Unilateral Divorce vs. Child Custody and Child Support in the U.S.

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Abstract:

This paper explores the response of the divorce rate to law reform introducing unilateral divorce after controlling for law reforms concerning the aftermath of divorce, which are omitted from most previous works. We introduce two main policy changes that have swept the U.S. 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 omission might obscure the impact of unilateral divorce reforms on divorce rates. Our results suggest that what has driven the decline in the divorce rate since the 1980s are law reforms concerning the aftermath of divorce rather than a reverse response of divorce rates to the adoption of unilateral divorce laws. Supplemental analysis, developed to examine the frequency of permanent shocks in U.S. divorce rates, indicates that the positive permanent changes in divorce rates can be associated with the implementation of unilateral divorce, and that the negative permanent changes can be related to the law reforms concerning living arrangement in aftermath of divorce. This seems to confirm the important role of those policies in the evolution of divorce rates.

Keywords: Divorce rate, unit root, structural break.

JEL: C12, C22, J12, J18, K36

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I. Introduction

In an article in the *American Economic Review*, Justin Wolfers (2006) finds that reforms concerning divorce law in the U.S.A. that occurred during the 1960s and 1970s had a transitory effect on divorce rates. Specifically, he claims that, after a decade, no effect on divorce rate could be discerned as a result of the implementation of no-fault unilateral divorce. Further, some of his estimates indicate that divorce rate declined 15 years after the reform. Even though the empirical evidence of an effect on divorce rate does not confirm the predictions of Coasian bargaining, Wolfers suggests that these findings can be considered as consistent with the Coasian assumption of efficient bargaining.[1] He argues that the small change observed in the divorce rate may indicate that, in most of the cases, couples are able to bargain efficiently even under unilateral divorce.

A potential concern with the analysis developed by Wolfers is that it omits reforms that introduced changes in divorce settlements.[2] There are two primary aspects of law 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 those reforms analyzed by Wolfers (2006). 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, those are not studied by Wolfers but they may have significance in the evolution of the divorce rate.[3] 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, 2006; and Halla, 2009), previous empirical research found that both changes in the financial obligation of parents and the introduction of joint custody negatively affect divorce rate (see Nixon, 1997; and Brinig and Buckley, 1998).[4] 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.

In the U.S.A., while the share of population covered by the no-fault unilateral reforms already analyzed by Wolfers (2006) increased from the late 1960s, achieving 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 included by Wolfers (2006) to capture the dynamic response of divorce are only picking up the

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[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] 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.

[3] 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).

[4] More recently some studies do not find a significant effect of changes in custody laws and child support on the divorce rate (Halla 2009, and Heim 2003).

path of the adjustment of divorce rates to unilateral divorce. Wolfers observes that the effect of the unilateral divorce law reform on divorce rates had dissipated a decade after the implementation of the unilateral divorce, which coincides with the rise in the incidence of joint custody, see 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, and Leo 2008). In the area of child support, the U.S. 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) again tallying with the time in which a negative response of divorce rates to divorce law reforms is found in Wolfers (2006). We argue that the analysis developed by Wolfers may be measuring the response of divorce rates to changes in divorce laws in addtition to or instead of the response of divorce rates to custody reforms and Child Support Enforcement efforts.

Initially, we attempted to replicate Wolfers' results using data on the divorce rate from 1956 to 1988 but including the reforms that govern the aftermath of divorce. We introduce both child custody law reforms and Child Support Enforcement efforts into Wolfers' analysis. Our results suggest that the long-run effect of divorce law reforms on divorce rate observed by Wolfers may be confounding both unilateral reforms and changes in the aftermath of divorce. 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 maintained even after considering a range of alternative specifications.

As an additional check that the changes in the aftermath of divorce are driving Wolfers' findings, we separate the analysis by group of divorcing couples with and without minors in order to check whether the behavior of the 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 reaction of couples with minors. This is a particularly strong test since custody reforms may have an impact on the number of married people (Halla 2009). However, 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 law have persistent effect on divorce, we explore the frequency of persistent shocks in U.S. divorce rates by exploiting another technique, a time-series analysis.[5] We analyse three possible scenarios (for a review of the literature on structural

[5] The time-series analysis is a technique that has been ignored in most previous work. As exception, we find the work of Marvell (1989) which was the first attempt to develop a complete time-series analysis of divorce rates across the

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 reform, 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 may cause permanent changes in the equilibrium rate itself, but most shocks would 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 would have permanent effects on the level of divorce.

Our results also contribute to a growing literature that evaluates whether shocks have permanent effects on socio-economic variables. Using statistical techniques very 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, 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). We add to this work by presenting empirical evidence of the frequency of permanent shocks in U.S. divorce rates.

