill on divorce rates.[1] This identification strategy was previously used by Bailey (2010) to ascertain the role of the birth control pill in the evolution of the US fertility rate. As Bailey (2010) explained, obviously, the Comstock anti-obscenity laws were not passed to control the use of the birth control pill, but they did have a significant impact on access to contraceptives several decades later. Among the states that passed those laws, 30 statutes explicitly considered contraception as an obscenity and 24 banned the sales of contraceptive supplies.[2] Then, when Enovid, the birth control pill, was approved by the US Food and Drug Administration (FDA) for the regulation of menses, and 3 years later, in 1960, as an oral contraceptive, its access was clearly limited. As shown in Bailey (2010), sales bans significantly reduced the use of the birth control pill in states with those restrictions. Of course, we certainly recognize that differences in social norms, states' judiciary, and legislatures, could result in differences in the application of these laws but, as in Bailey (2010), we favour the use of the variation in the language of Comstock laws to measure the effects of the birth control pill.

To begin with, the estimation strategy enables a difference-in-differences approach. The following equation forms the empirical framework of our analysis:

[1] There is no information on Comstock laws in the case of Alaska, District of Columbia, and Hawaii, see Bailey (2010). Then, those states are not considered in our analysis.
[2] These laws are coded in Bailey (2010).

$$Divorce\ rate_{s,t} = \beta Sales\ bans_{s,t} + \Sigma_s\ State\ fixed\ Effects_s + \Sigma_t\ Time\ fixed\ Effects_t + [\Sigma_s\ State_s{*}Time_t + \Sigma_s\ State_s{*}Time^2_t] + \varepsilon_{s,t} \quad (1)$$

where *Sales bans*$_{s,t}$ is a dummy variable equal to "1" when a state *s* had a sales ban of the birth control pill in year *t*, and *0* otherwise. The parameter β is interpreted as the average difference in the divorce rate in states with sales bans, versus the comparison group (see the discussion below). From a theoretical point of view, as mentioned above, if the use of the birth control pill makes divorce more attractive for women, then the sign of this β-parameter should be negative. The birth control pill diminishes the marginal costs of preventing births and so, for some women, it should lead to a reduction in the number of children born per woman, reducing the value of marriage (Goldin and Katz 2000; 2002). The birth control pill also helps women to maintain their attachment to the labour force and their human capital, which also makes divorce more attractive (Goldin and Katz 2000; 2002). Thus, those states with sales bans, which reduce the diffusion of this oral contraceptive (Bailey 2010), are expected to have lower divorce rates.

However, the birth control pill may also increase the value of marriage since it can have a positive impact on the number of births by eliminating the risk of overshooting, (Bailey 2010), or by improved sorting of young women at marriage, (Goldin and Katz 2000;2002). Then, the sign of β is theoretically ambiguous. Equation (1) also includes state fixed effects and year fixed effects to control for evolving unobserved state-level characteristics, and linear and quadratic trends, which allow us to capture trends in state-level unobserved attributes affecting divorce. The inclusion of those controls is quite common in the economic literature on divorce, (see, for example, Friedberg 1998, Wolfers 2006, González-Val and Marcén 2011).

III. Data

We utilize data on divorce for the period 1950 to 2000. The divorce rate is defined as the annual number of divorces per 1,000 married individuals. We acknowledge that this is not the standard measure of divorce (because of availability problems), though the population 'at risk of divorce' is considered appropriately with this measure.[3] We obtain the stock of married people in order to calculate this rate for the years in which the decennial US census was conducted.[4] Yearly data was reckoned by linear interpolation. The annual number of divorces comes from Vital Statistics of the United States, and Wolfers (2006).

Figure 1 plots the divorce rate, measured as annual divorces per 1,000 married individuals in the US. From 1950 to 1958, the US divorce rate slightly decreases, reaching the lowest rate in the period analysed of around 4 divorces per 1,000 married individuals, and there is then an increase in the divorce rate from 1959 until 1976. This period can be divided in two parts because of the differences in the rate of growth of the divorce rate, one until the late 1960s and the other from the late 1960s to 1976 when the divorce rate slopes steeply. That is followed by a stable period around a rate of 11 divorces per 1,000 married individuals. This stability terminates in the early 1980s, and since then we observe a smooth decline that continues until the end of our sample in 2000. The evolution of the divorce rate described here is not restricted to this specific measure of divorce. The standard definition of divorce, also plotted in Figure 1, the

[3] We also run all the analysis using the crude divorce rate, see below. Results do not substantially vary.
[4] This information was obtained from the Integrated Public Use Microdata Series (Ruggles et al. 2010).

crude divorce rate (annual number of divorces per 1,000 individuals), displays a quite similar behaviour.

This quick glance at the temporal evolution of the divorce rates appears to show the presence of a potential causal relationship between the diffusion of the birth control pill (Enovid), approved in 1957, and the increase in the divorce rate. This relationship is also reflected in Figure 2, representing the evolution of the average divorce rate across states with differences in the state-specific language of the Comstock anti-obscenity laws. We have included the average divorce rate of those states that prohibited the sale of any article, instrument, or medicine for the prevention of conception (24 states), and the average divorce rate of those without sales bans on contraceptive methods (24 states). The short-dashed line shows the evolution of the difference in the average divorce rate between those states that implemented sales bans versus those that did not establish such bans. We observe that the increase in the average divorce rate occurs in those states that did not introduce sales bans until the Griswold decision, and so it seems this restriction is relevant in the evolution of the divorce rate.

