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12. August 2011

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Abstract:

This paper explores the response of the divorce rate to law reforms introducing unilateral divorce after controlling for law reforms concerning the aftermath of divorce, which are omitted from most previous studies. We introduce two main policy changes that have swept the US since the late 1970s: the approval of the joint custody regime and the Child Support Enforcement program. Because those reforms affect divorce decisions by counteracting the reallocation of property rights generated by the unilateral divorce procedure and by increasing the expected financial costs of divorce, it is arguable that their omissions might obscure the impact of unilateral divorce reforms on divorce rates. After allowing for changes in laws concerning the aftermath of divorce, we find that the positive impact of unilateral divorce reforms on divorce rates does not vanish over time, suggesting that the Coase theorem may not apply to changes in divorce laws. Supplemental analysis, developed to examine the frequency of permanent shocks in US divorce rates, indicates that the positive permanent changes in divorce rates can be associated with the implementation of unilateral divorce reforms and that the negative permanent changes can be related to the law reforms concerning living arrangements in the aftermath of divorce. This seems to confirm the important role of these policies in the evolution of divorce rates.

Keywords: Divorce rate, child support, joint custody, unilateral divorce, unit root, structural break.

JEL: C12, C22, J12, J18, K36

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I. Introduction

In an article in the *American Economic Review*, Wolfers (2006) finds that reforms in the divorce law in the United States (US) during the 1960s and 1970s had a positive effect on divorce rates. Wolfers claims that this result does not back up the applicability of the Coase theorem to marital relations since divorce rates are not neutral to changes in divorce laws. [1] However, he also observed that the effect was transitory; after a decade, no effect on divorce rate could be discerned. [2] This generates doubts about the empirical evidence that does not support the predictions of efficient Coasian bargaining. To explain his puzzling results, Wolfers suggests that a situation where spousal bargaining was close enough to the efficient one – consistent with the Coasian approach – can account for the small and transitory effect estimated. In this paper, we provide an alternative explanation by presenting evidence that later reforms that introduced changes in divorce settlements may explain the diminished effect of unilateral divorce on the divorce rate.

Two primary aspects of law are relevant to divorce and both may affect divorce decisions (Fine and Fine 1994). First, there are laws that regulate how spouses obtain a divorce, and these include the unilateral divorce regime. [3] Second, there are laws that govern the living arrangements in the subsequent periods after divorce, including such matters as spousal support, child support, and child custody. [4] These are not included in Wolfers (2006) but they may have significance in the evolution of the divorce rate. [5] Although, from a theoretical point of view, it can be suggested that those changes in divorce settlements have an ambiguous effect on divorce (see Nixon 1997; Rasul 2006a; Halla 2011), previous empirical research has found that both changes in the financial obligations of parents and the introduction of joint custody negatively affect divorce rates (Nixon 1997; Brinig and Buckley 1998). [6] Thus, it is arguable that the analysis of one of those aspects of law relevant to divorce alone might in some way obscure the impact of unilateral reforms on divorce rates.

This is even more relevant in the US since while the share of the population covered by the no-fault unilateral reforms increased from the late 1960s, reaching 50 percent of the population in the early 1970s (see Figure 1), a trend of reforms occurred in the area of post-divorce child custody and child support. Empirically, it is unclear whether the dummy variables

[1] In Coasian terms, a change in divorce law only generates a redistribution of the property rights between spouses; thus, divorce reforms are not expected to affect the divorce rate (Becker 1981).

[2] Further, some of his estimates indicate that divorce rates were lower as a consequence of unilateral divorce 15 years after its implementation.

[3] Unilateral divorce does not require mutual consent and it can be granted at the request of either spouse.

[4] We do not pay attention to changes in spousal support or alimony (a court-ordered money transfer between ex-spouses for a limit period after the divorce) since only a small fraction of ex-spouses received alimony and in the period considered there were no significant changes in this issue (Beller and Graham 2003).

[5] Previous research on the effect of divorce law reforms on divorce rates also failed to account for changes in the aftermath of divorce. See Peters (1986, 1992), Allen (1992), Friedberg (1998), Gray (1998), and González and Viitanen (2009) among others.

[6] More recently, some studies have failed to find a significant effect of changes in custody laws and child support on the divorce rate (Halla 2011; Heim 2003).

included by Wolfers (2006) to capture the dynamic response of divorce only pick up the path of the adjustment of divorce rates to unilateral divorce. Wolfers observes that the effect of unilateral divorce law reforms on divorce rates had dissipated a decade after the implementation of the unilateral divorce law, which coincides with the rise in the incidence of joint custody (Figure 1). The timing of both reforms differs by at least a decade in almost all states in which those reforms were implemented (Friedberg 1998; Leo 2008). In the area of child support, the US Congress approved several laws to try to ensure child support payments. The main reforms were the Child Support Amendments of 1984, the Family Support Act of 1988, the Child Support Recovery Act of 1992, the Personal Responsibility and Work Opportunity Reconciliation Act of 1996, and the Deadbeat Parents Punishment Act in 1998 (see Sorensen and Halpern (1999) for a review of state statutes). This again tallied with the time at which a negative response of divorce rates to divorce law reforms was found in Wolfers (2006). We argue that what lies beneath Wolfers's results are two countervailing forces, which together produce the observed pattern. Thus, the initial increase and subsequent decrease in divorce rates would be the response of divorce rates to the initial changes in divorce laws followed by custody reforms and Child Support Enforcement (CSE) efforts.

Initially, we include the reforms that govern the aftermath of divorce into Wolfers's specification using data on the divorce rate from 1956 to 1988. We introduce both child custody law reforms and CSE efforts. Our results suggest that the long-run effect of divorce law reforms on the divorce rate observed by Wolfers may be the result of both unilateral reforms and changes in the aftermath of divorce. When we separate both effects, we find evidence of a persistent impact of divorce laws on divorce rates, although these results are sensitive to the inclusion of state-specific trends. This is robust to a range of alternative specifications and to the selection into marriage effect. These findings suggest that the Coase theorem cannot be applied to marital dissolution.

As an additional check that the changes in the aftermath of divorce are driving our findings, we separate the analysis into divorcing couples with and without minors in order to check whether the behavior of childless couples – the sub-population not affected by legal changes in the aftermath of divorce when they obtain a divorce – is driving our results instead of the reactions of couples with minors. We present additional evidence suggesting that the joint custody law and the reinforcement of child support predominantly affect the exit from marriages of couples with minors as opposed to changing the divorce pattern of childless couples.

Finally, since even after adding the reforms on the custody laws and child support to the analysis it is unclear whether divorce laws have a persistent effect on divorce, we explore the frequency of persistent shocks in US divorce rates by exploiting another technique, a time-series analysis.[7] The advantage of this methodology is that it lets data "speak for themselves", (see Piehl et al . 2003; Kuo 2011); this allows us to test whether and when there have been changes in divorce rates without imposing any a priori timing (such as the dates of the reforms). We analyze three possible scenarios (for a review of the literature on structural breaks, see Perron 2006). First, the divorce rate is stationary. In this scenario, the divorce rate is basically stable; after a shock, such as divorce law reforms, short-run effects on the divorce rate would be observed, but in the long-run, the divorce rate should return to its equilibrium level. In the second scenario, divorce is stationary around a process that is subject to structural breaks. In this setting, occasional shocks cause permanent changes in the equilibrium rate itself, but most shocks only cause temporary movements of the divorce rate around the equilibrium level. The third scenario consists of the divorce rate exhibiting a unit root. In this case, all shocks have permanent effects on the level of divorce.[8]

The clear result of the time-series analysis is that not all shocks have transitory effects on the divorce rate. This result is robust to a number of alternative tests. There is no single scenario to identify the behavior of the divorce rate; we find empirical evidence of stationarity around a process that is subject to structural breaks, where only a few occasional shocks have permanent effects, and of unit root, with all shocks having a permanent effect on the divorce rate. In addition, our results suggest that persistent positive changes can be associated with major changes in divorce laws and those permanent negative changes can be related to changes in custody laws and the CSE program, since the break dates and the dates of the reforms are close to each other.

The present paper is organized as follows. Section II discusses the results of Wolfers (2006). In section III and section IV, we introduce custody law reforms and CSE efforts into Wolfers's analysis. Section V includes the supplemental analysis of the frequency of permanent shocks in divorce and gives possible explanations for these changes, and Section VI concludes.

II. Replicating Wolfers

As mentioned above, Wolfers (2006) tests the dynamic response of the divorce rate to a change in the legal regime that governs how spouses divorce. To do that, he uses data on the divorce rate in each state between 1956 and 1988, as derived from the Vital Statistics of the United

[7] The time-series analysis is a technique that has been ignored in most previous work. As an exception, Marvell (1989) was the first attempt to develop a complete time-series analysis of divorce rates across the US, finding that the major impact on divorce rates of the change to no-fault laws was delayed for a year. In addition, Ellman and Lohr (1998) used an intervention analysis. In Europe, we find the works of van Poppel and de Beer (1993) for the Netherlands, and Smith (1997) for Britain. In both cases, they find evidence of permanent legal effects on divorce rates.

[8] By using statistical techniques similar to ours, studies have examined whether shocks have a permanent effect on the long-run level of most macroeconomic and financial aggregates: real gross national product (GNP), nominal GNP, and unemployment rate, among others (Nelson and Plosser 1982; Perron 1989; Zivot and Andrews 1992), on the import–GDP and export–GDP ratios (Ben-David and Papell 1997), on the purchasing power parity (Papell 1997; O'Connell 1998; Murray and Papell 2002; Papell 2002), and even on the evolution of city growth (Davis and Weinstein 2002; Bosker et al. 2008).

States. The divorce rate is defined as the annual number of divorces per thousand inhabitants in each state. Using this sample period, he is able to determine the dynamic response of divorce to the changes in divorce laws that occurred in the US from the late 1960s, once the pre-existing state-specific trends are identified. He estimates:

$$DR_{s,t} = \sum_{k\geq 1} \beta_k UD_{s,t,k} + \sum_s StateFE_s + \sum_t TimeFE_t + \left[\sum_s StateFE_s \cdot Time_t + \sum_s StateFE_s \cdot Time_t^{\ 2}\right] + \varepsilon_{s,t} \tag{1}$$

where $DR_{s,t}$ is the divorce rate in state s in year t and the variable $UD_{s,t,k}$ represents a series of dummy variables equal to one when state s has a unilateral divorce regime effective in year t for k periods. These dummy variables are supposed to capture the entire dynamic response of divorce to the new legal regime. The year fixed effects control for the unobserved national attributes that affect the divorce rate. The state characteristics unchanging over time that may influence the divorce rate are picked up by the state fixed effects. Finally, the state-specific time trends identify pre-existing trends in the divorce rate (Wolfers 2006).

Panel A of Table 1 replicates Wolfers's results where equation (1) is estimated using population-weighted least squares. In the specification of Column (1), which only includes state and year fixed effects, the dynamic estimates show that the positive effect on divorce rates following the adoption of unilateral divorce seems to fade over the subsequent decade. Coefficients then become negative and statistically significant, so the divorce rate declines as a result of the adoption of the unilateral divorce law. As Wolfers reflects, long-run estimates do not seem to be robust; when more controls are added, the coefficients become less negative or even positive but statistically insignificant, see Columns (2) and (3) which include state-specific time trends and quadratic state-specific time trends, respectively. All in all, Wolfers concludes that divorce law reforms in the US had an effect on the divorce rate, but the impact was transitory.

The dynamic response of divorce rates certainly seems at odds with the motivating theoretical Coase theorem approach. The reaction after a little more than a decade is hard to interpret. It is difficult to establish a clear causal link between the liberalization of the divorce law and the fall in divorce rates since the 1980s, since correlation does not automatically imply causation. In this paper, we investigate a potential explanation for these puzzling findings. We argue that dummy variables added to pick up the dynamic response of divorce may be capturing not only the reaction of divorce rates to laws that regulate how to obtain a divorce, but also the responses of those divorce rates to changes in laws that govern the aftermath of divorce, the implementation of a joint custody regime, and CSE efforts.

III. Joint Custody Regime

Why does reform in custody law matter in the analysis of divorce rates? The move from a sole custody regime to a setting with the possibility of joint custody may mean a return to a regime in which mutual consent is necessary. Under a sole custody regime, women have traditionally been responsible for the child, whereas under a joint custody regime, decisions affecting the child must be jointly made by parents, requiring discussion and collaboration between them (Bartlett and Stack 1991).[9] This necessity of cooperation and mutual consent in child custody may be counteracting the reassignment of property rights generated by the approval of the unilateral divorce regime.[10] Although the unilateral divorce regime transfers the right to divorce to the spouse most wanting a divorce and, as a consequence, it is the party who wants to continue to be married who has to compensate the spouse who wishes to leave, under the joint custody regime the requirement of cooperation and mutual consent produces a change in the direction of the compensation. Thus, it is the spouse who wants to divorce who has to compensate the other party to mutual consent in the custody of their child even if disparities in the value placed by the parties on custody exist. In fact, the greater the bargaining advantage given to the party who values the custody less highly, the more difficult mutual consent will be (Bartlett and Stack 1991).

In Coasian terms, both reforms consist of the reassignment of property rights between spouses, which should not affect the divorce rate under assumptions of full transferability, perfect information, and no transaction costs. However, what is observed by simply comparing the evolution of the divorce rate across states and the changes in laws related to divorce calls into question the applicability of the Coase theorem to marital dissolution.

Although 28 states passed a no-fault unilateral system between 1968 and 1977, from 1979 what swept the US was the introduction of a joint custody regime (Folberg 1991). In 1988, approximately 37 states had some form of joint custody statute.[11] This second wave of reforms

[9] We do not discern here between the various forms of joint custody such as “joint legal custody” (both parents share the right and the obligation to make major decisions about their child’s upbringing on issues such as religion, health, and education) and “joint physical custody” (the child spends a significant amount of time with each parent), or between the way in which parents achieve joint custody (parental agreement or award by a judge). We consider any kind of joint custody statute approved in the period considered since any of these systems requires the involvement of both parents.

[10] We do not aim to study how gender disparities introduced by the new law reforms affect the evolution of the divorce rate (for a review of the effect of joint custody on the bargaining positions of spouses, see Allen et al. (2011), Brinig and Allen (2000), Jacob (1988), Halla (2011), Nunley and Seals (2011), and Seltzer (1991)). It is important to note that although laws that regulate how to get a divorce are gender neutral; the traditional sole custody regime could be distorting this neutrality by increasing the power of the custodian parent, normally the mother, creating a “winner/loser” situation (Folberg 1991). Under a sole custody regime, it is the man who has to compensate his spouse to stay married and to see their child if it is the woman who wants to divorce. When the party who wants to divorce is the man, he also has to compensate his wife to be able to stay with his child, and so, for men it is costly to get a divorce under both the unilateral divorce and sole custody regimes. The implementation of a joint custody regime may correct this bias by increasing men’s rights. In this way, the expected utility of divorce increases for men, who traditionally had not been responsible for the child, and decreases for women, see Elkin (1991). In this setting, it is the husband, if he wants to divorce, who does not have to compensate his wife for having his child with him and for his wife it is going to be more costly to stay married. By contrast, if it is the wife who wants to divorce, she is not going to receive any compensation from her partner to be part of the parenting, she will have to compensate him to mutual consent in the custody of their child. Regardless of these gender disparities, the necessity of cooperation and mutual consent in the custody of children may lead to a reallocation of property rights.

[11] In 1957, North Carolina was the first state to pass a statute allowing for the joint custody of children after the dissolution of marriage if it was in the best interests of the child. Twenty-two years later, California declared a public

seems to have affected the divorce rates of those states that also introduced unilateral reforms, as can be seen in Figure 2. This figure represents the evolution of the average divorce rate across states that introduced both unilateral divorce and (the possibility of) joint custody (24 states), those which passed unilateral reforms (seven states), those with only joint custody reforms (14 states), and those states that did not change either divorce law (six states).[12] The long-dashed and short-dashed lines show the evolution of the difference in the average divorce rate between those states that introduced any reforms, unilateral reforms, joint custody reforms or both, with those that did not pass a reform. These lines allow us to compare the different evolution of the average divorce rate by the states that approved different aspects of the law on divorce. It is clearly observed that the decline in the average divorce rate occurs in those states that introduced both reforms; hence it seems that child custody law reforms neutralized the effect of unilateral divorce on divorce rates. Those states that only passed unilateral reforms maintained higher divorce rates from at least the mid-1950s, around two divorces per 1,000 inhabitants per year more on average, until the mid-1990s with respect to those states that did not pass any reforms. This simple comparison suggests one possible explanation to the dynamic response of divorce rates observed by Wolfers: changes in custody law.