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 result suggest that persistent positive changes can be associated in most of the cases with major changes in divorce laws and those permanent negative changes can be related to changes in custody laws and the Child Support Enforcement program, since the break dates and the dates of the reforms are quite close to each other.

The paper is organized as follows. Section II discusses the results of Wolfers (2006). In section III and section IV, we introduce custody law reform and Child Support Enforcement efforts, respectively, into Wolfers' 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.

U.S., finding that the major impact on divorce rates of the change to no-fault laws is delayed for a year, or Ellman and Lohr (1998) who used an intervention analysis. For the case of Europe, we find the works of van Poppel and de Beer (1993) for the Netherlands, and Smith (1997) for Britain. In both cases, they observe evidence of permanent legal effects on divorce rates.

II. Replicating Wolfers

As mentioned above, Wolfers (2006) tests the dynamic response of divorce rate to a change in the legal regime that governs how spouses divorce. To do that, Wolfers uses data on the divorce rate in each state ranging from 1956 to 1988, from *Vital Statistics of the United States*. The divorce rate is defined as the annual number of divorces per thousand inhabitants in each state. He claims that with this sample he is able to determine the dynamic response of divorce to changes in divorce laws that occurred in the U.S.A. from the late 1960s, once he identified the pre-existing state-specific trends. He estimates,

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

where $DR_{s,t}$ is the divorce rate in state *s* in year *t*, the variable $UD_{s,t,k}$ is a dummy, sets equal to one when the 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 while the state-specific time trends identify pre-existing trends.

Panel A of Table 1 simply replicates Wolfers' 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 appears 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 seem to be not quite 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 legal reforms that occurred in the U.S.A. have a transitory effect on the divorce rate.

The dynamic response after a little more than a decade, certainly, seems at odds. It is difficult to establish a clear causal link between the liberalization of divorce law and the fall in divorce rates since the 1980s, correlation does not automatically imply causation. Dummy variables added by Wolfers to pick up the dynamic response of divorce may include not only the reaction of divorce rates to laws that regulate how to obtain a divorce, but also the response of those divorce rates to changes in laws that govern the aftermath of divorce, the implementation of a joint custody regime and the Child Support Enforcement efforts.

III. Joint Custody Regime

Why does a 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 backward step 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).[6] 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.[7] Although the unilateral 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 of direction of the compensation; 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 the mutual consent will be (Bartlett and Stack 1991).

In Coasian terms, both reforms consist of reassignments of property rights between spouses which should not affect 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.

While between 1968 and 1977 28 states passed to a no-fault unilateral system, from 1979 what swept the U.S.A. was the introduction of a joint custody regime (Folberg 1991). In

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[6] We do not discern here either between various forms of joint custody such as "joint legal custody" (both parents share the right and the obligation of making major decisions about their child's upbringing in issues such as religion, health and education) and "joint physical custody" (the child spends a significant amount of time with each parent), nor 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.

[7] We do not aim to study how gender disparities introduced by the new law reforms affect the evolution of the divorce rate. 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 unilateral divorce and a sole-custody regime. 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. On the other hand, 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 with mutual consent in the custody of their child. Independently of these gender disparities, the necessity of cooperation and mutual consent in the custody of children may lead to a reallocation of property rights.

1988, approximately 37 states had some form of joint custody statute.[8] This second wave of reforms seems to have affected the divorce rate of those states that also had introduced a unilateral reform, 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 (7 states), those with only a joint custody reform (14 states), and those states which did not change either divorce laws (6 states).[9] The long-dashed and short-dashed lines show the evolution of the difference in the average divorce rate between those states that introduced any reform, unilateral reform, joint custody reform or both, with those which did not pass a reform. These lines allow a comparison of the different evolution of average divorce rate by states which approved different aspects of law relevant to divorce. If anything, it is clearly observed that the decline in the average divorce rate occurs in those states that introduced both reforms, unilateral and joint custody regime, hence it seems that child custody law reform has neutralized the effect of unilateral divorce on divorce rates. On the other hand, 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 reform. This simple comparison suggests that the dynamic response of divorce that is proposed by Wolfers may be confounding the reaction to the changes in custody law with a reverse response of divorce rates to the adoption of unilateral divorce laws.

The divorce rate of those states that only passed a joint custody regime also seems to fall with respect to the divorce rate of those states which do not introduce any reform, see Figure 2. Empirically, this may affect the estimates of the trend made by Wolfers, it may have confused the decline in the average divorce rate produced by the implementation of a new custody regime with a negative trend in the evolution of the divorce rate of those states that did not introduce unilateral divorce, since both states that passed the joint custody regime and those that did not pass any reform are considered as states without reforms in Wolfers (2006).