Despite the rise in the divorce rate in those states that did not explicitly introduce sales bans in their statutes, it is worth noting that it is not clear whether some of these states (mainly those located in the south) did not intentionally mention the prevention of conception in their Comstock laws, or whether they did not introduce it since there existed older statutes prohibiting the "corruption of morals", regulating the prevention of conception, as Bailey (2010) explained. To avoid this problem, we consider in our main analysis only those states that made any reference to prevention of conception in their Comstock statutes (29 states), although, for consistency, we have also repeated the analysis by including the rest of the States in the comparison group (18 states). [5]

IV. Results

A. Baseline Regression

Table 1 presents the estimates for Equation (1). In column (1), incorporating state and year fixed effects, divorce rates are significantly lower in those states with sales bans relative to those without sales bans. As can be seen in columns (2) and (3), this is supported even after introducing state-specific linear and quadratic time trends, although the magnitude of the estimated coefficient of the sales ban in those columns varies somewhat. This may be because we are removing not only state fixed attributes but also time-varying unobservable state-level characteristics that could bias the empirical findings shown in column (1).

To test whether our results are sensitive to our definition of divorce, we ran a quite simple robustness check. As mentioned above, we have also repeated the analysis using an additional dependent variable, the crude divorce rate, defined as the number of annual divorces per 1,000 individuals. Results are displayed in Table A (see Appendix A) that shows that, despite the small change in the magnitude of the impact of our variable of interest (this is not surprising since the denominator of the dependent variable has changed), we can still observe a negative and significant effect of sales bans on divorce. Then, sales bans of the birth control pill seem to be relevant in the evolution of the divorce rate.

[5] As suggested in Halla (2011), we have excluded Nevada since the evolution of the divorce rate in this state is quite different to the rest of the states. As a robustness check, we ran all the analysis including Nevada and results are maintained.

The interpretation of prior results may be a touch problematic since, in previous analysis, we have considered that all States repealed or amended their Comstock anti-obscenity statutes in 1970. We do that since almost all states removed their sales bans on contraception to married individuals by the year 1970 (Pilper and Wechsler 1969; Department of Health, Education, and Welfare 1974). However, as Bailey (2010) mentioned, it is not clear whether the decisions of those states were responses to the Griswold decision, (that decision imposed a provision in Connecticut's 1879 law, other States across the US responded by repealing or amending their Comstock anti-obscenity statutes in the subsequent years, (Bailey 2010)), since there were some state programs that subsidized family planning before the date on which state sales bans on contraception were repealed. Then, for instance, if states that repealed sales bans in year t-2 are being classified as states with sales bans in year *t*, this could lead to a bias in the estimates of the impact of sales bans on divorce. To examine this issue, we have re-run all the analysis by considering that sales bans were repealed in different years, (1965, 1967, 1975 and, 1980). Results are presented in Table B, Appendix B. As can be seen, in all cases, the estimated coefficients are negative and significant; thus, those states with sales bans had lower divorce rates. Note that the magnitude of the effect varies somewhat, especially when we assume that all sales bans were repealed in 1965. But, this is not striking; since, in this case, our estimates are clearly biased, we are categorizing some states that rescinded sales bans later as states that repealed their statutes in year 1965. For the rest of the estimates, we observe that the impact of the sales bans on divorce was around 0.5-0.6 divorces per 1,000 married population. As expected, the differences in divorce rates seem to be mitigated in absolute value as times goes by, which coincides with the removal of sales bans.

As an additional robustness check, we consider all states in our analysis.[6] It might bias our results since it is not clear whether some states did not intentionally introduce the prevention of conception in their Comstock laws (see above). After including all states, our estimates are not significant, albeit negative when we add state and year fixed effects and state-specific quadratic time trends, columns (1) and (3). Thus, it seems that, in this case, sales bans of the birth control pill do not matter. However, plotting the residuals from the specifications presented in Table 2, it is observed that, in this case, not only quadratic trends but also cubic trends need to be added to pick up the impact of the sales bans because of the trending behaviour of the residuals (see a similar analysis in Friedberg (1998)). The inclusion of these state-specific trends can be justified to address omitted variables bias. We revisit this issue below. After including cubic trends, column (4), we again obtain a negative and significant coefficient suggesting that those states with sales bans had lower divorce rates.[7] Although we acknowledge that these estimates may be biased, it is comforting that our results are maintained.

B. Is it sales bans, or is it the increase in female labour force participation?

Since an increase in the divorce rate can also be attributed to a rise in female labour force participation (Allen 1998; Bremmer and Kesselring 2004; Nunley and Kietz 2009), it is possible to argue that its effect, which is correlated with the outcome of interest, if omitted, would be captured by the coefficients measuring the effect of the

[6] As explained above, Nevada is also excluded in this case.

[7] Prior analysis that excludes those states that did not mention prevention of conception in their statutes, has also been repeated including cubic trends. However, in this case, the analysis of the residuals points to the specification that includes state-specific quadratic trends to address the omitted variable bias. All these figures are available upon request.

sales bans. To tackle this issue, we provide additional evidence to ascertain that our empirical findings are not driven by omitted variables. We add female labour force participation (FLFP) to the baseline regression. Data on FLFP at the state level come from the Current Population Survey and from the Integrated Public Use Microdata Series (gaps were filled by linear interpolation).