The divorce rates of those states that only passed a joint custody regime also seem to fall with respect to the divorce rates of those states that do not introduce any reforms (see Figure 2). From a theoretical point of view, the fall in the divorce rates of those states that only introduced custody reforms may be because of an increase in the cost of divorce. As Morrow (1991) remarks, when parents share physical custody after divorce, the total costs are further increased since some of the major expenses are duplicated.[13] By contrast, the divorce rate can also decline when investment in child quality increases as a result of the introduction of the joint custody regime (Rasul 2006a).

Whether a joint custody regime affects the divorce rate is an empirical question that has received hardly any attention in research. The first attempt to test this relationship was by Brinig and Buckley (1998), who find a negative effect of joint custody laws on divorce rates. This result was rebutted, more recently, by Halla (2011).[14] He does not find convincing evidence that the joint custody regime significantly affects divorce rates when adding a set of dummies for the joint custody law à la Wolfers:

$$DR_{s,t} = \sum_{k\geq 1}\beta_k UD_{s,t,k} + \sum_{r\geq 1}\alpha_r JC_{s,t,r} + \sum_{s} StateFE_s + \sum_{t} TimeFE_t + \varepsilon_{s,t}. \qquad (2)$$

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policy of encouraging parents to continue to share their parenting rights and responsibilities after divorce. Many of the statutes that were approved later were inspired by the early Californian legislation (Jacob 1988).

[12] Unilateral divorce laws are coded from Wolfers (2006) and the joint custody regime is from Leo (2008) and Folberg (1991).

[13] The introduction of a joint custody regime may also reduce the costs that would be incurred in the sole custody regime because sole custody resolutions tend to exacerbate parental differences and cause predictable post-divorce disputes, which clearly generate greater costs of divorce (Halla and Hölzl 2007; Folberg 1991). This will increase the divorce rate.

[14] Halla (2011) uses data on divorce from 1969 to 2003.

Rather than the dynamic response of divorce rates, α_r, to the introduction of a joint custody regime, $JC_{s,t,r}$, we are interested in how divorce rates adjust to unilateral divorce once the change in custody law has been controlled for. Panel B of Table 1 shows the results from running equation (2) on the same unbalanced panel of divorce rates that we used when we ran equation (1). The sign of the dynamic effects of divorce law reforms on divorce rates is consistent with previous findings in all three specifications, but the magnitudes of the dynamic responses considerably differ from those obtained in Wolfers's analysis. Specifically, the decline in divorce rates because of the unilateral divorce reforms is softened in specifications (1) and (2), where state and year fixed effects and state-specific time trends, respectively, are added. In addition, the conclusion that reforms have no significant effect after a decade is not robust when the dynamic response to custody law reforms is included. After controlling for quadratic state-specific time trends, it is observed that the long-run effects are positive and statistically significant.[15] Therefore, these results generate doubts about what is being captured by the dummy variables included in Panel A of Table 1.

Alternatively, we can test whether the divorce rate really decreases after the implementation of the unilateral divorce reforms just by focusing on those states that only passed such unilateral divorce reforms. To study the dynamic response of divorce rates to the unilateral divorce reforms in those states that only adopted unilateral divorce, we add to the analysis the interactions between unilateral divorce dummies and joint custody dummies. We would expect to observe no change in the sign of the coefficients capturing the dynamic response of divorce rate to unilateral divorce if the β_k coefficients of equation (1) only measure the effect of unilateral divorce. To formalize these ideas, consider the following equation:

$$DR_{s,t} = \sum_{k\geq 1} \beta_k UD_{s,t,k} + \sum_{r\geq 1} \alpha_r JC_{s,t,r} + \sum_{k\geq 1}\sum_{r\geq 1} \gamma_{k,r} UD_{s,t,k} * JC_{s,t,r} + \sum_{s} StateFE_s + \sum_{t} TimeFE_t + \varepsilon_{s,t} \tag{3}$$

where $DR_{s,t}$ is the divorce rate in state s in year t, $UD_{s,t,k}$ represents a series of binary variables equal to one if a state adopted unilateral divorce k years ago in year t, and $JC_{s,t,r}$ is a series of dummies equal to one when a state introduced a joint custody regime r years ago in year t. β_k coefficients are now measuring the dynamic response of divorce rates to the unilateral divorce reforms in those states that only adopted unilateral divorce. If the impact of the introduction of a unilateral divorce system is reversed as time goes by, we may expect that the rise in the divorce rate produced by the adoption of unilateral divorce should be inverted, so

[15] Of course, these results can be because of omitted variable bias. If the omitted time-varying factors are correlated with the law reforms, then our estimates without state-specific time trends will be biased. Thus, as Friedberg (1998) claims, the inclusion of state-specific time trends to address that problem would be necessary. She explains in detail the necessity of the introduction of state-specific time trends to control for unobserved factors (e.g. social attitudes, religious beliefs, and family size) that influence divorce and that may vary within a state over time.

β_k in the subsequent periods after the adoption of unilateral divorce should be positive, but then it should turn negative. By contrast, if divorce rates do not decrease as a result of the adoption of unilateral divorce then β_k should be always positive or non-significant.[16]

Table 2 presents the regression results of the β_k coefficients in equation (3), but the full set of control variables and the dynamic effects of joint custody laws are included in the models. The results suggest that divorce rates rose after the adoption of unilateral divorce laws. The dynamic response after a decade is similar to that described by Wolfers (2006) in specifications (1) and (2); the effect of the introduction of unilateral divorce was reversed over the ensuing decade, although there are differences in the magnitude of the effect.

An attractive feature of this approach is that it can explain some of the potential sources of bias in Wolfers's dynamic analysis. By comparing estimates in Table 2 with those in Panel A of Table 1, it is observed that the exclusion of controls for the adoption of joint custody laws leads to a greater negative impact of the unilateral divorce reforms on divorce rates. When controls for state-specific quadratic trends are added, the rise in divorce rates following the implementation of unilateral reforms is persistent. The specification in Column 3 of Table 2 shows that the long-run effects are positive and statistically significant, suggesting that unilateral divorce has a permanent effect on divorce rates. The same is seen in specification (3) in Panel B of Table 1, although the impact is greater for those states that introduced unilateral divorce systems. Again, our results contribute to explaining what may be behind the results obtained with the model implemented by Wolfers to analyze the dynamic response of divorce rates to unilateral divorce reforms.

Couples with and without Children

It is complicated to interpret the differences between our estimates and Wolfers's results because the divorce rate includes a sub-population that is not affected by the joint custody reforms. The necessity of mutual consent required by the joint custody reforms is limited to couples with minors, but the divorce rate includes both couples with children and couples without children. This is problematic since the behavior of the sub-population not affected by the custody law reforms could be driving our results instead of the reaction of couples with minors to custody law reforms.

[16] Although we are not interested in the effect of joint custody on the divorce rate, α_r, the dynamic response of divorce rates to the custody laws would be expected to be negative if the costs of divorce increase for those states that introduced custody law reforms. By contrast, for those states affected by both waves of reforms, we might expect $\alpha_r + \gamma_{k,r}$ to be negative, at least until reversing the positive effect of the unilateral reforms on divorce rate, when the increase in the divorce rate following unilateral divorce reforms is reversed because of the interruption of joint custody reforms. In addition, $\beta_k + \gamma_{k,r}$ is not expected to turn negative since the effect of the unilateral reforms would be cancelled by the joint custody regime. $\beta_k + \gamma_{k,r}$ captures the dynamic effect of unilateral reforms for those states that introduced both unilateral divorce and joint custody reforms.

It is certainly difficult, if not impossible, for researchers to test the effect of the changes in divorce law reforms on all the states considered in the analysis because of the scarcity of data. Detailed information on the number of divorces by number of children involved is publicly available in the Vital Statistics of the United States for each state belonging to the divorce registration area until 1990. Figure 3 separately shows the evolution of the average divorce rate for couples with and without children at the time of divorce for those states that implemented only unilateral divorce, only joint custody reforms, or both reforms.[17] Clearly, we observe higher divorce rates for couples with children (red and black lines). As expected, the divorce rate of couples with children considerably decreased in those states that introduced both a unilateral divorce system and a joint custody law, after the introduction of the new custody system (black line), compared with the divorce rate of couples with children in those states that only introduced unilateral divorce reforms (red line). The divorce rate of couples without children (blue line) in those states that approved both reforms did not decrease from the early 1980s when the joint custody law was adopted by most states.[18] This suggests that our results might be driven by a change in the divorce rate of couples with children in those states that introduced joint custody laws as opposed to a decreasing trend in the divorce rate of those childless couples.

To probe this further, we reran equation (1) and equation (3) using as dependent variables the divorce rates among childless couples and among couples with children, with data for all states belonging to the divorce registration area.[19] In these regressions, we would not expect to find any effect of custody law reforms on the divorce rates of childless couples since joint custody reforms would not be an issue in the divorce decisions of such couples. Thus, we would not expect changes in the estimates of the dynamic response of divorce rates to unilateral reforms when we run equations (1) and (3) for couples without minors.

Figure 4 shows the results graphically. As predicted, we observe differences in the coefficients capturing the response of the divorce rate to the unilateral divorce reforms for couples with children with that being remarkable when quadratic state-specific time trends are added. For the case of childless couples, the coefficients slightly differ when joint custody reforms are included, but again, those differences are almost insignificant when quadratic state-specific time trends are included.[20]

[17] The number of states varied substantially, from 18 states in 1960 to 32 states in 1990. For 18 states, there are no data available and in the case of 15 states some observations are missing.

[18] The fall in the average divorce rate of childless couples (blue line) takes place two years prior to the approval of the first legislation on joint custody in 1979. Thus, we would not expect that this change was determined by the custody reforms.

[19] We also ran the rest of the analyses using only data for those states belonging to the divorce registration area and the results were quite similar. However, we prefer to use data for all states to make our findings comparable with previous works.

[20] We acknowledge that these results should be taken with caution since selection into marriage may matter in this framework. However, as shown below, after separating the sample among those married before and after the unilateral divorce reforms, the results do not change.

Because we would not expect the joint custody reforms to have any effect on the divorce rates of childless couples, the little differences with respect to that prediction observed in Figure 4 may indicate that the β_k coefficients of equation (1) may be capturing second-order effects. The change in the custody law may produce two different effects in the behavior of couples without children. This can lead to a decrease in the number of divorces since there are fewer opportunities outside marriage to find someone to remarry because of the increase in the married population (marriage rates having increased as a result of the adoption of new custody laws; Halla 2011). Further, an increase in the married population implies an increase in the population at risk of divorce; thus, the divorce rate is more likely to rise in subsequent periods. In Figure 4, we observe an increase in the coefficients of the unilateral divorce reforms when controls for the joint custody reforms and state-specific trends are added, with this being 10 years after the approval of unilateral reforms. This suggests that those coefficients might be capturing the second-order effects of joint custody on marriage rather than the unilateral divorce reforms alone. We then detect a decrease in the effect of the unilateral divorce reforms when the same controls are added. Again, this could be because the coefficients are capturing the second-order effects of custody reforms in addition to or instead of unilateral divorce.

The decline in the divorce rate for couples with children in those states that introduced joint custody laws can also be attributed to other factors, such as an increase in the age of individuals that divorce, since older individuals are less likely to have young children or a decline in the number of children in married-couple families. As can be seen in Figure 5, the number of children that were involved in divorce slightly declined in the 1980s, coinciding with the period of the implementation of joint custody laws (data from the Vital Statistics of the United States). However, the fact that the rate of children involved in divorce per 1,000 children under 18 years of age also slightly declined from 1981 may reinforce the idea that what is declining is the number of divorces of couples with children.

The interpretation of the results presented in this and the next sections may also be difficult because there could be other determinants of divorce, which may vary by state but have little to do with the changes in divorce laws. Other determinants of divorce that have been suggested are economic growth (South 1985), price stability (Nunley 2009), unemployment (Jensen and Smith 1990), female labor force participation (Allen 1998), public transfers, tax laws, and welfare reforms (Bitler et al. 2004; Tjøtta and Vaage 2008), property distribution within marriage (Gray 1998), fertility behavior (Svarer and Verner 2008), religiosity (Vaaler et al. 2009), television (Chong and La Ferrara 2009), or even culture (Furtado et al. 2010). Not controlling for these demographic and economic characteristics would be problematic if the factors associated with a rising divorce rate were more likely in states that did not introduce divorce reforms, and this might lead to a bias in the estimates as the dynamic response to changes in divorce laws might be capturing differences in the evolution of these characteristics by state rather than the effect of the reforms. Of course, the inclusion of these omitted factors may bias the estimates of the dynamic response to divorce law reforms when they are correlated with the divorce law reforms. For instance, changes in divorce laws have been found to affect marriage rates (Halla 2011), which affects the population at risk of divorce, and to reduce fertility rates (Drewianka 2008). The introduction of measures of economic performance in the estimations, such as female labor force participation and female earnings, or other demographic variables such as fertility rates, may also produce problems of endogeneity since many of these measures of economic performance have not been truly exogenous (Allen 2002). Causality between the divorce rate and these variables may run in both directions (Becker 1981); for example, Ressler and Waters (2000) find that the divorce rate may be influenced by and may itself influence female earnings. To make our results comparable with previous analyses we do not introduce these socio-economic variables into the analysis.[21]

IV. Child Support Enforcement

The analysis presented in the previous subsection left out the third wave of transforming aspects of law relevant to divorce that has occurred since the mid-1970s when the US Congress implemented several reforms aimed at enforcing support obligations to prevent poverty among children and to reduce welfare costs. Marking the beginning of what would become an important period in the development of child support legislation, it established the Federal Child Support Enforcement Program as Title IV-D of the Social Security Act in 1975.[22] This law created a separate division, the federal Office of Child Support Enforcement (OCSE), to oversee the operation of a CSE program and required each state to establish a CSE agency to be responsible for that program. Subsequent reforms in 1984, the Family Support Act in 1988, the Child Support Recovery Act of 1992, the Personal Responsibility and Work Opportunity Reconciliation Act of 1996, and the Deadbeat Parents Punishment Act in 1998 required all states revise and expand CSE services and techniques.[23]

The CSE amendments of 1984 required every state's Child Support Enforcement Agency (CSEA) develop mandatory procedures for withholding income as well as expedited processes for establishing and enforcing support orders (such as income tax refund interceptions and property liens), without having to request court intervention. The Family Support Act of 1988 requires every state implement various procedures for immediate and mandatory wage

[21] Just to provide additional evidence that our results are not conducted by those omitted variables, as suggested by one referee, we rerun all the analysis adding controls for female labor force participation using data from the Current Population Survey and from the Integrated Public Use Microdata (gaps were filled by linear interpolation). The results do not vary. Because of endogeneity concerns, we prefer not to include those tables in the analysis but those are available upon request.

[22] Prior to 1975, child support policy was dictated largely by family law in each state and enforced by the court system. To obtain a child support order, to enforce an existing order that was not being paid, or to establish legal paternity, a custodial parent had to go to court.

[23] See Garfinkel et al. (1998), Lerman (1993), and Sorensen and Hill (2004) for a review of child support policies in the US.

withholding for all support orders being enforced by every state's CSEA. By 1994, states were required to provide for the immediate withholding of wages for all support orders (regardless of whether IV-D services were used or payments were in arrears). The Child Support Recovery Act of 1992 imposed a federal criminal penalty for the willful failure to pay a past due child support obligation to a child living in another state and that has remained unpaid for more than one year or is greater than $5,000. Failure to pay was punishable by up to six months imprisonment and/or a fine. Second and subsequent violations were punishable by two years imprisonment and/or a fine. Upon the implementation of these laws, child support collections increased from $2.4 billion in 1984 to $8 billion in 1992. The number of absent parents located to establish and enforce or modify an order rose from almost 900,000 in 1984 to 3.7 million in 1992, and the number of paternities established also increased from nearly 220,000 in 1984 to 520,000 in 1992 (OCSE Annual Reports to Congress).

The CSE was also a top priority during the Clinton administration. Child support collections doubled from $9 billion in 1993 to nearly $18 billion in 2000. The number of absent parents located to establish and enforce or modify an order also doubled from 3.7 million in 1992 to nearly 7 million in 1998. On paternity establishment, nearly 900,000 paternities were established in 2000, almost twice as many as in 1992. The Clinton administration also passed the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) of 1996 and the Deadbeat Parents Punishment Act in 1998. The PRWORA introduced significant revisions in child support legislation to improve the functioning of the CSE program. These changes included requiring states to increase the percentage of fathers identified, establishing an integrated network linking all states to collect information about the locations and assets of parents, requiring states to implement more enforcement techniques such as withholding wages, seizing assets, and even revoking the driving and professional licenses of those parents who owed child support, and also allowed for the creation of the new hires database, which requires all employers report information about newly hired employees. The Deadbeat Parents Punishment Act of 1998 established two new categories of felony offenses punishable by a fine and up to two years in prison. The offenses were traveling interstate or overseas with the intent to evade a support obligation if the obligation has remained unpaid for more than one year or is greater than $5,000; and willfully failing to pay a child support obligation regarding a child residing in another state if the obligation has remained unpaid for more than two years or is greater than $10,000. It is arguable that all these policies that aimed at ensuring that child support was paid might have an effect on the evolution of divorce rates.