Column (1) of Table 3 shows the regression results of Equation (1) after adding FLFP as a control. In this specification, the effect of the sales bans is still negative and significant. The coefficient capturing the effect of FLFP, as expected, is positive and significant. Results on our variable of interest do not vary. In the second column, we have also included a quadratic term for FLFP. Although, to our knowledge, there is no existing literature finding a nonlinear relationship between divorce and FLFP, some works have suggested that this relationship exists between FLFP and other outcomes such as the Total Fertility Rate, (Ahn and Mira 2002). Thus, as an additional check, we have also added this quadratic term to our specification. Results do not change, column (2), and the coefficients capturing the impact of FLFP are no longer significant. Then, it seems that there is not a quadratic relationship between them.

We acknowledge that we have not included in the analysis other factors of divorce suggested in the literature, which may be different by state but have little to do with the sales bans of the birth control pill. The inclusion of these may be problematic since, as Allen (2002) suggested, many of the measures of economic performance, such as female earnings and even FLFP, or other demographic attributes such as fertility rates, have not been truly exogenous. Indeed, Becker (1981) explained that causality between these variables and the divorce rate may run in both directions. Thus, because of endogeneity concerns, we prefer not to include them in the rest of the analysis, although, as shown previously, results are unchanged after adding, for example, FLFP. [8]

C. Is it sales bans, or is it other reforms?

While sales bans on contraception could affect the diffusion of the birth control pill in the US, other US regulations, such as the liberalization of access to this oral contraceptive for young women, could also have an impact on the diffusion of the birth control pill. As shown in Goldin and Katz (2002), access to the birth control pill has a negative effect on the share of college women currently divorced. Therefore, one can argue that the effect of sales bans may be confounded with changes in early legal access to the pill. To explore this issue, we include in our baseline regression a dummy variable to control for the changes in the early legal access to the pill, using information from Bailey (2006). This variable is defined as follows: it takes the value "1" when a state s has a law that allows unmarried, childless women under 21 to legally obtain the birth control pill without parental, and 0 otherwise.

Results are presented in column (3) of Table 3. Again, the relationship between the sales bans and the divorce rate is negative and significant, although coefficients are slightly lower in absolute value. With respect to the changes in early legal access to the birth control pill, it is observed that parameters are not significant. [9] These results should be viewed with caution, since our dependent variable is defined as the annual number of divorces per 1,000 married individuals. Then, we incorporate a sub-population that is not affected by changes in the early legal access to the birth control pill, (those already married when the reforms took place). Because of that, and since our results do not vary

[8] Note that all the analysis has also been run after including FLFP and results are maintained.
[9] Our findings remain unchanged even if we only introduce state and year fixed effects or state-specific linear trends.

by much, we prefer not to include the early legal access to the pill in the rest of the analysis. [10]

While the Comstock statutes were repealed or amended, there was another wave of reforms that could drive the behaviour of the divorce rate since the late 1960s. This potential determinant of divorce is the liberalization of the divorce laws (Friedberg 1998; Wolfers 2006; González-Val and Marcén 2011). This is relevant since, in that period, many states passed new divorce laws that made divorce easier; for instance, in the period 1969 to 1971, 11 states approved divorce law reforms (Friedberg, 1998). Thus, it is possible to claim that our estimated points are confounding both effects: the effect of divorce law reforms and the effect of sales bans. To tackle this issue, we run the next equation:

$$Divorce\ rate_{s,t} = \beta Sales\ bans_{s,t} + \Sigma_k\ \delta_k Reform_{s,t,k} + \Sigma_s\ State\ fixed\ Effects_s + \\ + \Sigma_t\ Time\ fixed\ Effects_t + [\Sigma_s\ State_s * Time_t + \Sigma_s\ State_s * Time^2_t] + \varepsilon_{s,t} \qquad (2)$$

where we control for the response of divorce rates to divorce law reform, à la Wolfers, Wolfers (2006). $Reform_{s,t,k}$ is a set of dummy variables equal to "1" when the state s has implemented a new divorce law regime in year t for k periods, and "0" otherwise. By including these dummies, the entire dynamic response of divorce to divorce law reforms is supposed to be captured. The remaining variables are defined as in equation (1). In column (4) of Table 3, we show the estimates of interest for Equation (2). Once more, results do not vary. We still observe a negative and significant effect of sales bans on divorce after adding controls for the liberalization of divorce law reforms. Finally, we have also introduced in a specification all controls to check whether our results

[10] We repeated all the analysis adding controls for the variations in early legal access to the pill and results do not vary substantially.

remained unchanged. The fifth column in Table 3 reports these estimates. Again, our empirical findings do not vary substantially, though the magnitude of the effect of sales bans declines slightly in absolute value. Thus, our findings suggest that the impact of the birth control pill on divorce is relevant.

D. Differential Evolution of the Divorce Rate

As previously explained, one interpretation of our results is that the increase in the divorce rates in states with sales bans simply occurred for reasons unrelated to differences in the language of the Comstock statutes. If this were the case, then differences in divorce rates might not be mitigated, even if states repealed or revised their statutes in the aftermath of the Griswold decision. To probe this further, following Bailey (2010), we use an alternative strategy estimating the short-run and long-run differences between those states with and without sales bans.[11] We estimate the next expression:

$$\begin{aligned} Divorce\ rate_{s,t} &= Sales\ bans_{s,t}\,\boldsymbol{f}'_t\boldsymbol{\beta} + \Sigma_s\ State\ fixed\ Effects_s + \Sigma_t\ Time\ fixed\ Effects_{t}\,+ \\ &\quad + [\Sigma_s\ State_s{*}Time_t + \Sigma_s\ State_s{*}Time^2_t] + \varepsilon_{s,t} \qquad (3) \end{aligned}$$

with $\boldsymbol{f}'_t$ =(1, 1(t=1951),…,1(t=1980)), the remaining variables remain as in equation (1). In this case, $\boldsymbol{\beta}$ parameters capture changes in the gap in divorce rates in states with sales bans, relative to those without sales bans, in the period considered, from 1951 to 1980. A negative sign of the β parameter indicates that the divorce rate in state s with a sales

[11] The methodology used earlier only identifies discrete series break.

ban was lower in year t than that in a state without sales bans. The interpretation of a positive sign would be just the opposite.

Figure 3 plots our estimates of the β coefficients of Equation (3) and the 95% confidence interval, including state and year fixed effects, state-specific linear trends, and also quadratic trends. As explained above, those β parameters capture changes in the gap in divorce rates in states with sales bans, relative to those without sales bans. In the early and mid-1950s, the gap in divorce rates is not statistically significant. After the approval of Enovid to regulate the menses in 1957, coefficients are statistically significant and with little variation in their magnitude until the early 1960s. Since then, we observe that the gap in the divorce rate between those states with and without sales bans considerable increased, with those with sales bans having much lower divorce rates. After 1970, the gap in the divorce rate decreases until 1980. The fact that sales bans on contraception are associated with lower divorce rates since the 1960s, and that those differences decreased since the 1970s, is consistent with their increasing relevance in step with the approval of its use as a contraceptive in 1960, and the decline in importance of the sales bans since 1970, when almost all states had eliminated that prohibition. Results remain unchanged, even if we re-define f'_t to consider differences in divorce rates due to sales bans until 1970, and then until 1975. Additionally, we have repeated this analysis including all controls (see Figure 4). The behaviour of the gap in the divorce rate between those states with and without sales bans, described above, is maintained. All in all, our results suggest that sales bans on the birth control pill played an important role in the evolution of US divorce rates.

V. The effect on divorce of the divorce law reforms

Up to this point, we have empirically analysed the effect on the divorce rate of banning the birth control pill. In this section, we examine whether considering the birth control pill impact reduces the effect of divorce law reforms on divorce rates. Both changes were implemented, in many states, at about the same time during the 1960s and 1970s, so that it can be argued that prior analysis of the effect of divorce law reform on divorce rates (such as Friedberg 1998; Wolfers 2006; González-Val and Marcén 2010) may be measuring the response of divorce rates to the advent of the pill, in addition to, or instead of, the response of divorce rates to changes in divorce laws.

We test this by comparing the effect of no-fault unilateral divorce reforms on divorce rates, with and without controls for sales bans. Our results are shown in Table 4 where the variables capturing the effect of the divorce law reforms are introduced à la Wolfers, (Wolfers 2006), as in Equation (2). Columns (1) to (3) show the estimates without the sales ban variable, and columns (4) to (6) with the sales ban variable. We can see that coefficients measuring the effect of divorce law reforms change vary little after including sales bans as controls. Column (7) reports our estimates after including controls for law reforms concerning the aftermath of divorce, which may also have an impact on the evolution of divorce rates in subsequent decades (Nixon 1997; and Brinig and Buckley 1998; González-Val and Marcén 2010; Halla 2011). We introduce two main policy changes, as in González-Val and Marcén (2011), that have swept the U.S. since the late 1970s: the approval of the joint-custody regime, and the Child Support Enforcement program. Data come from Leo (2008) and González-Val and Marcén (2011). Even after adding all those controls, we find similar results: the effects of the divorce law reforms do not change.[12] However, in contrast with existing works on the impact of divorce law reforms on divorce rates that find permanent effects, we simply observe that divorce law reforms have a positive and significant impact on divorce rates, two years after their introduction. This may be due to the fact that our sample only contains a portion of states (29 states) rather than all the states as in other studies. If we are considering many states that did not pass reforms, or those in which the impact of the reform was lower, our results could be biased. Then, it is possible to argue that, due to this sample selection problem, our results are not comparable with previous works.

Finally, Table 5 shows our results after including all the states.[13] In the first column, we see that all coefficients are positive and significant, suggesting a permanent response of divorce rates to divorce law reforms. When we add the sales ban dummy in column (2), results do not change. The coefficient capturing the impact of sales bans on divorce is negative and significant, albeit just at the 10% level, even after adding only state-specific quadratic trends.[14] Thus, results suggest that sales bans do not affect the subsequent response of divorce rates to divorce law reforms.

VII. Conclusions

The purpose of this paper is to analyse the role of the birth control pill on US divorce rate. Since women using the birth control pill can easily decide when, and whether, to have children, and whether or not to maintain their attachment to the labour force (which makes divorce more attractive), we would expect a negative impact of sales bans

[12] The number of observations is lower in the case of column (7) since there is only information until 1998 for one of the Child Support Enforcement variables that we have introduced, see González-Val and Marcén (2011).

[13] Alaska, District of Columbia, Hawaii and Nevada are also excluded here, as explained above.

[14] In the analysis presented in Table 2, we only obtain significant results when adding cubic trends. After adding the divorce law reform variables, we get better results on the effect of the birth control pill on divorce, introducing State-specific quadratic time trends. Then, cubic trends could be capturing the effect of these omitted variables in Table 2. In this case, we should recognize that these estimates are biased, since it is not clear whether some of the States in the comparison group regulated prevention of conception issues in earlier statutes.

of the contraceptive pill on divorce. To explore this issue, we utilize data from the US for the period from 1950 to 2000.