Since there was more than one child on average involved in each divorce from the mid-1950s until 1976, and almost one child on average from 1976 onwards (Figure 5), the incorporation of these reforms seems to be necessary to estimate precisely the effect of no-fault unilateral reforms on divorce rates. Additionally, although changes in joint custody laws can affect divorce rates, the percentage of joint custody agreements is not quite significant. By 1990, the wife was awarded custody of the children in almost three-quarters of the divorces with children involved. Joint custody was the second most common arrangement, at 16 percent (Monthly Vital Statistics Report in 1990). The largest percentage of children living with one parent was living with their mother, and this fact did not considerably change in the period considered (see Figure 6). Therefore, changes in the financial obligations of non-custodial parents, i.e., child support, might play a more important role in divorce.[24]

It is possible that what is being captured by the dynamic response of divorce rates to divorce law reforms is the application of CSE programs. To pick up the effect of CSE on divorce, we ran equation (3) by including several measured of CSE efforts. An alternative strategy would be the introduction of the legislative history of reforms that enforce child support. However, this might fail to account for the effects of these reforms on divorce rates, since by using this strategy of identification we are not measuring the effectiveness of the application of those reforms. Federal laws establish the guidelines under which each state CSE agency must operate, but there is considerable variation in the manner in which the laws are implemented since CSE efforts are executed by state authorities (for a review of state statutes, see Sorensen and Halpern (1999)).[25] This is relevant in the analysis of the response of divorce rates to divorce law reforms when less restrictive divorce laws are associated with greater state interest in CSE. Couples that live in states that passed joint custody laws or where they cannot unilaterally divorce might fail less in their child support obligations because of the necessity of mutual consent in child custody. Therefore, those states that only introduced unilateral divorce regimes would need to be stricter at putting CSE into effect to achieve their objectives of reducing child poverty and welfare costs.

We use state-level administrative data provided by the OCSE.[26] The status of the application of the CSE in all states considered in the analysis is reported yearly from fiscal year

[24] From a theoretical point of view, the effect of the increase in the CSE efforts on divorce is ambiguous. For men, normally the absent parent, it may raise the expected financial responsibility in divorce, and thus it increases the costs of divorce. For women, those in charge of children after divorce, the increase in child support increases the mother's expected income after marriage, which may reduce the costs of divorce for these women. Thus, two opposite effects might be operating (Nixon 1997).

[25] It can also be argued that the use of the legislative history of CSE reforms is not useful to capture the effect of CSE since each reform was implemented by all states only a few years after the approval of the federal laws and those federal laws were passed very close to each other. Thus, by using the variation in the timing of the reforms, there is not enough gap between the reforms to clearly identify the effect of CSE reforms.

[26] Although OCSE data include detailed information on CSE programs, parents not utilizing OCSE services are not included in its publications; see Guyer et al. (1996). This might affect our estimates if the greater presence of OCSE non-users is associated with less restrictive divorce laws. Those states operating under unilateral divorce laws may need to be more stringent in applying CSE programs since parents can fail more in their child support obligations. This can lead to an increase in the number of parents carrying out their child support payments, even if those parents do not utilize the OCSE services because the threat of making them pay their obligations is credible. Unfortunately, no dataset contains information on both parents using OCSE and non-users. Thus, our estimated effects of CSE efforts on divorce rate will still partly pick up the unilateral divorce effect in addition to the CSE efforts.

1977 by the OCSE.[27] Four different variables are used to represent the effectiveness of the CSE program. Similar to Nixon (1997) and Heim (2003), we analyze the effect of enforcing child support orders and increasing collections by using the *collection rate* variable, which is defined as the percentage of CSE cases in which a collection was made by obligation, and by including the *average collections*, which is calculated as the dollars collected per CSE case divided by state per capita GDP to adjust for the fact that richer states will have higher amounts collected regardless of CSE efforts. Following Heim (2003), we also include two more variables to control for the differential effects of CSE policies. We use a *paternity rate*, measured as the number of paternities established in a given year per 1,000 inhabitants, and a *location rate*, defined as the number of absent parents located in a given year per 1,000 inhabitants.[28] A higher value of any of these variables represents more effective CSE.

Summary statistics are presented in Table 3, where population-weighted sample means of the CSE variables by divorce law regime are included. The average state that introduced joint custody and unilateral divorce has a slightly greater percentage of CSE cases collected and a slightly greater average of collections than does the average state that passed any other divorce law reforms. A similar pattern is also observed for both paternity rate and location rate. On average, states that implemented joint custody or joint custody and unilateral divorce make greater efforts in CSE.

Table 4 presents estimates of the dynamic effect of unilateral divorce reforms after controlling for the effect of CSE on divorce by using the collection rate, Columns (1) to (3), and the average collections, Columns (4) to (6), separately.[29] As can be seen in Table 4, the results do not differ from those observed when we just introduce controls for custody reforms in all specifications (Table 2). The dynamic response of divorce rates to unilateral divorce reforms after a decade is similar to that observed by Wolfers (2006) in specifications (1) and (2), when we introduced collection rate, and in specifications (4) and (5), after controlling for average collections. The effect of the introduction of unilateral divorce was reversed over the subsequent

[27] The data come from the third Annual Report to the US Congress on the CSE program for the period October l, 1977–September 30, 1978 to the 13th annual report for the period ending in 1988. Data from the first annual report are not included in the analysis since they differ in the period covered, from January 4, 1975 to June 30, 1976. For the same reason, we do not include data from the special supplemental report that was issued to cover the period July 1 to September 30, 1976. Information from the second report is not included since the average annual CSE caseload is not available.

[28] Owing to lack of data, we cannot introduce into the analysis precisely the same measures of CSE as those used by Nixon (1997) or Heim (2003). However, our database contains information on a longer period, from 1977 to 1988; Heim (2003) only utilized data for the period 1991–1995 to capture the effect of CSE efforts on divorce rates.

[29] All those measures of CSE efforts take a value of zero from 1956 to 1977 and then they take the value of the CSE measure. This can be problematic since we are not considering previous differences in the child support policies by state; however, the introduction of state fixed effects and state-specific time trends should mitigate this problem. We also repeated the analysis by using only data from 1978 and the results do not change substantially, namely we observe no effect of unilateral divorce on the divorce rate in the long run.

decade. However, when controls for state-specific quadratic trends are added, the rise in divorce rate following the introduction of unilateral divorce reforms seems to be permanent. [30]

Another strategy to capture the effect of the CSE efforts is individually considering the effect of child support reinforcement by the divorce law regime. As explained above, if CSE efforts differ under different divorce laws, we would expect to observe differences in the impact of the CSE on the divorce rate by the divorce law regime.

The results in Table 5 suggest that the distinction between CSE efforts by divorce law reform is empirically important for our purposes. Although the sign of the long-run effect of the unilateral divorce reform does not turn positive in all the coefficients of interest, albeit those are not statistically significant, it seems that what is driving the results obtained by Wolfers 10 years after the introduction of unilateral divorce are those changes in divorce laws that govern the aftermath of divorce – see Columns (1), (2), (4), and (5).

We also looked at the effect of other CSE policies, namely paternity rate and location rate, on the divorce rate to check whether our results are maintained when we extend CSE variables. The inclusion of the four variables used to measure the CSE efforts together in the same specification is possible since those variables are not highly correlated as they are in Heim (2003), see Table 6. As can be seen in Table 7, our results are quite consistent.

Further, we reran all the regressions presented in this research by using a longer panel with data on divorce rates from 1956 to 1998. Table 8 shows the results of the dynamic effect of divorce law reforms, excluding controls for custody law reforms and CSE policies in Columns (1) to (3) and including those controls in Columns (4) to (6). Our results are robust. Therefore, the long-run effect of unilateral divorce on the divorce rate observed by Wolfers (2006) seems to be explained by changes in the aspects that regulate the aftermath of divorce.

As in the previous section, we repeated the analysis individually for couples with and without children in order to check whether our results operate through the behavior of the childless couples, the sub-population not affected by the CSE efforts at the time of divorce. We ran equation (1) and equation (3) using as dependent variables the divorce rates among couples with and without children and controlling for CSE measures.

The results show greater differences in the coefficients measuring the response of divorce rate to unilateral divorce reforms for couples with children when quadratic state-specific time trends are included (see Figure 7). For childless couples, we observe slight differences in the coefficients after adding quadratic state-specific time trends, but as explained above, those

[30] The effect of the divorce law on the divorce rate is also sensitive to the introduction of state-specific quadratic trends in Wolfers (2006). As mentioned above, we can justify the inclusion of those state-specific trends to address omitted variable bias. To check this further, we also plotted the residuals from the regressions presented in Table 7 for each state; see a similar analysis in Friedberg (1998). It was observed that not only linear trends but also quadratic trends need to be added to capture the effect of the reforms because of the trending behavior of residuals. It seems that our estimates confound the influence of law reforms with the omitted trend in a state’s divorce propensity when state-specific quadratic trends are not added, biasing the estimated effect of the reforms. These figures are available upon request.

differences might be because of second-order effects (Halla 2011). Although, to our knowledge, there is no published research studying the effect of the CSE program on marriage rates, stricter enforcement efforts seem to influence fertility decisions and the investment in child outcomes (Aizer and McLanahan 2006). Increases in CSE efforts provide men with clear disincentives to have children in order to reduce the costs of divorce; hence, we would expect an increase in the number of childless couples at risk of divorce, and so, an increase in the divorce rate. If the coefficients measuring the effect of unilateral divorce captured these second-order effects in addition to or instead of the unilateral divorce response, the magnitude of the effect should decrease after controlling for CSE measures. The results suggest that the effect of the CSE efforts also seems to be picked up by the coefficients capturing the dynamic response of divorce rates to unilateral divorce even after separating the divorce rates of couples with and without children.

We make out a case for the importance of controlling for the aftermath of divorce to determine the effect of divorce law reforms on divorce rates, but acknowledge that our list of controls is rather limited. For example, Aid to Families with Dependent Children (AFDC) Benefits, or Temporary Assistance for Needy Families (TANF) since 1996 are not considered in our analysis. Our omission of these variables is partly because the CSE program aims at reducing those welfare benefits. It is unclear whether we would want to include them since, as Hoffman and Duncan (1995) show, welfare benefits have a small effect on the probability that a married woman will become divorced; thus, it is not a significant determinant of divorce decisions.

Selection into marriage

There are other potential explanations of the somewhat puzzling change in the response of divorce rate to unilateral divorce reforms in the long run. On one hand, the number of people getting married after the adoption of the new divorce legal regime may change in size. Thus, we would expect a drop in the divorce rate if fewer individuals marry. Additionally, there is existing literature that suggests that unilateral divorce impacts not only on the decision to end a marriage by decreasing the costs of divorce, which is called the *incentive effect*, but also on the decision to enter into a marriage, termed the *selection effect* (see, for instance, Matouschek and Rasul 2008; Mechoulan 2006). This second effect, the *selection effect*, could also explain the drop in the divorce rate in the long run. The divorce rate should fall if those getting married after the introduction of the unilateral divorce law are couples who were able to sort themselves better at marriage under the new divorce legal regime, in order to enhance the stability of their marriages and increase the quality of the match (Matouschek and Rasul 2008; Mechoulan 2006; Rasul 2006b; Weiss and Willis 1997).[31]

[31] Note that the effect of unilateral divorce on the divorce rate of those who married after the reforms is not so clear since as the costs of divorce have been reduced with the liberalization of divorce laws, the costs of entering into a

To tackle the first issue, namely changes in the number of couples getting married, we add as a control to our analysis the crude marriage rate, defined as the number of marriages per 1,000 inhabitants. These data come from the Vital Statistics of the US. The results are shown in Appendix A. Table A1 reports the estimates of Wolfers's main analysis (replicated in Panel A of Table 1) after adding the crude marriage rate as a control. It can be seen that the results vary a little, implying that Wolfers's findings are not being driven by changes in the number of couples getting married. Similarly, our estimates are also robust to the inclusion of the crude marriage rate. We repeated all analyses by including that control. Table A2 presents the estimates of the regressions shown in Table 7 but after adding the crude marriage rate to the analysis. Thus, the results suggest that changes in the laws governing the aftermath of divorce seem to conduct the behavior of the divorce rate 10 years after the introduction of unilateral divorce instead of a negative response of the divorce rate to that reform or a change in the number of couples getting married.

To test this further, we also reran all analyses by utilizing the annual number of divorces per 1,000 married inhabitants as the dependent variable in lieu of the annual number of divorces per 1,000 inhabitants. Although the population 'at risk' is considered properly with that measure of divorce, it is important to note that this is not the standard measure of divorce used by researchers because of problems of availability. We obtained the stock of married people in order to calculate that measure of divorce rate for the years in which the decennial US census was conducted. This information was obtained from the Integrated Public Use Microdata Series (Ruggles et al. 2010). Yearly data was calculated by linear interpolation. In Appendix B, we present the results for the same specifications as before, which correspond to those displayed in Panel A of Table 1 and in Table 7. The results do not change although, as expected, the magnitude of the effect varies a little. Thus, our empirical findings suggest that changes in child custody and child support were driving the negative response of the divorce rate instead of the unilateral divorce reforms. Our findings are also consistent to the use of another measure of the stock of married population obtained from the decennial US censuses from 1950 to 2000 and on the flow into and out of marriage from the Vital Statistics of the US.

With respect to the second concern, namely the *selection effect* as a potential explanation of the negative response of the divorce rate, we provide additional empirical evidence by using data on divorce rates by duration of marriage. Here, the divorce rate is defined as the number of annual divorces of couples who have been married for d years per 1,000 marriages d years ago. This variable is constructed by dividing the number of couples in state s who divorce in year t and have been married for d years over the total number of

bad marriage (in which couples are more likely to divorce) are also reduced (Alesina and Giuliano 2007; Drewianka 2008). Thus, in contrast to the prediction of the *selection effect*, an increase in the divorce rate of those getting married after the reforms could also be observed.

marriages in state s in year $t-d$. Matouschek and Rasul (2008) use a similar dependent variable to test for the *selection effect*. Data on annual divorces per duration of marriage come from the Vital Statistics of the United States (1956–1967) and the National Center for Health Statistics (1968–1988), whereas the information on the number of marriages is obtained from the Vital Statistics of the United States. This divorce rate allows us to observe the responses of those individuals that marry before and after the introduction of the unilateral divorce reforms in the same state, which can be useful to test whether the selection effect conducts our empirical findings.

If we observe a variation in the coefficients picking up the response of those couples married under the new regime, it is possible to argue that it is selection into marriage that drives the response of divorce rates to unilateral divorce in the long run. For instance, in the case of the dynamic response to unilateral divorce of the divorce rates of those couples married for three years, since the coefficient measuring the response of the divorce rate after 1–2 years of the adoption of the reforms only captures the response of those married under the old regime, we would expect that coefficient to be positive and significant. In the case of the coefficient measuring the impact of the divorce law reforms 3–4 years after its adoption, which captures the behavior of those married the same year as the reforms or one year later, if the selection effect matters, that coefficient should be statistically significant and negative or non-significant. The coefficient measuring the effect of divorce law reforms 5–6 years after its adoption picks up the responses of those couples married 2–3 years after the reforms; thus, again if the selection effect matters, that coefficient should be statistically significant and negative or non-significant, and so on.

The results are shown in Appendix C. We only display the results considering as dependent variable the divorce rate of those married for one year, three years, and five years ($d = 1$, 3, and 5) in Tables C1, C2, and C3, respectively. The results are maintained for the rest of the couples. Columns (1) to (3) replicate Wolfers's specification but with a different dependent variable. In Columns (4) to (6), we add controls for joint custody reforms and all CSE variables as in Table 7. If anything, we can observe that the response of the divorce rate to unilateral divorce reforms of couples married before and after the reforms does not vary substantially. It can be seen that the coefficients are positive and significant even when they capture the response of the divorce rate of those couples married under the new legal regime. Regardless of the duration of marriages, a decade after the introduction of unilateral divorce reforms, again the effect is not clear. Thus, these findings suggest that the composition of those couples that married after the reforms cannot explain the negative response of divorce rates to unilateral divorce.