Our empirical findings indicate that sales bans have a negative impact on divorce, irrespective of the measure of divorce rate considered. We further provide additional empirical evidence showing that our findings are picking up the effect of banning the birth control pill rather than the impact of omitted variables. The relationship between divorce rates and sales bans is consistent with the inclusion of controls for female labour force participation, with changes in the early legal access to the pill, and with the liberalization of the divorce laws. All these findings are quite robust after adding controls for unobserved state-specific attributes, and time-varying attributes at the state level.

Moreover, we examine the evolution of the divorce rate gap between states having, or not, introduced sales bans. We find that sales bans of the birth control pill are related to lower divorce rates since the 1960s, and that those differences decrease since the 1970s. This is consistent with the increasing relevance of the birth control pill with the approval of its use as a contraceptive in 1960 and the decrease in the importance of the sales bans since 1970, when almost all states had repealed or amended their regulations on banning the sales of the birth control pill.

Finally, we explore whether considering the effects of the advent of the birth control pill diminishes, or even annuls, the impact of divorce law reforms on divorce rates. Both changes were implemented in many states quite close to each other in time, so that, it is arguable that prior work on the impact of divorce law reforms on divorce rates may be measuring the response of divorce rates to the pill, in addition to or instead of the response of divorce rates to the implementation of the new divorce legal regimes. Our clear finding is that, although sales bans are relevant, the impact of divorce law reforms remains unaffected even after the introduction of an extensive number of potential explanations that could also be responsible for the evolution of the divorce rate in the US.

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Table 1- THE EFFECT OF SALES BANS ON DIVORCE
(*Dependent variable: Annual divorces per 1,000 married individuals*)

	(1)	(2)	(3)
	Basic specification	State-specific linear trends	State-specific quadratic trends
Sales Ban	-0.470***	-0.311*	-0.645***
	(0.151)	(0.170)	(0.138)
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
State * time	No	Yes	Yes
State * timesq	No	No	Yes
Observations	1413	1413	1413
R-squared	0.930	0.950	0.964

Notes: Estimated using state population weights (equal to the denominator of the dependent variable). Robust standard errors in parentheses. The dependent variable is defined as the annual number of divorces per 1,000 of married population. Data on annual divorces come from the Vital Statistics of the United States and the information on total married population was obtained from the Integrated Public Use Microdata Series. Yearly data on married population was calculated by linear interpolation. Sales bans coded by Bailey (2010). Significant at the *** 1% level; ** 5% level, and * 10% level.

Table 2- THE EFFECT OF SALES BANS ON DIVORCE CONSIDERING ALL STATES

(*Dependent variable: Annual divorces per 1,000 married individuals*)

	(1)	(2)	(3)	(4)
	Basic specification	State-specific linear trends	State-specific quadratic trends	State-specific cubic trends
Sales Ban	-0.074	0.044	-0.091	-0.263**
	(0.119)	(0.109)	(0.103)	(0.132)
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
State * time	No	Yes	Yes	Yes
State * timesq	No	No	Yes	Yes
State * timecb	No	No	No	Yes
Observations	2314	2314	2314	2314
R-squared	0.921	0.949	0.964	0.970

Notes: Estimated using state population weights (equal to the denominator of the dependent variable). Robust standard errors in parentheses. The dependent variable is defined as the annual number of divorces per 1,000 of married population. Data on annual divorces come from the Vital Statistics of the United States and the information on total married population was obtained from the Integrated Public Use Microdata Series. Yearly data on married population was calculated by linear interpolation. Sales bans coded by Bailey (2010). Significant at the *** 1% level; ** 5% level, and * 10% level

Table 3- THE EFFECT OF SALES BANS ON DIVORCE AFTER ADDING CONTROLS

(*Dependent variable: Annual divorces per 1,000 married individuals*)

	(1)	(2)	(3)	(4)	(5)
Sales Ban	-0.614***	-0.615***	-0.633***	-0.657***	-0.597***
	(0.139)	(0.139)	(0.140)	(0.130)	(0.132)
FLFP	0.024*	0.042			0.039***
	(0.014)	(0.047)			(0.014)
FLFP square		-0.016			
		(0.039)			
Early Legal Access			-0.100		-0.091
			(0.178)		(0.180)
Years Unilateral Divorce	No	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
State * time	Yes	Yes	Yes	Yes	Yes
State * timesq	Yes	Yes	Yes	Yes	Yes
Observations	1413	1413	1413	1413	1413
R-squared	0.964	0.964	0.964	0.965	0.966

Notes: Estimated using state population weights (equal to the denominator of the dependent variable). Robust standard errors in parentheses. The dependent variable is defined as the annual number of divorces per 1,000 of married population. Data on annual divorces come from the Vital Statistics of the United States and the information on total married population was obtained from the Integrated Public Use Microdata Series. Yearly data on married population was calculated by linear interpolation. Sales bans coded by Bailey (2010). Divorce laws coded by Wolfers (2006), see http://bpp.wharton.upenn.edu/jwolfers/data.shtml. Female Labour Force Participation (FLFP) is from the the Current Population Survey and from the Integrated Public Use Microdata (gaps were filled by linear interpolation). Early Legal Access coded by Bailey (2006). Significant at the *** 1% level; ** 5% level, and * 10% level.