We acknowledge that these results should be taken with caution. One can argue that our estimated parameters cannot precisely measure the effect of the divorce law reforms on the divorce rate since the number of states considerably varies from just 23 states in 1956 to 32 states in 1988. To examine this issue, we also reran the analysis with the states that had data for all periods, just 12 states, but the results are not conclusive even when we just replicate Wolfers's analysis. Divergences in the sample used could make the *selection effect* more or less relevant, for example by using more recent data, but we prefer to use the same sample as did Wolfers (2006) to make our results comparable to his.[32] It is also arguable that this empirical evidence is being driven by those couples that need time to adapt to the new legal regime. In this case, changes in the coefficients should not be observed for those couples married immediately after the reforms (note that we show that even the estimates capturing the response of the divorce rate of those couples married 5–6 years after the reforms do not vary). This is problematic to probe with this dataset since the results on the divorce rates of couples married for more than five or six years are hard to interpret. First, our measure of the divorce rate is potentially problematic. As time goes by, and as couples move between states, it is difficult to argue that we are adequately considering the population at risk of divorce. This is relevant in our analysis, but the most important problem is the availability of information on the stock of marriages in the 1940s or even earlier – information that is necessary, for example, to construct the divorce rates of those marriages of more than six years of duration in 1956 – and the pattern of the marriages in that period because of the Second World War. Despite these concerns, it is comforting that our results do not seem to be driven by the *selection effect.*[33]

V. Permanent shocks in US divorce rates

Up to this point, we have considered whether the reforms in relevant aspects of the aftermath of divorce are important to determine the effect of unilateral reforms on divorce rates. In this section, we explore the frequency of permanent shocks in divorce rates by examining whether the divorce rate is a stationary series, exhibits a unit root, or is stationary around a process subject to structural breaks.[34] Two main reasons justify this approach. First, it allows us to test whether and when there have been changes in divorce rates without imposing any a priori timing, such as the dates of the reforms, as many other factors can also explain the behavior of the divorce rate (see above). Second, we can test whether these changes in divorce rates are permanent (structural breaks). This analysis is necessary since even after controlling for law reforms concerning the aftermath of divorce it is unclear whether the rise in divorce as a result

[32] Previous literature on the *selection effect* issue only uses data from the late 1960s or even later (see Matouschek and Rasul (2008) who used data from 1968 to 1995, or Mechoulan (2006) who utilized CPS data from 1971–1998). In those cases, the sample begins in a period really close to the unilateral divorce reforms, which can complicate the identification of its effect.

[33] Because of limitations in the data available, this analysis cannot be repeated for couples with and without children.

[34] Note that permanent here means that the change is still in effect given a sample of data, but not that the change will last forever.

of the approval of unilateral divorce laws is persistent. Our results are sensitive to the inclusion of state-specific time trends.[35] Thus, we utilize an alternative econometric technique used in the economic literature of policy evaluation, namely the structural change methodology. It has been used to study the effect of policy interventions: the Boston Gun Project (Piehl et al. 2003), Public Interest Litigation in India (Rathinam and Raja 2008) or the Californian Under-age Drunk Driving Laws (Kuo 2011), and to track the evolution of economic and social variables subject to public and legal interventions such as the unemployment rate (Mitchell 1993; Papell et al. 2000) or the rate of crime (Narayan et al. 2005).

We also present possible explanations for the permanent shifts in the divorce rate. We relate it to divorce law reforms and to the law reforms concerning the aftermath of divorce. These policy changes can be considered to be major events that are known to have occurred in the period considered in the analysis and which may have caused the structural change in the behavior of the divorce rate series. In this case, the analysis is more interpretive since, in order to determine whether policy reforms have had a permanent impact on the divorce rate, we simply compare the timing of the reforms with the break dates in the stationary divorce rate series.

Unit roots in US divorce rate series

We first apply standard unit root methods to the divorce rate for 50 states from 1956 to 1998 (Louisiana is excluded because of the scarcity of data).[36] Formally, consider the following expression:

$$DR_t = \alpha + \rho DR_{t-1} + \varepsilon_t \,, \tag{4}$$

where DR_t is the divorce rate, α and ρ are parameters, and ε_t is the perturbation term. If $-1 < \rho < 1$, fluctuations would be transitory. The divorce rate would be a stationary time-series and any shock will dissipate over time.[37] However, when $\rho = 1$, any sudden shock would have permanent effects on the long-run level of the divorce rate. In this case, the divorce rate will be a nonstationary time-series, and the stochastic process modeled by equation (4) would be a random walk with drift (Brockwell and Davis 1991), which is referred to as a unit root process (see Banerjee et al. 1993; Hamilton 1994; Gujarati 1995).

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[35] As mentioned above, this weakness is also observed in Wolfers (2006); the effect of divorce laws on the divorce rate is also sensitive to the introduction of state-specific quadratic trends. However, it is worth noting that a visual inspection of the residuals points to the necessity of including state-specific quadratic trends to address omitted variable bias.

[36] We favor the use of the divorce rate with a longer series since the results are less reliable with data from 1956 to 1988. We also repeated the analysis with data from 1950 to 2007, the longest series on divorce rate available; the results are quite similar and are available upon request.

[37] A stochastic process is said to be stationary if its mean and variance are time-independent and if the covariance between any two periods depends only on the lag and not on the current time at which the covariance is calculated.

In order to test for the presence of unit roots, where $\rho = 1$, we apply the Augmented Dickey-Fuller (ADF) test (Dickey and Fuller 1979; 1981). The ADF test for non-trending data is carried out by running the following regression:

$$\Delta DR_t = \alpha + \gamma DR_{t-1} + \sum_{i=1}^{k} \left(c_i \Delta DR_{t-1} \right) + \varepsilon_t \,, \qquad (5)$$

where $\Delta DR_t = DR_t - DR_{t-1}$, $\gamma = (\rho - 1)$, and with k being the number of lags added to ensure that the residuals, ε_t, are Gaussian White Noises.[38] The optimal k is chosen using a "general-to-specific procedure" based on the t-statistic (Ng and Perron 1995). The null and alternative hypotheses are, respectively, $H_0 : \gamma = 0$, $H_A : \gamma < 0$. If γ is found to be equal to 0, then the divorce rate series will follow a random walk. If, by contrast, γ is found to be significantly smaller than 0, the divorce rate will be stationary around α.

Table 9 shows a summary of the results of the individual state unit root tests. The results suggest that the unit root scenario seems to describe the experience of US divorce rates best. When using ADF tests, the null hypothesis of a unit root in the divorce rate is not rejected for four out of 15 states, or eight percent of the states, at the 10 percent level of significance.[39] For these four states, fluctuations are transitory but for the rest of the states any sudden shock has permanent effects on the divorce rate. Although ADF tests are widely used, they are biased towards the non-rejection of the null hypothesis of a unit root (Perron 1989). This is problematic since a stationary process with a mean that exhibits a one-time permanent change in level may previously have been identified as a unit root process (Perron 1990). We revisit this issue below.

Robustness Checks: Panel Unit Root Test

We also consider the states jointly in a panel in order to test for a unit root in a balanced panel (excluding California, Indiana, Kentucky, Louisiana, New York, and West Virginia) and in an unbalanced panel that includes all states. We use three different panel unit root tests. The first is the Levin et al. (2002) test, which tests the null hypothesis that all series have a unit root versus the alternative where all series are stationary on the balanced panel. The second is the less restrictive test developed by Im et al. (2003). This test allows us to test the null of a unit root in all series versus the alternative that some of the series are stationary, with a potentially varying autoregressive parameter. We then use the Pesaran (2007) test for unit roots in heterogeneous panels with cross-section dependence. Pesaran's CADF eliminates cross-dependence by augmenting the standard DF (or ADF) regressions with cross-section averages of lagged levels

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[38] The residuals are Gaussian White Noises when they have a zero mean and a constant variance that is uncorrelated with ε_s for $t \neq s$.

[39] We also ran ADF tests incorporating a trend and the results were consistent.

and with first-differences of the individual series. Similar to the Im et al. (2003) test, Pesaran's CADF test is consistent under the alternative that only a fraction of the series is stationary. Moreover, to test for unit root in an unbalanced panel, we use a generalization of Pesaran's CADF test (Pesaran 2007).

Panel B in Table 9 reports the results of applying the panel unit root tests presented above. The results indicate that it is hard to maintain that all divorce rate series show unit root processes. When using the Levin et al. panel unit root test and the Im et al. test, we cannot reject the null hypothesis of a unit root even at the 10 percent level. However, Pesaran's test shows that, when controlling for cross-sectional dependence, the null hypothesis of a unit root is rejected at the one percent level. This is also observed when Pesaran's test is applied to an unbalanced panel. Thus, the evidence in favor of a unit root in the divorce rate is weak.

Unit Roots in the Presence of Structural Breaks

In the presence of a one-time structural break, standard ADF tests are biased towards the non-rejection of the null hypothesis because of a misspecification of the deterministic trend (Perron 1989). The estimator of the autoregressive parameter goes asymptotically to values close to one when the variable is generated by a stationary process in which the effect of a structural break is present. In our finite divorce rate series, this can be problematic since what we identified as a unit root process could have been specified better as a stationary process around a persistent shock. To tackle this type of problem, we utilize the unit root test proposed by Perron and Vogelsang (1992), which works properly in a structural break framework where the date of the break is supposed to be unknown, and is suitable for non-trending data.[40]

We estimate an additive outlier (AO) model or crash model for each state divorce rate, which allows for a sudden change in mean (the change is assumed to take effect instantaneously).[41] The model is estimated by the following two expressions:

$$DR_t = \mu + \delta DU_t + \eta_t \tag{6}$$

and

$$\eta_t = \sum_{i=0}^{k} \omega_i DTB_{t-i} + \rho \eta_{t-1} + \sum_{i=0}^{k} c_i \Delta \eta_{t-i} + \varepsilon_t \tag{7}$$

where η_t is the estimated residual from equation (6), with TB being the date of the break, $DTB_t = 1$ if $t = TB + 1$, and is 0 otherwise, and $DU_t = 0$ if $t \leq TB$, and is 1 otherwise.

[40] Other papers in which the break point selection is also endogenized are Banerjee et al. (1993) and Zivot and Andrews (1992).

[41] Since Wolfers (2006) found different short-run and long-run effects of the divorce law reforms on divorce rates, it is arguable that changes in divorce rates take place gradually. Thus, from a robustness perspective, we also used innovational outlier (IO) models, which allow for gradual changes in divorce rates. Our results are similar, although some of the structural breaks are detected some years later than those determined when using the AO model.

Both equations are estimated in two stages by OLS for each break year $TB = k+2,...,T-1$, with T being the number of observations and k the truncation lag parameter (Perron and Vogelsang 1992).

The results of applying the AO model to test for a unit root in the divorce rates of each state in the US under the null versus stationarity around a shifting mean under the alternative are also summarized in Table 9. The effect of taking into account the possible shock is substantial. At the 10 percent confidence level, the unit root null hypothesis is rejected in favor of a regime-wise stationary process in which the effect of a structural break is present for 48 percent of the states, or 24 out of 50. Thus, the results suggest that there is not a single scenario. These findings provide evidence in favor of both unit root processes and stationary processes subject to a structural break.

However, since socio-economic variables rarely show just one break (Clemente et al. 1998), and given that there is no economic reason for restricting the analysis to one break, we also explore the existence of multiple structural breaks in the divorce rate series once stationarity has been established using the methodology proposed by Bai and Perron (1998, 2003).[42] For the case with no trending regressors, we first estimate the linear regression with only a constant as the regressor:

$$DR_t = \mu + \delta DU_t + \eta_t \qquad (8)$$

with DR_t being the divorce rate, the observed independent variable. $DU_t = 1$ if $t > TB$, and 0 otherwise where TB is the break date explicitly treated as unknown. The method of estimation is based on the least-squares principle. The sup-F statistic is obtained by maximizing the difference between the restricted (without DU_t) and unrestricted sums of squared residuals over all potential break dates. When a break point is found, the full sample is divided into two subsamples at the break point, and subsequently the test is applied to each of the subsamples. This subdivision process does not end until the test fails to reject the null hypothesis of no additional structural changes, or until the subsamples become too small. In order to establish the final breaks, we use the repartition method defined in Bai (1997), estimating breaks one at a time.[43] We allow for heterogeneity and autocorrelation in the residuals. The method utilized is Andrews's (1991) automatic bandwidth with AR(1) approximation and the quadratic kernel. It

[42] We also repeated the analysis of unit roots considering the presence of two endogenous break points by using the methodology developed by Clemente et al. (1998). The results are consistent and are available upon request. Because of the short time span of the data, the use of other econometric techniques to test for unit roots allowing for the possibility of multiple structural breaks is problematic (Lumsdaine and Papell 1997).

[43] For those US states in which the sequential procedure found no break since the $\sup F_T(1)$ test was not significant, we use the LWZ method which is a modified Schwarz criterion proposed by Liu et al. (1997) to determine the number of breaks (see Bai and Perron (1998, 2003)).

imposes a trimming of 15 percent, thus each segment has at least 15 observations, and allows up to five breaks (Bai and Perron 1998, 2003).

Table 10 presents the significant break dates at the five percent level from the Bai and Perron tests for multiple structural changes. It also reports the mean divorce rates before the first break and after each subsequent break. For those states in which the one-break unit root tests provide evidence of stationarity, it is observed that 14 out of the 24 states (Alabama, Delaware, Georgia, Hawaii, Iowa, Michigan, Minnesota, Mississippi, New York, North Dakota, Pennsylvania, South Carolina, South Dakota, and Washington) have one significant break at the five percent level; four states (Arkansas, Massachusetts, Vermont, and West Virginia) present two breaks; another four states have three structural breaks (Idaho, Montana, New Jersey, and Texas), and just two states (Oregon and Utah) exhibit four breaks. The break dates chosen by the Bai and Perron procedure are close to that determined by the unit root in the presence of the one-time structural break. There are no more than three years between the break dates chosen by the one-time break test of unit root and those found by the Bai and Perron procedure.

Several aspects of these results are worth drawing attention to. Our findings provide strong empirical evidence against the view that all shocks have temporary effects on divorce. For all the states, we detected at least one significant structural break. These occasional shocks cause persistent changes in the equilibrium rate itself; thus, divorce rate series may be characterized as being stationary around occasional persistent shocks.

None of the 35 significant breaks detected in the 1960s and 1970s is negative, reflecting the increase in divorce in that period. However, the seven breaks chosen in the 1980s and 1990s are all negative. Note that the average divorce rate after those negative breaks is always greater than that before the first break and even greater than the average divorce rate after the structural breaks detected in the 1960s. Thus, the rise in the divorce rate during the 1970s is not totally compensated by the fall in the divorce rate during the 1980s and 1990s. Another interesting finding is that most of the break dates are clustered. Out of 42 breaks, 29 occurred between 1968 and 1978, but the greater concentration of breaks occurred from 1968 to 1972. Six of the breaks are found in the early and mid-1960s and just four in the 1980s and three in the 1990s.[44] All these permanent changes in divorce can be related to major events that occurred since the 1960s, such as a particular government policy (divorce law reforms, custody law reforms, or/and child support programs), but can be also associated with economic crises, wars, or other factors. We revisit this issue in the next subsection.

[44] It is likely that the methodology applied here was unable to detect breaks in the late 1980s and 1990s because of the proximity of the end of the sample. Once we extended the sample with data from 1950 to 2007, the number of breaks in the 1980s and 1990s considerably increases as well as those in the 1950s and 1960s, although there are still a greater number of breaks in the late 1960s and early 1970s. Note that the signs of the breaks do not change; they are positive from the 1950s to the 1970s and negative in the subsequent decades.

We also applied the Bai and Perron methodology to the 26 states for which the single-break tests do not provide evidence of stationarity. Even though we cannot strictly speak of a change in the mean caused by a structural break, since the assumptions of the Bai and Perron methodology are not satisfied, we consider these results to be an illustration of the pattern of the divorce rates for nonstationary states. All but one breaks chosen in the 1960s and 1970s are positive, the exception is Nevada, and among those located since the 1980s only one structural break is positive, Kentucky in 1985.[45] Thus, these findings suggest that stationary and nonstationary divorce rates have a similar pattern, although for the nonstationary divorce rate series all shocks have permanent effects on the level of divorce and for those stationary around occasional breaks only these breaks cause permanent changes in the divorce rate.

Reforms and Permanent Shifts in Divorce Rates

The time-series analysis allows us to ascertain the break dates, which is valuable information for studying whether a structural break on a certain date can be associated with a major event, see Piehl et al. (2003) and Kuo (2011). We focus on comparing the timing of the main policy reforms and the timing of the structural breaks that are determined by using the Bai and Perron test. Of course, such an analysis is interpretive in nature; hence, here it is not possible to derive causality between law reforms and divorce rates.

We concentrate first on the divorce rate series for which the Bai and Perron test is applicable, or those 24 states for which the unit root null can be rejected by the single-break test of the unit root. Of these 24, 13 have a break that is located close to the time of the divorce law reforms that were passed at the beginning of the 1970s. Only for the case of South Dakota is there no structural break in the divorce rate detected close to the adoption of the unilateral divorce law in 1985. For five of the 13 US states, (Alabama, Idaho, Iowa, North Dakota, and Oregon), the structural break is chosen in the year in which the divorce law was reformed or two years later. In the case of the other eight divorce rate series (Georgia, Hawaii, Massachusetts, Michigan, Minnesota, Montana, Texas, and Washington), breaks are found before the reforms although the reform dates are included in the confidence interval at the 95 percent level. Admittedly, one may conjecture that there are other factors associated with five of these eight breaks because those structural breaks are located more than two years before the reforms and because they coincide with the break dates of those states that did not pass any divorce law reforms in the period analyzed.