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Table 4- DYNAMIC EFFECTS OF UNILATERAL REFORM

(*Dependent variable: Annual divorces per 1,000 married individuals*)

	(1) Basic specification	(2) State-specific linear trends	(3) State-specific quadratic trends	(4) Basic specification	(5) State-specific linear trends	(6) State-specific quadratic trends	(7) State-specific quadratic trends
Sales Ban				-0.451***	-0.371**	-0.657***	-0.628***
				(0.143)	(0.150)	(0.130)	(0.134)
First 2 years	0.832***	0.630*	0.486*	0.863***	0.651*	0.516*	0.496*
	(0.319)	(0.346)	(0.283)	(0.323)	(0.349)	(0.288)	(0.292)
Years 3-4	0.519**	0.286	0.137	0.533**	0.291	0.141	0.150
	(0.209)	(0.259)	(0.224)	(0.209)	(0.260)	(0.228)	(0.238)
Years 5-6	0.424**	0.171	0.041	0.438**	0.175	0.044	0.136
	(0.206)	(0.268)	(0.229)	(0.204)	(0.268)	(0.228)	(0.254)
Years 7-8	0.581***	0.316	0.237	0.595***	0.318	0.241	0.392
	(0.188)	(0.278)	(0.236)	(0.183)	(0.277)	(0.229)	(0.284)
Years 9-10	-0.070	-0.341	-0.340	-0.056	-0.340	-0.334	-0.214
	(0.161)	(0.296)	(0.267)	(0.157)	(0.296)	(0.262)	(0.366)
Years 11-12	-0.546***	-0.819**	-0.713**	-0.532***	-0.821**	-0.705**	-0.525
	(0.174)	(0.327)	(0.296)	(0.169)	(0.326)	(0.287)	(0.355)
Years 13-14	-0.821***	-1.093***	-0.869***	-0.806***	-1.096***	-0.855***	-0.655*
	(0.165)	(0.342)	(0.312)	(0.159)	(0.341)	(0.304)	(0.395)
Years 15	-0.958***	-1.165***	-0.617*	-0.940***	-1.173***	-0.593	-0.369
Onwards	(0.139)	(0.383)	(0.371)	(0.139)	(0.384)	(0.364)	(0.476)
Controls							
Years Joint Custody	No	No	No	No	No	No	Yes
CSE variables	No	No	No	No	No	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State * time	No	Yes	Yes	No	Yes	Yes	Yes
State * timesq	No	No	Yes	No	No	Yes	Yes
Observations	1413	1413	1413	1413	1413	1413	1361
R-squared[2]	0.937	0.953	0.965	0.937	0.953	0.965	0.969

Notes: Estimated using state population weights (equal to the denominator of the dependent variable). Robust standard errors in parentheses. The dependent variable is defined as the annual number of divorces per 1,000 of married population. Data on annual divorces come from the Vital Statistics of the United States and the information on total married population was obtained from the Integrated Public Use Microdata Series. Yearly data on married population was calculated by linear interpolation. Sales bans coded by Bailey (2010). Divorce laws coded by Wolfers (2006), see http://bpp.wharton.upenn.edu/jwolfers/data.shtml. Joint Custody laws were coded by Leo (2008). Data on CSE come from González-Val and Marcén (2011). Significant at the *** 1% level; ** 5% level, and * 10% level.

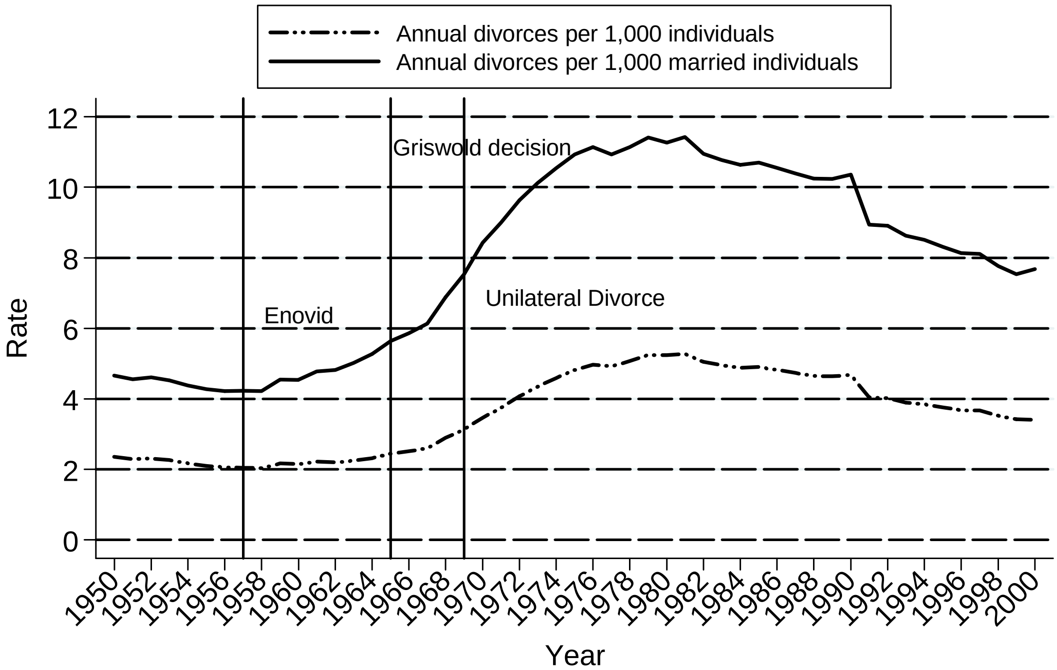
Table 5- DYNAMIC EFFECTS OF UNILATERAL REFORM CONSIDERING ALL STATES

(*Dependent variable: Annual divorces per 1,000 married individuals*)