[45] To check whether our results are sensitive to the introduction of Nevada and Kentucky, we also ran several simple robustness checks on the analysis of previous sections. First, we dropped Nevada since the behavior of the divorce rate is clearly different to that of the rest of the states and this may be driving the results. In another specification, we dropped Kentucky since the divorce rate seems to have increased in this state during the 1980s, which might affect our estimates of the dynamic effect of the divorce rate on the unilateral divorce reforms. The results are consistent and are available upon request.

The structural breaks chosen in the 1960s and 1970s for the other 10 states (Arkansas, Delaware, Mississippi, New Jersey, New York, Pennsylvania, South Carolina, Utah, Vermont, and West Virginia) clearly cannot be associated with divorce law reforms in these states since they did not introduce such policy changes. One can argue that other major events caused all the permanent changes in that period since those changes are similar in all the states independent of the introduction of unilateral divorce. As an alternative explanation, it is possible to hypothesize that the Vietnam War was one of these particular events.[46] An increase in the number of divorces is a general pattern observed during and after a war (Pavalko and Elder 1990; South 1985). The rise in the divorce rate might be produced by a decrease in the population after wartime but also by the weakening of marriages under wartime conditions, the increase in war marriages, the separation imposed by the war, the opportunities for adultery, and even by an increase in the options for remarriage because of the rise in the number of widows (Philips 1988).[47] Fourteen of the 15 breaks are found in the Vietnam War and post-war period in those states without divorce law reforms. In the case of the states that implemented divorce law reforms, although for five breaks it is unclear whether divorce law reforms or the Vietnam War led to a change in the divorce rate, for the five states having more than one structural break, we observed two changes: one in the 1960s, at the time of the war, and another one close to the adoption of the unilateral divorce reforms. This finding suggests that permanent changes in divorce may have been produced by the reforms of laws regulating how spouses obtain a divorce.

With respect to the negative structural breaks, as mentioned above, those changes are grouped in the 1980s and 1990s, in this case, at the time of the custody law changes and the main reforms in the laws that try to ensure child support payments. Six of the seven structural breaks detected since the 1980s can be associated with the introduction of joint custody, although for two of them, Idaho and Texas, the break dates are located one year before the approval of custody reforms. For Oregon, another break in the divorce rate occurred in 1983, which is hard to relate to the adoption of the joint custody law since it occurs four years previous, but it is close to the changes in the CSE program.[48] Thus, a decade after the unilateral divorce reforms, what seems to conduct the behavior of the divorce rates are those reforms on

[46] We acknowledge that the women's movement of the 1960s and 1970s, the sexual revolution of the 1960s, the introduction of the birth control pill, and even the economic crisis of the 1970s can be considered to be major events that caused those permanent changes in the divorce rate, but since most of the break dates coincide with the year of the divorce law reforms, we indicate that those reforms could cause the permanent shocks.

[47] The rise in divorce because of a war can be permanent if it causes a change in attitudes towards divorce, since a greater number of divorces can mean that divorce becomes more acceptable.

[48] It is important to note that once the sample is extended to include data from 1950 to 2007, the longest series available, in addition to the rise in the number of breaks located in the 1980s and 1990s, the number of those breaks that can be related to those changes in the aftermath of divorce also increases.

the laws that govern the aftermath of divorce, which can be associated with negative permanent shocks in the divorce rate.

In a final analysis, we look at the divorce rate series of those nonstationary states. Although, as said above, it is not possible to speak of a change in the mean, it is comforting that the structural breaks located by the Bai and Perron procedure can also be related to the major events mentioned in this subsection. First, there is a wave of positive breaks at the time of the Vietnam War in almost all states. Then, we found a second positive wave of shocks close to the date of the implementation of the unilateral divorce reforms. Finally, the last wave of changes is negative, as previously stated, tallying with the custody law reforms and with the increase in CSE efforts.

VI. Conclusions

This paper aimed to disentangle the effects of law reforms that govern the aftermath of divorce from the effects of unilateral divorce in determining the behavior of US divorce rates. Because empirically it is unclear whether the coefficients measuring the response of divorce rates to divorce law reforms are only capturing the adjustment path of divorce rates to unilateral divorce when it is omitted major reforms that have swept the US since the late 1970s, we introduce to the analysis of the impact of unilateral divorce two main reforms in the area of post-divorce: the adoption of the joint custody regime and the CSE program.

The incorporation of the custody law change is important since the possibility of joint custody may counteract the reassignment of property rights generated by the unilateral divorce reforms, according to the Coase theorem. Under joint custody, parents have to collaborate and cooperate in decisions affecting the child; this implies a return to a situation in which mutual consent is necessary. It is not possible to leave out of this analysis the CSE program either. The increase in the efforts to try to ensure child support payments is relevant to the study of the response of divorce rates to divorce law reforms when less restrictive divorce laws are correlated with stricter enforcement efforts made by the states in order to achieve the objective of reducing child poverty and welfare costs.

Our results suggest that the divorce rate increased immediately after the adoption of unilateral divorce as in Wolfers (2006). After a decade, two countervailing forces seem to be operating. We show empirical evidence indicating that the negative evolution of the divorce rate since the 1980s seems to be because of law reforms concerning the aftermath of divorce rather than the reverse response of divorce rates to the implementation of unilateral divorce laws. Further, some of our estimates point to a permanent impact of the unilateral divorce reforms on divorce rates, suggesting that the Coase theorem cannot be applied to marital relations. All in all, we view our results as evidence in favor of the important role of laws that regulate the aftermath of divorce, but we also believe that a more thorough examination of the mechanisms through which those reforms operate is an interesting question for future research.

We also developed a supplemental analysis to explore the frequency of permanent shocks in US divorce rates. A clear finding from this analysis is that not all shocks have transitory effects on divorce rates, which is robust to a range of alternative tests. This result can be interpreted in the context of evaluating the effects of divorce laws on divorce rates. The positive permanent changes in divorce can be associated with the implementation of unilateral divorce and the negative permanent changes can be related to the reforms in the laws that regulate the aftermath of divorce, again suggesting an important impact of divorce law reforms on the evolution of divorce rates.

Acknowledgements

Rafael González-Val acknowledges financial support from the Spanish Ministerio de Educación y Ciencia (ECO2009-09332 and ECO2010-16934 projects), the DGA (ADETRE research group), and FEDER. Miriam Marcén acknowledges financial support from the Spanish Ministerio de Educación y Ciencia (ECO2008-01297 project). We benefited from the helpful comments of Olivier Donni, Adriaan Kalwij, Victor Montuenga, Almudena Sevilla-Sanz, and Arantza Ugidos. Comments from the editor William Neilson and anonymous reviewers have also improved the version originally submitted. Earlier versions of this paper were presented at the 9th Annual Conference of the European Economics and Finance Society (Athens, 2010), at the XXIV Annual Conference of the European Society for Population Economics (Essen, 2010), at the XIII Encuentro de Economía Aplicada (Seville, 2010), at the 25th Annual Congress of the European Economic Association (Glasgow, 2010), at the XXXV Symposium of Economic Analysis (Madrid, 2010), and at the IX Jornadas de Economía Laboral (Santiago de Compostela, 2011), with all the comments made by participants being highly appreciated. Comments from seminar participants at the Universidad del País Vasco are also acknowledged. All remaining errors are ours.

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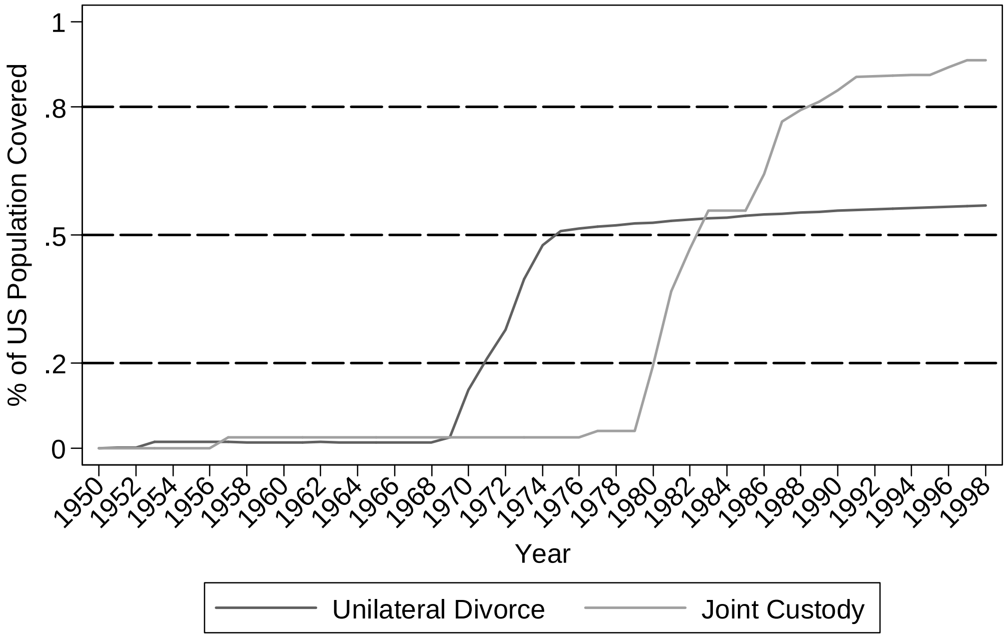
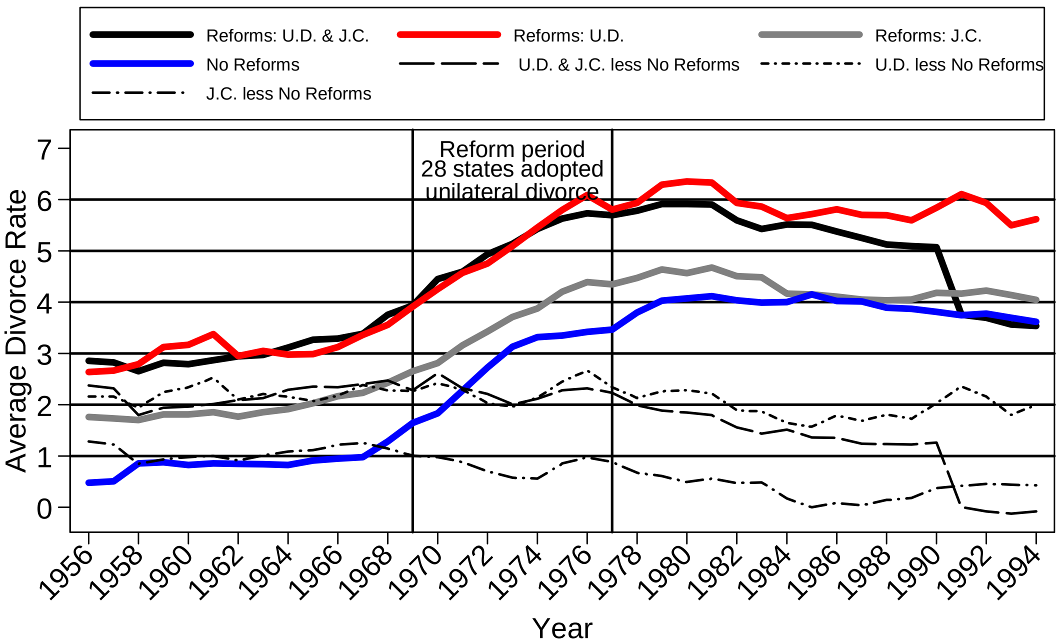
Figure 1: Coverage and Timing of Reforms



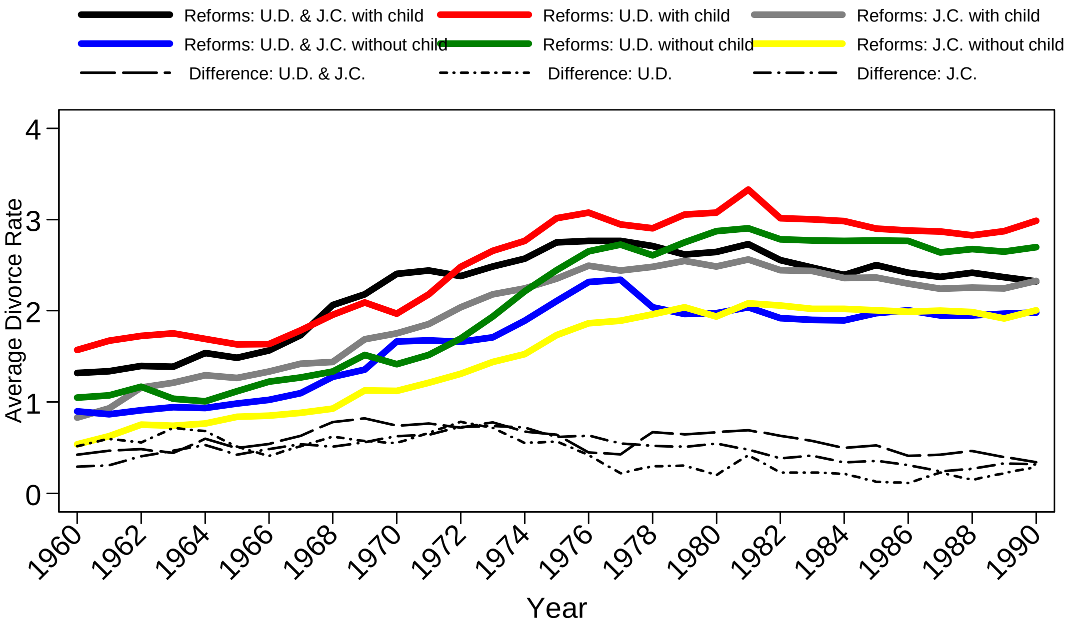
Source: US Census Bureau, Population Estimates. See a similar figure in Leo (2008)

Figure 2: Unilateral Divorce (U.D.)& Joint Custody Regime (J.C.)



Note: Joint Custody Regime from 1957-1988

Figure 3:
Average Divorce Rate: Couples with and without Children



Note: Joint Custody Regime from 1957-1988

Figure 4:
Response of Divorce Rate to Divorce Law Reform

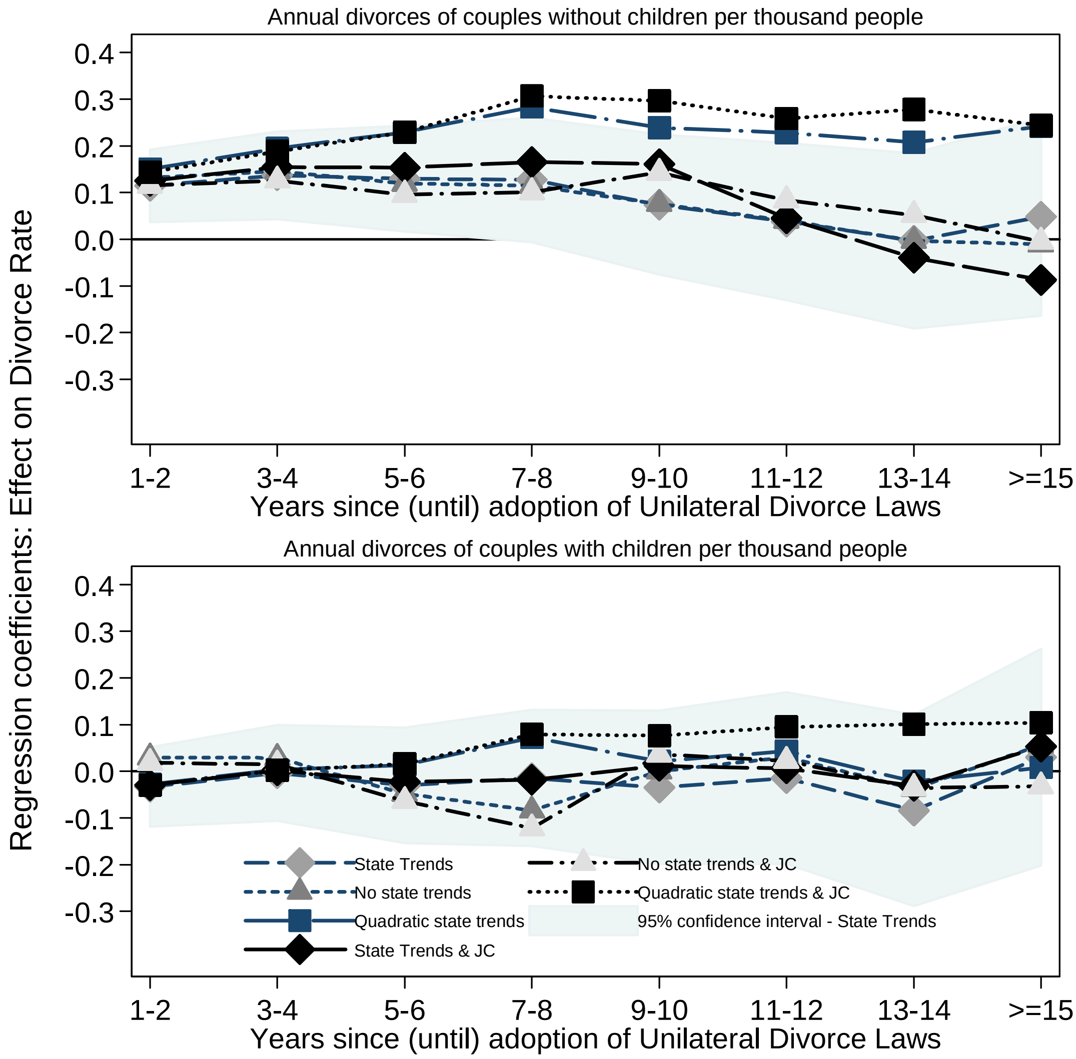
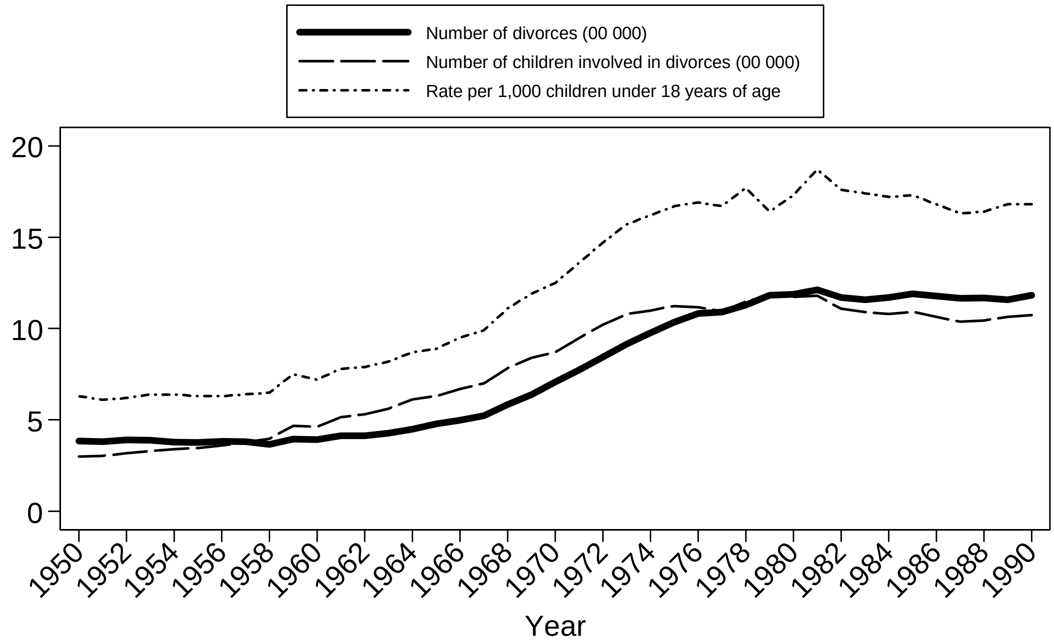
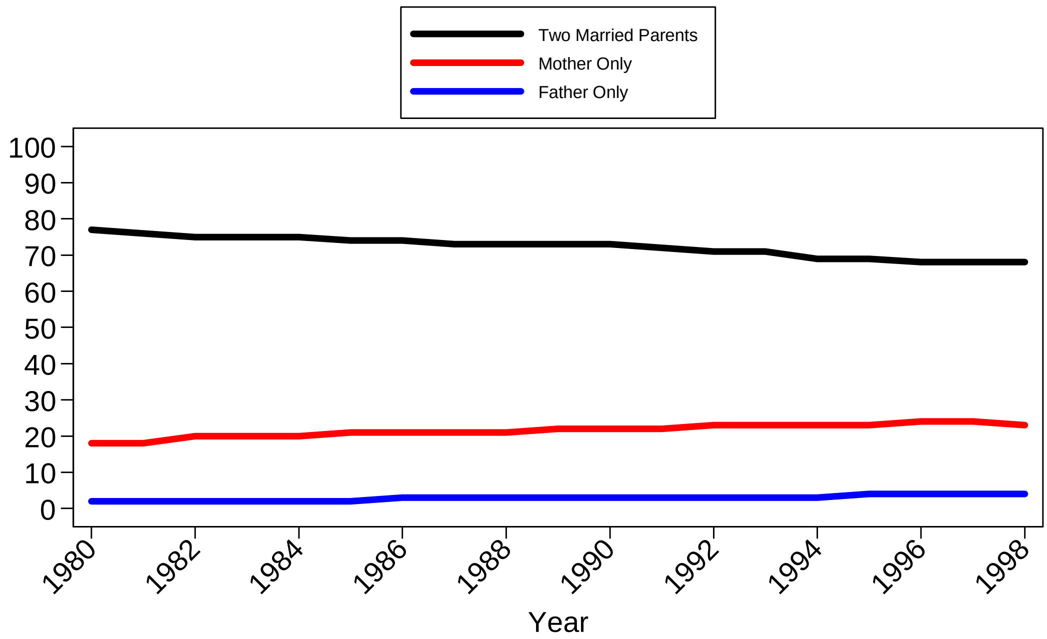


Figure 5:
Number of divorces and children involved in divorces



Source: Monthly Vital Statistics Report Vol. 43, No. 9.

Figure 6: Percentage of children ages 0–17
by presence of parents in the household



Source: U.S. Census Bureau, Current Population Survey, Annual Social and Economic Supplements

Figure 7:
Response of Divorce Rate to Divorce Law Reform

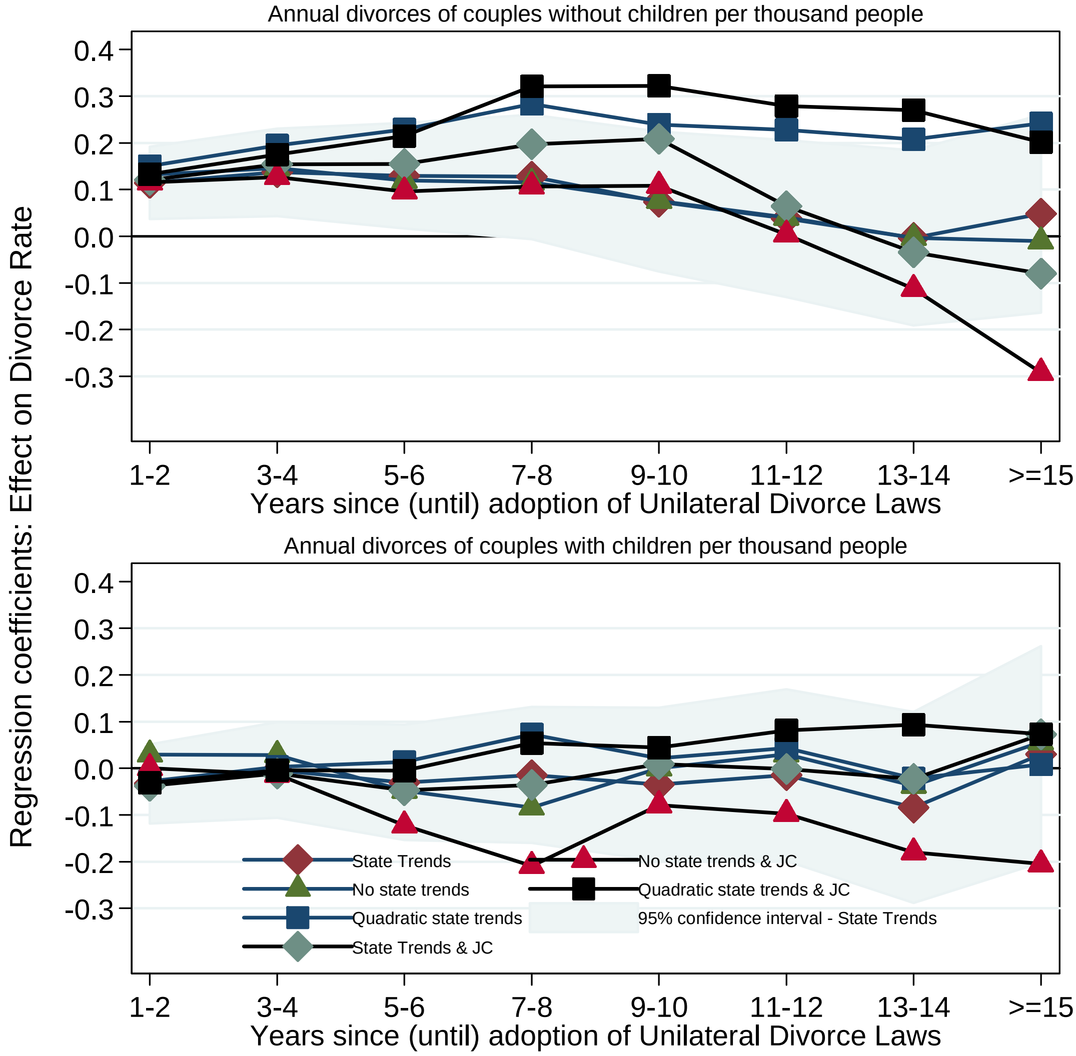


Table 1- WOLFERS'S RESULTS AND THE DYNAMIC EFFECTS AFTER ADOPTING JOINT CUSTODY LAWS

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

Panel A	(1) Basic specification	(2) State-specific linear trends	(3) State-specific quadratic trends
First two years	0.267***	0.342***	0.302***
	(0.085)	(0.062)	(0.054)
Years 3–4	0.210**	0.319***	0.289***
	(0.085)	(0.070)	(0.065)
Years 5–6	0.164*	0.300***	0.291***
	(0.085)	(0.077)	(0.079)
Years 7–8	0.158*	0.322***	0.351***
	(0.084)	(0.084)	(0.097)
Years 9–10	-0.121	0.081	0.161
	(0.084)	(0.091)	(0.117)
Years 11–12	-0.324***	-0.102	0.047
	(0.083)	(0.099)	(0.142)
Years 13–14	-0.461***	-0.202*	0.031
	(0.084)	(0.107)	(0.167)
Years 15	-0.507***	-0.210*	0.251
Onwards	(0.080)	(0.119)	(0.205)
Controls			
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
State * time	No	Yes	Yes
State * timesq	No	No	Yes
R^2	0.935	0.975	0.984
Sample	1956-88, n=1631 state-years		

Panel B	(1) Basic specification	(2) State-specific linear trends	(3) State-specific quadratic trends
First two years	0.273***	0.331***	0.324***
	(0.084)	(0.062)	(0.054)
Years 3–4	0.219***	0.306***	0.338***
	(0.084)	(0.070)	(0.066)
Years 5–6	0.174**	0.286***	0.376***
	(0.084)	(0.077)	(0.082)
Years 7–8	0.170**	0.310***	0.480***
	(0.083)	(0.084)	(0.101)
Years 9–10	-0.088	0.082	0.340***
	(0.083)	(0.091)	(0.125)
Years 11–12	-0.208**	-0.062	0.277*
	(0.084)	(0.099)	(0.152)
Years 13–14	-0.321***	-0.168	0.269
	(0.086)	(0.107)	(0.181)
Years 15	-0.298***	-0.176	0.503**
Onwards	(0.088)	(0.120)	(0.219)
Controls			
Years Joint Custody	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
State * time	No	Yes	Yes
State * timesq	No	No	Yes
R^2	0.937	0.976	0.985
Sample	1956-88, n=1631 state-years		

Notes: Estimated using state population weights. 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. Divorce laws coded by Wolfers (2006), http://bpp.wharton.upenn.edu/jwolfers/data.shtml, and Joint Custody laws are coded by Leo (2008). Significant at the *** 1% level; ** 5% level, and * 10% level.

Table 2- DYNAMIC EFFECTS OF UNILATERAL REFORM
(*Dependent variable: Annual divorces per 1,000 inhabitants*)

	(1) Basic specification	(2) State-specific linear trends	(3) State-specific quadratic trends
First two years	0.274***	0.324***	0.352***
	(0.084)	(0.062)	(0.056)
Years 3–4	0.221***	0.296***	0.387***
	(0.085)	(0.070)	(0.070)
Years 5–6	0.177**	0.270***	0.449***
	(0.084)	(0.077)	(0.090)
Years 7–8	0.174**	0.283***	0.578***
	(0.086)	(0.085)	(0.113)
Years 9–10	-0.060	0.035	0.457***
	(0.093)	(0.096)	(0.139)
Years 11–12	-0.277**	-0.131	0.468***
	(0.118)	(0.113)	(0.172)
Years 13–14	-0.471***	-0.279**	0.511**
	(0.148)	(0.133)	(0.211)
Years 15	-0.246*	-0.009	0.918***
Onwards	(0.147)	(0.139)	(0.264)
Controls			
Years Joint Custody	Yes	Yes	Yes
Years JC*Years UD	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
State * time	No	Yes	Yes
State * timesq	No	No	Yes
R^2	0.937	0.976	0.985
Sample	1956-88, n=1631 state-years		

Notes: Estimated using state population weights. 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. Divorce laws coded by Wolfers (2006), http://bpp.wharton.upenn.edu/jwolfers/data.shtml, and Joint Custody laws are coded by Leo (2008). Significant at the *** 1% level; ** 5% level, and * 10% level.

Table 3- CHILD SUPPORT ENFORCEMENT VARIABLES
(*Means and Standard Deviations*)

		Reforms			
	All	Unilateral Divorce	Joint Custody	UD & JC	No Reform
Collection Rate	15.603	15.008	15.167	16.422	15.505
	(9.676)	(13.742)	(7.806)	(7.720)	(8.200)
Average Collections	0.137	0.137	0.142	0.126	0.146
	(0.117)	(0.117)	(0.074)	(0.046)	(0.171)
Paternity Rate	0.861	0.564	1.286	0.879	0.865
	(0.587)	(0.561)	(0.632)	(0.459)	(0.559)
Location Rate	3.567	2.873	4.208	4.582	2.804
	(2.700)	(1.985)	(3.290)	(3.197)	(1.802)

Notes: Standard deviations in parentheses and population-weighted sample means. CSE data come from the OCSE Annual Reports.

Table 4- DYNAMIC EFFECTS OF UNILATERAL DIVORCE AND CONTROLS FOR CSE VARIABLES

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

	(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
First two years	0.275***	0.324***	0.354***	0.273***	0.324***	0.352***
	(0.084)	(0.062)	(0.056)	(0.084)	(0.062)	(0.056)
Years 3–4	0.224***	0.295***	0.391***	0.220***	0.295***	0.387***
	(0.084)	(0.070)	(0.070)	(0.085)	(0.070)	(0.070)
Years 5–6	0.190**	0.269***	0.459***	0.172**	0.268***	0.449***
	(0.084)	(0.078)	(0.090)	(0.084)	(0.077)	(0.090)
Years 7–8	0.182**	0.281***	0.588***	0.175**	0.283***	0.578***
	(0.086)	(0.086)	(0.113)	(0.086)	(0.085)	(0.113)
Years 9–10	-0.059	0.034	0.467***	-0.062	0.034	0.457***
	(0.093)	(0.096)	(0.139)	(0.093)	(0.096)	(0.139)
Years 11–12	-0.290**	-0.131	0.475***	-0.278**	-0.132	0.467***
	(0.118)	(0.113)	(0.172)	(0.118)	(0.113)	(0.172)
Years 13–14	-0.492***	-0.278**	0.512**	-0.472***	-0.280**	0.511**
	(0.148)	(0.133)	(0.211)	(0.148)	(0.133)	(0.211)
Years 15	-0.274*	-0.008	0.915***	-0.247*	-0.009	0.917***
Onwards	(0.148)	(0.139)	(0.264)	(0.147)	(0.139)	(0.264)
Collection Rate	-0.006**	0.000	-0.003*			
	(0.003)	(0.002)	(0.002)			
Average Collections				-0.173	-0.074	-0.012
				(0.186)	(0.120)	(0.099)
Years Joint Custody	Yes	Yes	Yes	Yes	Yes	Yes
Years JC*Years UD	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
State * time	No	Yes	Yes	No	Yes	Yes
State * timesq	No	No	Yes	No	No	Yes
R^2	0.938	0.976	0.985	0.937	0.976	0.985
Sample	1956-88, n=1631 state-years					

Notes: Estimated using state population weights. 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. CSE variables are from the OCSE Annual Reports. Divorce laws coded by Wolfers (2006), http://bpp.wharton.upenn.edu/jwolfers/data.shtml, and Joint Custody laws are coded by Leo (2008). Significant at the *** 1% level; ** 5% level, and * 10% level.