	(1)	(2)
	State-specific quadratic trends	State-specific quadratic trends
Sales Ban		-0.195*
		(0.100)
First 2 years	0.684***	0.700***
	(0.225)	(0.225)
Years 3-4	0.657***	0.661***
	(0.184)	(0.185)
Years 5-6	0.781***	0.779***
	(0.190)	(0.191)
Years 7-8	1.073***	1.069***
	(0.217)	(0.218)
Years 9-10	0.877***	0.871***
	(0.256)	(0.257)
Years 11-12	0.660**	0.659**
	(0.264)	(0.264)
Years 13-14	0.588**	0.589**
	(0.284)	(0.283)
Years 15 Onwards	0.876**	0.880**
	(0.345)	(0.344)
Controls		
Years Joint Custody	Yes	Yes
CSE variables	Yes	Yes
Year FE	Yes	Yes
State FE	Yes	Yes
State * time	Yes	Yes
State * timesq	Yes	Yes
Observations	2226	2226
R-squared	0.968	0.968

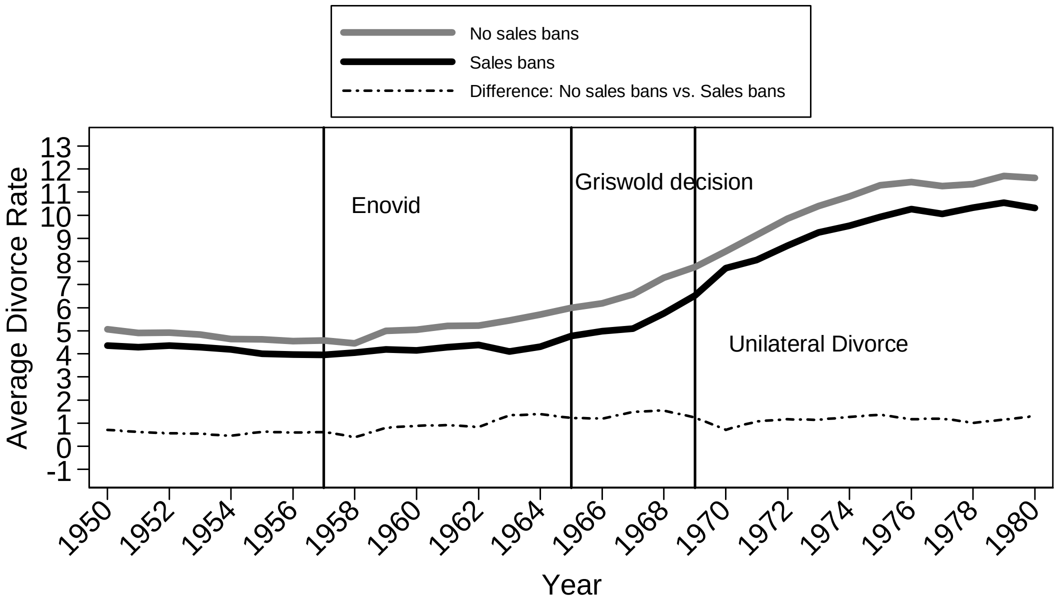
Notes: Estimated using state population weights (equal to the denominator of the dependent variable). Robust standard errors in parentheses. The dependent variable is defined as the annual number of divorces per 1,000 of married population. Data on annual divorces come from the Vital Statistics of the United States and the information on total married population was obtained from the Integrated Public Use Microdata Series. Yearly data on married population was calculated by linear interpolation. Sales bans coded by Bailey (2010). Divorce laws coded by Wolfers (2006), see http://bpp.wharton.upenn.edu/jwolfers/data.shtml. Joint Custody laws were coded by Leo (2008). Data on CSE come from González-Val and Marcén (2011). Significant at the *** 1% level; ** 5% level, and * 10% level.

Figure 1: Divorce Rate in the U.S. (1950-2000)



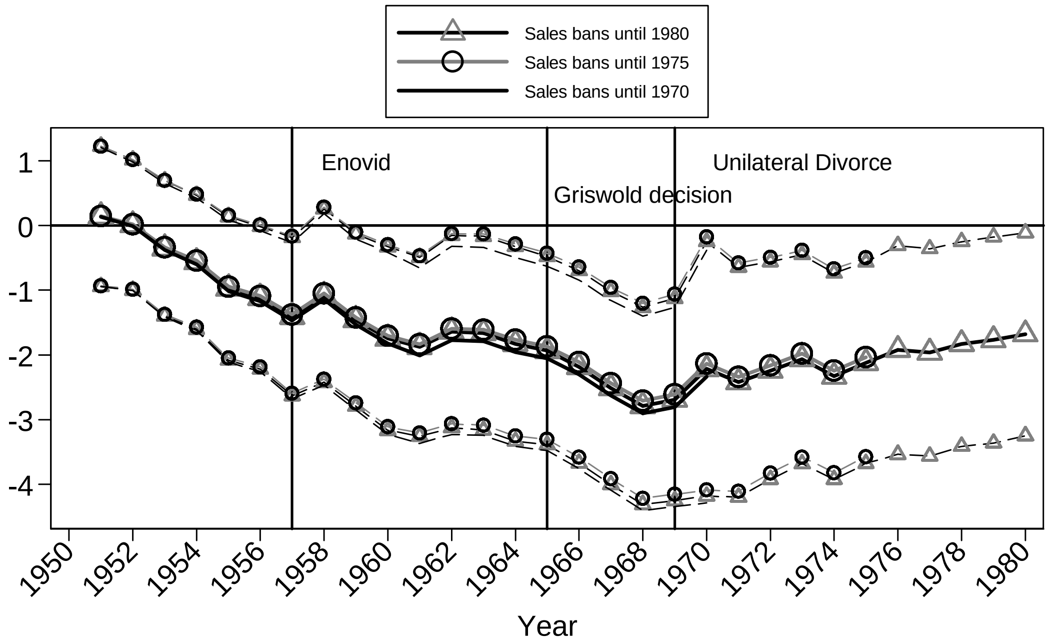
Source: Vital Statistics of the United States and Integrated Public Use Microdata Series.