Table 5- DYNAMIC EFFECTS OF UNILATERAL REFORM AND CONTROLS FOR CSE VARIABLES BY DIVORCE LAW REGIME
(*Dependent variable: Annual divorces per 1,000 inhabitants*)

	(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
First two years	0.283***	0.323***	0.351***	0.282***	0.327***	0.347***
	(0.084)	(0.061)	(0.055)	(0.084)	(0.062)	(0.055)
Years 3–4	0.246***	0.303***	0.387***	0.245***	0.315***	0.386***
	(0.085)	(0.069)	(0.070)	(0.085)	(0.069)	(0.070)
Years 5–6	0.251***	0.312***	0.462***	0.223***	0.319***	0.459***
	(0.089)	(0.079)	(0.090)	(0.086)	(0.078)	(0.090)
Years 7–8	0.275***	0.348***	0.593***	0.293***	0.398***	0.623***
	(0.097)	(0.090)	(0.113)	(0.095)	(0.088)	(0.113)
Years 9–10	0.063	0.124	0.471***	0.065	0.163*	0.503***
	(0.110)	(0.103)	(0.140)	(0.103)	(0.099)	(0.139)
Years 11–12	-0.161	-0.058	0.460***	-0.153	-0.013	0.499***
	(0.132)	(0.119)	(0.172)	(0.125)	(0.115)	(0.171)
Years 13–14	-0.355**	-0.205	0.492**	-0.352**	-0.171	0.532**
	(0.161)	(0.138)	(0.211)	(0.152)	(0.134)	(0.210)
Years 15	-0.148	0.032	0.874***	-0.143	0.105	0.945***
Onwards	(0.159)	(0.144)	(0.263)	(0.152)	(0.141)	(0.263)
CSE in states with:						
Unilateral Reform	-0.010***	-0.004*	-0.004**	-1.102***	-0.935***	-0.586***
	(0.003)	(0.002)	(0.002)	(0.338)	(0.218)	(0.181)
Joint Custody	0.012*	0.024***	0.010*	-0.056	0.264	0.183
	(0.007)	(0.006)	(0.006)	(0.640)	(0.433)	(0.362)
UD & JC	-0.016**	0.016***	0.011**	3.259***	-0.222	-0.191
	(0.007)	(0.005)	(0.005)	(1.039)	(0.713)	(0.602)
No Reform	-0.001	0.005	-0.001	-0.017	0.179	0.165
	(0.004)	(0.003)	(0.003)	(0.213)	(0.137)	(0.113)
Years Joint Custody	Yes	Yes	Yes	Yes	Yes	Yes
Years JC*Years UD	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
State * time	No	Yes	Yes	No	Yes	Yes
State * timesq	No	No	Yes	No	No	Yes
R^2	0.938	0.977	0.985	0.938	0.977	0.985
Sample	1956-88, n=1631 state-years					

Notes: Estimated using state population weights. 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. CSE variables are from the OCSE Annual Reports. Divorce laws coded by Wolfers (2006), http://bpp.wharton.upenn.edu/jwolfers/data.shtml, and Joint Custody laws are coded by Leo (2008). Columns 1, 2, and 3 include as CSE variable Collection Rate, Columns 4, 5, and 6 include as CSE variable Average Collections. Significant at the *** 1% level; ** 5% level, and * 10% level.

Table 6- CORRELATION BETWEEN CSE VARIABLES

	Collection Rate	Average Collections	Paternity Rate	Location Rate
Collection Rate	1			
Average Collections	-0.0607	1		
Paternity Rate	0.1019	-0.057	1	
Location Rate	0.0704	-0.0327	0.3566	1

Notes: Standard deviations in parentheses and population-weighted sample means. CSE data come from the OCSE Annual Reports.

Table 7- DYNAMIC EFFECTS OF UNILATERAL REFORM AND CONTROLS FOR ALL CSE VARIABLES BY DIVORCE LAW REGIME

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

	(1)	(2)	(3)		(1)	(2)	(3)
	Basic specification	State-specific linear trends	State-specific Quadratic trends	Cont.	Basic specification	State-specific linear trends	State-specific quadratic trends
First two years	0.286***	0.322***	0.347***	Paternity Rate in states with:			
	(0.084)	(0.061)	(0.055)	Unilateral Reform	0.186*	-0.073	0.119
Years 3–4	0.258***	0.318***	0.386***		(0.105)	(0.090)	(0.094)
	(0.085)	(0.069)	(0.070)	Joint Custody	0.055	0.073	0.088
Years 5–6	0.260***	0.350***	0.464***		(0.102)	(0.072)	(0.066)
	(0.092)	(0.080)	(0.090)	UD & JC	0.102	-0.339***	0.018
Years 7–8	0.321***	0.454***	0.614***		(0.113)	(0.093)	(0.100)
	(0.106)	(0.093)	(0.114)	No Reform	0.096	0.084	0.050
Years 9–10	0.084	0.251**	0.476***		(0.094)	(0.069)	(0.059)
	(0.123)	(0.109)	(0.142)	Location Rate in states with:			
Years 11–12	-0.114	0.071	0.461***	Unilateral Reform	-0.005	0.005	-0.006
	(0.144)	(0.123)	(0.173)		(0.027)	(0.020)	(0.018)
Years 13–14	-0.335*	-0.083	0.484**	Joint Custody	-0.009	-0.021	-0.016
	(0.178)	(0.144)	(0.213)		(0.019)	(0.014)	(0.014)
Years 15	-0.243	0.215	0.828***	UD & JC	-0.026*	0.015	-0.025*
Onwards	(0.189)	(0.155)	(0.267)		(0.015)	(0.012)	(0.013)
Collection Rate in states with:				No Reform	0.005	0.005	0.003
Unilateral Reform	-0.009***	-0.002	-0.003*		(0.026)	(0.019)	(0.016)
	(0.003)	(0.002)	(0.002)	Years Joint Custody	Yes	Yes	Yes
Joint Custody	0.012	0.023***	0.011*	Years JC*Years UD	Yes	Yes	Yes
	(0.007)	(0.006)	(0.006)	Year FE	Yes	Yes	Yes
UD & JC	-0.014**	0.020***	0.012**	State FE	Yes	Yes	Yes
	(0.007)	(0.006)	(0.005)	State * time	No	Yes	Yes
No Reform	-0.005	0.000	-0.003	State * timesq	No	No	Yes
	(0.005)	(0.003)	(0.003)				
Average Collections in states with:							
Unilateral Reform	-0.964***	-0.819***	-0.573***				
	(0.350)	(0.225)	(0.188)				
Joint Custody	-0.385	0.244	0.013				
	(0.718)	(0.463)	(0.397)				
UD & JC	3.346***	0.354	0.177				
	(1.085)	(0.725)	(0.627)				
No Reform	-0.066	0.164	0.151	R^2	0.939	0.978	0.985
	(0.216)	(0.138)	(0.115)	Sample	1956-88, n=1631 state-years		

Notes: Estimated using state population weights. 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. CSE variables are from the OCSE Annual Reports. Divorce laws coded by Wolfers (2006), http://bpp.wharton.upenn.edu/jwolfers/data.shtml, and Joint Custody laws are coded by Leo (2008). Significant at the *** 1% level; ** 5% level, and * 10% level.

Table 8- DYNAMIC EFFECTS OF UNILATERAL REFORM. Sample: 1956–1998.
(*Dependent variable: Annual divorces per 1,000 inhabitants*)

	(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
First two years	0.274***	0.399***	0.294***	0.281***	0.316***	0.295***
	(0.096)	(0.065)	(0.053)	(0.094)	(0.066)	(0.054)
Years 3–4	0.223**	0.398***	0.272***	0.253***	0.310***	0.284***
	(0.096)	(0.071)	(0.058)	(0.095)	(0.073)	(0.062)
Years 5–6	0.180*	0.399***	0.263***	0.247**	0.328***	0.303***
	(0.095)	(0.076)	(0.063)	(0.100)	(0.082)	(0.073)
Years 7–8	0.179*	0.442***	0.306***	0.337***	0.446***	0.394***
	(0.095)	(0.082)	(0.068)	(0.113)	(0.094)	(0.086)
Years 9–10	-0.095	0.215**	0.095	0.121	0.291***	0.194*
	(0.094)	(0.087)	(0.073)	(0.127)	(0.106)	(0.100)
Years 11–12	-0.302***	0.065	-0.042	-0.100	0.115	0.084
	(0.093)	(0.094)	(0.078)	(0.149)	(0.120)	(0.114)
Years 13–14	-0.445***	-0.018	-0.091	-0.297*	-0.042	-0.046
	(0.092)	(0.101)	(0.085)	(0.178)	(0.138)	(0.130)
Years 15 Onwards	-0.576***	0.016	0.054	-0.042	0.254*	0.123
	(0.061)	(0.113)	(0.098)	(0.171)	(0.145)	(0.145)
By Divorce Law Regime:						
Collection Rate	No	No	No	Yes	Yes	Yes
Average Collections	No	No	No	Yes	Yes	Yes
Paternity Rate	No	No	No	Yes	Yes	Yes
Location Rate	No	No	No	Yes	Yes	Yes
Controls:						
Years Joint Custody	No	No	No	Yes	Yes	Yes
Years JC*Years UD	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
State * time	No	Yes	Yes	No	Yes	Yes
State * timesq	No	No	Yes	No	No	Yes
R^2	0.906	0.966	0.980	0.913	0.969	0.981
Sample	1956-98, n=2102 state-years					

Notes: Estimated using state population weights. 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. CSE variables are from the OCSE Annual Reports. Divorce laws coded by Wolfers (2006), http://bpp.wharton.upenn.edu/jwolfers/data.shtml, and Joint Custody laws are coded by Leo (2008). Significant at the *** 1% level; ** 5% level, and * 10% level.

Table 9- RESULTS OF THE UNIT ROOT TESTS ON DIVORCE RATES

A: State-specific tests[1]		
Alternative hypothesis	Trend stationary	Trend stationary with one break
Significance level	% Unit root rejected	% Unit root rejected
1%	2%	8%
5%	4%	30%
10%	8%	48%
B: Panel tests (p=1)	Balanced panel[2]	Unbalanced panel[3]
	Test-statistic (p-value)	Test-statistic (p-value)
Levin et al. (2002)	-1.109 (0.133)	
Im et al. (2003)	-0.949 (0.171)	
Pesaran (2007)	-5.137 (0.000)	-5.676 (0.000)

Notes: The null hypothesis is in all cases a unit root in the divorce rate. Following the suggestion in Ng and Perron (1995), we choose the optimal number of lagged growth rates to be included in the regression to control for autocorrelation using a 'general-to-specific procedure' based on the t-statistic. The maximum lag length to start this procedure is set at 11. The panel test statistics are the t^*, the $W[\bar{t}]$, and the $Z[\bar{t}]$-statistic in case of the Levin et al. (2002), Im et al. (2003), and Pesaran (2007) test respectively. Panel statistics are based on univariate AR(1) specifications including constant.

[1] Excluding Louisiana.

[2] Excluding California, Indiana, Kentucky, Louisiana, New York, and West Virginia.

[3] Including all states, except Louisiana.

Table 10- RESULTS OF MULTIPLE STRUCTURAL CHANGES

State	Mean Divorce Rate Before Break	TB_1	TB_2	TB_3	TB_4	TB_5
Alabama	3.94	6.22				
		1971				
Alaska	3.00	4.13	6.07	8.25	6.31	5.10
		1961	*1968*	*1974*	*1985*	*1992*
Arizona	5.17	6.99	5.90			
		1966	*1992*			
Arkansas	3.11	4.17	6.85			
		1964	*1970*			
California	3.12	3.86	5.61	4.68		
		1964	*1969*	*1985*		
Colorado	3.51	5.74				
		1969				
Connecticut	1.41	3.57				
		1971				
Delaware	1.58	4.55				
		1969				
District of Columbia	1.96	4.49	5.97	3.58		
		1970	*1978*	*1984*		
Florida	4.23	5.20	7.04	5.96		
		1965	*1971*	*1987*		
Georgia	2.63	5.49				
		1969				
Hawaii	2.11	4.53				
		1969				
Idaho	3.94	4.97	6.70	6.12		
		1966	*1973*	*1981*		
Illinois	2.28	3.97				
		1968				
Indiana	3.28	6.39				
		1969				
Iowa	1.96	3.69				
		1971				
Kansas	2.56	5.10	4.42			
		1969	*1992*			
Kentucky	2.36	3.38	4.40	5.67		
		1967	*1973*	*1985*		
Maine	2.44	4.72				
		1969				
Maryland	1.85	2.37	3.82	3.36		
		1966	*1972*	*1985*		
Massachusetts	1.14	1.80	2.84			
		1964	*1971*			
Michigan	2.35	4.28				
		1969				
Minnesota	1.45	3.44				
		1970				
Mississippi	2.63	5.04				
		1970				
Missouri	2.82	4.14	5.39	4.95		
		1967	*1974*	*1983*		
Montana	3.02	4.77	6.09	5.00		

		1968	*1974*	*1983*	
Nebraska	1.84	3.86			
		1971			
Nevada	25.29	14.94	10.72		
		1962	*1980*		
New Hampshire	2.01	3.33	5.24	4.68	
		1966	*1972*	*1984*	
New Jersey	0.99	2.75	3.58	3.13	
		1971	*1977*	*1991*	
New Mexico	3.16	7.56	5.92		
		1970	*1986*		
New York	0.68	3.28			
		1971			
North Carolina	1.35	2.51	3.85	4.95	
		1964	*1971*	*1977*	
North Dakota	1.23	3.26			
		1972			
Ohio	2.53	4.73			
		1969			
Oklahoma	5.18	7.22			
		1969			
Oregon	3.34	4.80	6.55	5.58	4.87
		1967	*1973*	*1983*	*1992*
Pennsylvania	1.48	3.22			
		1972			
Rhode Island	1.18	2.28	3.70	3.36	
		1969	*1975*	*1990*	
South Carolina	1.43	3.97			
		1971			
South Dakota	1.49	3.69			
		1972			
Tennessee	3.01	6.30			
		1971			
Texas	3.80	4.75	6.29	5.39	
		1967	*1973*	*1986*	
Utah	2.03	2.97	4.15	5.15	4.52
		1961	*1968*	*1974*	*1992*
Vermont	1.49	3.55	4.60		
		1970	*1978*		
Virginia	2.10	4.31			
		1972			
Washington	3.68	6.02			
		1968			
West Virginia	2.08	3.45	5.12		
		1968	*1974*		
Wisconsin	1.43	3.42			
		1972			
Wyoming	3.99	5.63	7.62	6.62	
		1966	*1973*	*1985*	

Notes: Columns 3 to 7 include the mean divorce rates following the break, with the break date reported in italics. States with a short time span divorce rate series: CA, IN, KY, LA, NY, WV. Breaks are selected by the repartition method from the sequential procedure at the 5% level with the exception of these states, for which breaks are selected by LWZ method: AK, AR, CA, DC, FL, ID, KY, MD, MO, MT, NH, NC, OR, RI, TX, UT, WV, WY.

Appendix A

Table A1- RESULTS AFTER ADDING THE CRUDE MARRIAGE RATE
(*Dependent variable: Annual divorces per 1,000 inhabitants*)

	(1) Basic specification	(2) State-specific linear trends	(3) State-specific quadratic trends
First two years	0.257***	0.341***	0.306***
	(0.070)	(0.062)	(0.054)
Years 3–4	0.218***	0.317***	0.298***
	(0.071)	(0.070)	(0.065)
Years 5–6	0.180**	0.297***	0.304***
	(0.070)	(0.077)	(0.079)
Years 7–8	0.191***	0.317***	0.369***
	(0.070)	(0.084)	(0.096)
Years 9–10	-0.091	0.076	0.181
	(0.069)	(0.092)	(0.117)
Years 11–12	-0.299***	-0.108	0.067
	(0.069)	(0.099)	(0.141)
Years 13–14	-0.427***	-0.208*	0.055
	(0.069)	(0.107)	(0.167)
Years 15	-0.499***	-0.216*	0.275
Onwards	(0.067)	(0.119)	(0.204)
Crude Marriage Rate	0.125***	-0.006	0.026***
	(0.005)	(0.008)	(0.007)
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
State * time	No	Yes	Yes
State * timesq	No	No	Yes
R^2	0.955	0.975	0.984
Sample	1956-88, n=1631 state-years		

Notes: Estimated using state population weights. 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. Divorce laws coded by Wolfers (2006), http://bpp.wharton.upenn.edu/jwolfers/data.shtml. Significant at the *** 1% level; ** 5% level, and * 10% level.

Table A2- DYNAMIC EFFECTS OF UNILATERAL REFORM AND CONTROLS FOR ALL CSE VARIABLES BY DIVORCE LAW REGIME
(*Dependent variable: Annual divorces per 1,000 inhabitants*)

	(1) Basic specification	(2) State-specific linear trends	(3) State-specific Quadratic trends	Cont.	(1) Basic specification	(2) State-specific linear trends	(3) State-specific quadratic trends
First two years	0.279***	0.321***	0.351***	Paternity Rate in states with:			
	(0.068)	(0.061)	(0.055)	Unilateral Reform	0.266***	-0.076	0.124
Years 3–4	0.270***	0.315***	0.396***		(0.086)	(0.091)	(0.094)
	(0.070)	(0.069)	(0.070)	Joint Custody	0.048	0.071	0.083
Years 5–6	0.277***	0.346***	0.480***		(0.084)	(0.072)	(0.065)
	(0.075)	(0.080)	(0.090)	UD & JC	-0.220**	-0.333***	-0.008
Years 7–8	0.352***	0.448***	0.638***		(0.093)	(0.093)	(0.100)
	(0.087)	(0.093)	(0.114)	No Reform	0.071	0.085	0.053
Years 9–10	0.048	0.248**	0.495***		(0.077)	(0.069)	(0.059)
	(0.101)	(0.109)	(0.141)	Location Rate in states with:			
Years 11–12	-0.150	0.065	0.487***	Unilateral Reform	-0.019	0.005	-0.009
	(0.118)	(0.124)	(0.172)		(0.022)	(0.020)	(0.018)
Years 13–14	-0.310**	-0.093	0.525**	Joint Custody	-0.003	-0.021	-0.015
	(0.145)	(0.144)	(0.212)		(0.016)	(0.014)	(0.013)
Years 15	-0.242	0.210	0.864***	UD & JC	0.014	0.014	-0.023*
Onwards	(0.155)	(0.155)	(0.266)		(0.013)	(0.012)	(0.013)
Collection Rate in states with:				No Reform	0.028	0.004	0.004
Unilateral Reform	-0.008***	-0.002	-0.004*		(0.022)	(0.019)	(0.016)
	(0.003)	(0.002)	(0.002)	Crude Marriage Rate	0.128***	-0.008	0.026***
Joint Custody	0.008	0.023***	0.010*		(0.005)	(0.008)	(0.007)
	(0.006)	(0.006)	(0.006)	Years Joint Custody	Yes	Yes	Yes
UD & JC	-0.002	0.020***	0.012**	Years JC*Years UD	Yes	Yes	Yes
	(0.006)	(0.006)	(0.005)	Year FE	Yes	Yes	Yes
No Reform	-0.008**	0.001	-0.003	State FE	Yes	Yes	Yes
	(0.004)	(0.003)	(0.003)	State * time	No	Yes	Yes
Average Collections in states with:				State * timesq	No	No	Yes
Unilateral Reform	-1.021***	-0.821***	-0.581***				
	(0.286)	(0.225)	(0.187)				
Joint Custody	-0.146	0.243	0.061				
	(0.588)	(0.463)	(0.395)				
UD & JC	0.274	0.394	0.044				
	(0.895)	(0.726)	(0.625)				
No Reform	-0.104	0.165	0.150	R^2	0.959	0.978	0.986
	(0.177)	(0.138)	(0.114)	Sample		1956-88, n=1631 state-years	

Notes: Estimated using state population weights. 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. CSE variables are from the OCSE Annual Reports. Divorce laws coded by Wolfers (2006), http://bpp.wharton.upenn.edu/jwolfers/data.shtml, and Joint Custody laws are coded by Leo (2008). CSE variables are from the OCSE Annual Reports. Significant at the *** 1% level; ** 5% level, and * 10% level.

Appendix B

Table B1- DYNAMIC EFFECTS OF UNILATERAL REFORM

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

	(1) Basic specification	(2) State-specific linear trends	(3) State-specific quadratic trends
First two years	0.526**	0.656***	0.572***
	(0.239)	(0.178)	(0.162)
Years 3–4	0.334	0.526***	0.515***
	(0.237)	(0.198)	(0.193)
Years 5–6	0.180	0.422*	0.519**
	(0.234)	(0.218)	(0.235)
Years 7–8	0.185	0.482**	0.738**
	(0.231)	(0.237)	(0.286)
Years 9–10	-0.305	0.072	0.531
	(0.229)	(0.257)	(0.348)
Years 11–12	-0.707***	-0.289	0.425
	(0.228)	(0.278)	(0.418)
Years 13–14	-0.994***	-0.491	0.524
	(0.230)	(0.301)	(0.495)
Years 15	-0.991***	-0.419	1.200**
Onwards	(0.221)	(0.336)	(0.605)
Year FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
State * time	No	Yes	Yes
State * timesq	No	No	Yes
R^2	0.902	0.962	0.973
Sample	1956-88, n=1631 state-years		

Notes: Estimated using state population weights (equal to the denominator of the dependent variable). 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. Divorce laws coded by Wolfers (2006), http://bpp.wharton.upenn.edu/jwolfers/data.shtml. Significant at the *** 1% level; ** 5% level, and * 10% level.

Table B2- DYNAMIC EFFECTS OF UNILATERAL REFORM AND CONTROLS FOR ALL CSE VARIABLES BY DIVORCE LAW REGIME

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

	(1)	(2)	(3)		(1)	(2)	(3)
	Basic specification	State-specific linear trends	State-specific Quadratic trends	Cont.	Basic specification	State-specific linear trends	State-specific quadratic trends
First two years	0.577**	0.608***	0.646***	Paternity Rate in states with:			
	(0.236)	(0.176)	(0.169)	Unilateral Reform	0.200	-0.036	0.049
Years 3–4	0.456*	0.538***	0.707***		(0.132)	(0.116)	(0.128)
	(0.239)	(0.198)	(0.212)	Joint Custody	0.035	0.056	0.037
Years 5–6	0.455*	0.618***	0.930***		(0.131)	(0.094)	(0.091)
	(0.255)	(0.228)	(0.272)	UD & JC	0.383***	-0.124	-0.074
Years 7–8	0.613**	0.882***	1.363***		(0.142)	(0.121)	(0.138)
	(0.294)	(0.266)	(0.344)	No Reform	0.109	0.137	0.144*
Years 9–10	0.340	0.684**	1.417***		(0.119)	(0.089)	(0.081)
	(0.339)	(0.309)	(0.426)	Location Rate in states with:			
Years 11–12	-0.019	0.383	1.391***	Unilateral Reform	-0.017	-0.009	0.001
	(0.396)	(0.350)	(0.519)		(0.034)	(0.025)	(0.025)
Years 13–14	-0.434	0.156	1.479**	Joint Custody	-0.008	-0.022	-0.004
	(0.491)	(0.409)	(0.639)		(0.025)	(0.018)	(0.018)
Years 15	-0.356	0.840*	2.358***	UD & JC	-0.054***	-0.007	-0.023
Onwards	(0.526)	(0.442)	(0.802)		(0.019)	(0.015)	(0.018)
Collection Rate in states with:				No Reform	0.027	0.012	0.011
Unilateral Reform	-0.020**	-0.004	-0.007		(0.033)	(0.024)	(0.021)
	(0.009)	(0.006)	(0.006)	Years Joint Custody	Yes	Yes	Yes
Joint Custody	0.030	0.073***	0.045**	Years JC*Years UD	Yes	Yes	Yes
	(0.019)	(0.017)	(0.018)	Year FE	Yes	Yes	Yes
UD & JC	-0.032*	0.034**	0.008	State FE	Yes	Yes	Yes
	(0.019)	(0.016)	(0.016)	State * time	No	Yes	Yes
No Reform	-0.006	0.013	0.004	State * timesq	No	No	Yes
	(0.013)	(0.010)	(0.009)				
Average Collections in states with:							
Unilateral Reform	-3.113	-2.404*	-0.895				
	(2.070)	(1.357)	(1.200)				
Joint Custody	-1.484	2.383	1.971				
	(4.155)	(2.746)	(2.491)				
UD & JC	22.825***	5.802	2.621				
	(6.415)	(4.394)	(4.045)				
No Reform	-0.500	1.170	0.955	R^2	0.908	0.964	0.974
	(1.335)	(0.873)	(0.768)	Sample	1956-88, n=1631 state-years		

Notes: Estimated using state population weights (equal to the denominator of the dependent variable). 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. Divorce laws coded by Wolfers (2006), http://bpp.wharton.upenn.edu/jwolfers/data.shtml, and Joint Custody laws are coded by Leo (2008). CSE variables are from the OCSE Annual Reports. Significant at the *** 1% level; ** 5% level, and * 10% level.

Appendix C

Table C1- DYNAMIC EFFECTS OF UNILATERAL REFORM. Sample: 1956–1988

(*Dependent variable: Annual divorces of couples who have been married for one year per 1,000 marriages one year ago*)

	(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
First two years	2.973***	2.603***	1.672*	3.705***	2.208**	2.393**
	(1.068)	(0.974)	(0.987)	(0.973)	(0.921)	(1.013)
Years 3–4	4.525***	3.345***	1.833	5.547***	2.714**	3.338**
	(1.095)	(1.130)	(1.302)	(1.020)	(1.094)	(1.408)
Years 5–6	8.422***	6.745***	5.252***	9.922***	5.505***	7.389***
	(1.082)	(1.314)	(1.649)	(1.098)	(1.349)	(1.882)
Years 7–8	7.766***	4.573***	4.177**	10.065***	3.175*	7.111***
	(1.084)	(1.515)	(2.034)	(1.344)	(1.740)	(2.457)
Years 9–10	6.485***	4.178**	3.518	9.858***	3.320	7.691**
	(1.189)	(1.669)	(2.535)	(1.735)	(2.040)	(3.060)
Years 11–12	4.384***	1.669	1.138	4.901**	-3.373	2.370
	(1.186)	(1.840)	(3.087)	(1.927)	(2.284)	(3.724)
Years 13–14	3.204***	0.076	-0.350	1.969	-6.625**	0.544
	(1.197)	(2.025)	(3.712)	(2.281)	(2.669)	(4.589)
Years 15	3.967***	1.770	2.291	4.101	-3.321	4.852
Onwards	(1.135)	(2.254)	(4.583)	(2.744)	(3.313)	(5.756)
By Divorce Law Regime:						
Collection Rate	No	No	No	Yes	Yes	Yes
Average Collections	No	No	No	Yes	Yes	Yes
Paternity Rate	No	No	No	Yes	Yes	Yes
Location Rate	No	No	No	Yes	Yes	Yes
Controls:						
Years Joint Custody	No	No	No	Yes	Yes	Yes
Years JC*Years UD	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
State * time	No	Yes	Yes	No	Yes	Yes
State * timesq	No	No	Yes	No	No	Yes
R^2	0.929	0.960	0.968	0.944	0.966	0.970
Sample	1956–88, n=868 state-years					

Notes: Estimated using state population weights (equal to the denominator of the dependent variable). Standard errors in parentheses. The dependent variable is defined as the annual divorces of couples who have been married for one year per 1,000 marriages one year ago. Data on annual divorces per duration on marriage come from the Vital Statistics of the United States (1956–1967) and the National Center for Health Statistics (1968–1988), the information on the number of marriages is obtained from the Vital Statistics of the United States. Divorce laws coded by Wolfers (2006), http://bpp.wharton.upenn.edu/jwolfers/data.shtml, and Joint Custody laws are coded by Leo (2008). CSE variables are from the OCSE Annual Reports. Significant at the *** 1% level; ** 5% level, and * 10% level.

Table C2- DYNAMIC EFFECTS OF UNILATERAL REFORM. Sample: 1956–1988
(*Dependent variable: Annual divorces of couples who have been married for three years per 1,000 marriages three years ago*)

	(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
First two years	3.712***	3.861***	3.467***	3.589***	3.009***	3.100***
	(1.042)	(1.001)	(0.988)	(0.968)	(0.968)	(0.995)
Years 3–4	2.404**	2.936**	2.113	3.165***	2.673**	2.796**
	(1.068)	(1.162)	(1.304)	(0.996)	(1.135)	(1.369)
Years 5–6	3.273***	4.385***	3.407**	4.500***	3.977***	4.608**
	(1.055)	(1.351)	(1.652)	(1.067)	(1.394)	(1.826)
Years 7–8	4.884***	6.172***	5.026**	6.539***	6.036***	7.324***
	(1.057)	(1.558)	(2.038)	(1.306)	(1.799)	(2.381)
Years 9–10	2.623**	4.694***	3.037	4.742***	4.952**	5.902**
	(1.159)	(1.717)	(2.539)	(1.698)	(2.111)	(2.967)
Years 11–12	1.069	3.623*	1.749	1.398	2.391	3.084
	(1.157)	(1.893)	(3.092)	(1.883)	(2.364)	(3.609)
Years 13–14	0.490	3.617*	1.590	0.749	3.682	3.593
	(1.168)	(2.082)	(3.718)	(2.213)	(2.756)	(4.439)
Years 15	-0.746	4.656**	1.270	-2.348	5.715*	0.588
Onwards	(1.107)	(2.318)	(4.591)	(2.665)	(3.420)	(5.563)
By Divorce Law Regime:						
Collection Rate	No	No	No	Yes	Yes	Yes
Average Collections	No	No	No	Yes	Yes	Yes
Paternity Rate	No	No	No	Yes	Yes	Yes
Location Rate	No	No	No	Yes	Yes	Yes
Controls:						
Years Joint Custody	No	No	No	Yes	Yes	Yes
Years JC*Years UD	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
State * time	No	Yes	Yes	No	Yes	Yes
State * timesq	No	No	Yes	No	No	Yes
R^2	0.904	0.940	0.954	0.920	0.946	0.958
Sample	1956-88, n=868 state-years					

Notes: Estimated using state population weights (equal to the denominator of the dependent variable). Standard errors in parentheses. The dependent variable is defined as the annual divorces of couples who have been married for three years per 1,000 marriages three years ago. Data on annual divorces per duration on marriage come from the Vital Statistics of the United States (1956–1967) and the National Center for Health Statistics (1968–1988), the information on the number of marriages is obtained from the Vital Statistics of the United. Divorce laws coded by Wolfers (2006), http://bpp.wharton.upenn.edu/jwolfers/data.shtml, and Joint Custody laws are coded by Leo (2008). CSE variables are from the OCSE Annual Reports. Significant at the *** 1% level; ** 5% level, and * 10% level.

Table C3- DYNAMIC EFFECTS OF UNILATERAL REFORM. Sample: 1956–1988
(*Dependent variable: Annual divorces of couples who have been married for five years per 1,000 marriages five years ago*)

	(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
First two years	1.638*	3.183***	2.514***	1.218	2.146**	2.081**
	(0.918)	(0.888)	(0.861)	(0.870)	(0.856)	(0.863)
Years 3–4	0.072	2.402**	1.212	0.162	1.553	1.331
	(0.941)	(1.031)	(1.135)	(0.892)	(0.999)	(1.180)
Years 5–6	1.206	4.298***	3.731***	1.309	2.848**	3.400**
	(0.930)	(1.199)	(1.438)	(0.943)	(1.214)	(1.567)
Years 7–8	1.244	5.010***	5.690***	1.196	3.077**	5.216**
	(0.931)	(1.383)	(1.774)	(1.152)	(1.565)	(2.036)
Years 9–10	-0.090	4.615***	4.146*	0.900	3.771**	4.834*
	(1.021)	(1.523)	(2.211)	(1.496)	(1.832)	(2.535)
Years 11–12	-0.776	4.729***	4.222	-0.852	3.318	4.435
	(1.019)	(1.679)	(2.692)	(1.662)	(2.052)	(3.080)
Years 13–14	-2.022**	4.413**	3.896	-2.782	3.856	4.223
	(1.028)	(1.848)	(3.237)	(1.949)	(2.388)	(3.787)
Years 15	-1.245	7.559***	5.206	-3.830	8.486***	4.237
Onwards	(0.975)	(2.057)	(3.997)	(2.341)	(2.955)	(4.738)
By Divorce Law Regime:						
Collection Rate	No	No	No	Yes	Yes	Yes
Average Collections	No	No	No	Yes	Yes	Yes
Paternity Rate	No	No	No	Yes	Yes	Yes
Location Rate	No	No	No	Yes	Yes	Yes
Controls:						
Years Joint Custody	No	No	No	Yes	Yes	Yes
Years JC*Years UD	No	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
State * time	No	Yes	Yes	No	Yes	Yes
State * timesq	No	No	Yes	No	No	Yes
R^2	0.883	0.926	0.945	0.898	0.934	0.951
Sample			1956–88, n=868 state-years			

Notes: Estimated using state population weights (equal to the denominator of the dependent variable). Standard errors in parentheses. The dependent variable is defined as annual divorces of couples who have been married for five years per 1,000 marriages five years ago. Data on annual divorces per duration on marriage come from the Vital Statistics of the United States (1956–1967) and the National Center for Health Statistics (1968–1988), the information on the number of marriages is obtained from the Vital Statistics of the United. Divorce laws coded by Wolfers (2006), http://bpp.wharton.upenn.edu/jwolfers/data.shtml, and Joint Custody laws are coded by Leo (2008). CSE variables are from the OCSE Annual Reports. Significant at the *** 1% level; ** 5% level, and * 10% level.