Figure 2:
Divorce Rate by Comstock Laws
Related to Contraception



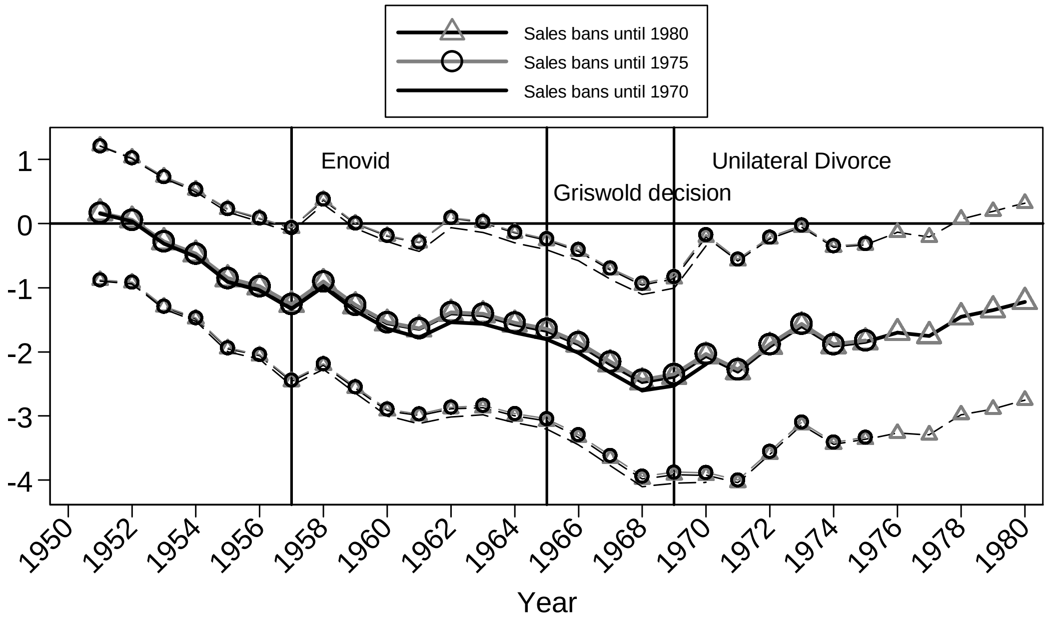
Divorce rate: Annual divorces per 1,000 married individuals

Figure 3: Differential evolution of divorce rates in states with sales bans



Note: With state-specific linear and quadratic trends and state and year fixed effects.

Figure 4: Differential evolution of divorce rates
in states with sales bans



Note: Including FLFP, ELA and UD as controls. With state-specific linear and quadratic trends and state and year fixed effects.

Appendix A

Table A- THE EFFECT OF SALES BANS ON DIVORCE USING CDR
(*Dependent variable: Annual divorces per 1,000 individuals*)

	(1)	(2)	(3)
	Basic specification	State-specific linear trends	State-specific quadratic trends
Sales Ban	-0.197***	-0.176***	-0.303***
	(0.059)	(0.063)	(0.058)
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
State * time	No	Yes	Yes
State * timesq	No	No	Yes
Observations	1413	1413	1413
R-squared	0.941	0.964	0.975

Notes: Estimated using state population weights. Robust standard errors in parentheses. Divorce rate data and population weights are from the Vital Statistics of the United States and from Wolfers (2006), http://bpp.wharton.upenn.edu/jwolfers/data.shtml. Sales bans coded by Bailey (2010). Significant at the *** 1% level; ** 5% level, and * 10% level.

Appendix B

Table B- THE EFFECT OF SALES BANS ON DIVORCE CONSIDERING THAT SALES BAN WAS REPEALED IN 1965, 1967, 1975, AND 1980
(*Dependent variable: Annual divorces per 1,000 married individuals*)

	(1)	(2)	(3)	(4)
Sales Ban	-0.449***	-0.584***	-0.564***	-0.562***
	(0.154)	(0.139)	(0.137)	(0.143)
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
State * time	Yes	Yes	Yes	Yes
State * timesq	Yes	Yes	Yes	Yes
Observations	1413	1413	1413	1413
R-squared	0.964	0.964	0.964	0.964

Notes: Estimated using state population weights (equal to the denominator of the dependent variable). Robust standard errors in parentheses. The dependent variable is defined as the annual number of divorces per 1,000 of married population. Data on annual divorces come from the Vital Statistics of the United States and the information on total married population was obtained from the Integrated Public Use Microdata Series. Yearly data on married population was calculated by linear interpolation. Sales bans coded by Bailey (2010). Sales bans variable takes the value 1 when a state has sales bans and 0 when they do not. In columns (1) to (4), it is considered that all sales bans were repealed them in 1965, 1967, 1975, 1980, respectively. Significant at the *** 1% level; ** 5% level, and * 10% level.