From a theoretical point of view, the fall in the divorce rate of those states that only introduced custody reforms may be due to an increase in the cost of divorce. As Morrow (1991) remarks, when parents share physical custody after divorce, total costs are further increased since some of the major expenses are duplicated. The 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, and Folberg 1991). On the other hand,

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

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

the divorce rate can also decline when investment in child quality increases under a joint custody regime and the benefits from child quality are considered as marriage-specific investments (Rasul 2006).

Whether a joint custody regime affects the divorce rate is an empirical question which has received hardly any attention among researches. The first attempt to test this relationship was accomplished by Brinig and Buckley (1998), who found a negative effect of joint custody laws on divorce rates. This result has been rebutted, more recently, by Halla (2009). He does not find convincing evidence that the joint custody regime significantly affects divorce rates when adding a set of dummies for joint custody law à la Wolfers:

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

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. Panel B of Table 1 shows results 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' analysis. Concretely, the decline of divorce rates due to the unilateral divorce reform 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 quite 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. Therefore, those results generate doubts about what is being captured by the dummy variables included in Panel A of Table 1.

Alternatively, we can test whether divorce rate really decreases after the implementation of unilateral divorce just by focusing on those states that only passed unilateral divorce reforms. We would expect to observe a change in the sign of the coefficients if the β_k coefficients of equation (1) are measuring the effect of the joint custody regime in addition to or instead of the unilateral divorce. To formalize these ideas, consider the following equation:

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

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 has adopted unilateral divorce *k* years ago in year *t* and $JC_{s,t,r}$ is a dummy equal to one when a state has 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 β_k in the subsequent periods after the adoption of unilateral divorce should be positive, but then it should turn negative. In contrast, if divorce rate do not decrease as a result of the adoption of unilateral divorce then β_k should be always positive or non-significant.[10]

Table 2 presents 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. 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 speak to some of the potential sources of bias in Wolfers' 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 reform 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 the specification (3) in Panel B of Table 1, although the impact is greater for those states that just introduced unilateral divorce systems. Again, our results generate doubts about what is being picked up in the model implemented by Wolfers to analyse 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' results because the divorce rate includes a sub-population that is not affected by the joint custody reform. The necessity of mutual consent required by the joint custody reform is limited to couples with minors, but the divorce rate includes both couples with children and couples without children.

[10] Although we are not interested in the effect of joint custody on 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 only introduced custody law reforms. On the other hand, 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 reform on divorce rate, when the increase in divorce rate following unilateral divorce reform is reversed due to 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 reform would be cancelled by the joint custody regime. $\beta_k + \gamma_{k,r}$ is capturing the dynamic effect of unilateral reforms for those states that introduced both unilateral divorce and joint custody reform.

This is problematic since the behavior of the sub-population not affected by the custody law reform could be driving our results instead of the reaction of couples with minors to custody law 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 due to the scarcity of data. The 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 (DRA) 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 and both reforms.[11] Clearly, we observe higher divorce rates for couples with children (red and black lines) and a decreasing trend in the divorce rate for couples with children (black line) but not in the divorce rate of couples without children (blue line) from the early 1980s when the joint custody law was adopted by most of the states.[12] The evolution of the difference between the average divorce rate of couples with and without children (see long-dashed and short-dashed lines) is maintained as quite similar for all three kinds of reforms from the 1960s. 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). 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 or a differential distribution of divorces among couples with and without children across states that implemented different divorce law reforms.

To probe this further, we rerun equation (1) and equation (3) using as dependent variable the divorce rates among childless couples and among couples with children, with data for all states belonging to the divorce-registration area (DRA).[13] 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 reform 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 rate to unilateral reform when we run equation (1) and (3) for couples without minors.

[11] 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.
[12] 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 the 1970s. Thus, we would not expect that this change was determined by the custody reforms.
[13] We also ran all the analyses using only data for those states belonging to the DRA and the results are quite similar. However, we prefer to use data for all the states to make our findings comparable to previous works.

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 reform 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.

Because we would not expect joint custody reform to have any effect on the divorce rates of childless couples, the 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 minor children. Immediately, it can lead to a decrease in the number of divorces since there are fewer opportunities outside marriage to find someone to remarry due to the increase in married population (marriage rates having increased as a result of the adoption of new custody laws (Halla, 2009)). 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 the subsequent periods. In Figure 4, we observe an increase in the coefficients of the unilateral divorce reform when controls for the joint custody reforms and state specific trends are added, with this being ten years after the approval of unilateral reforms. This suggests that those coefficients might be capturing second-order effects of joint custody on marriage rather than the unilateral divorce reforms alone. We can then detect a decrease in the effect of the unilateral divorce reform when the same controls are added. Again, this could be due to the fact that the coefficients are capturing second-order effects of custody reforms in addition to or instead of unilateral divorce.

The decline in the divorce rate for couples with minor 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 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.

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 factors associated with a rising divorce rate are more likely in states that did not introduce divorce reforms, and 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, 2009), 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) found that the divorce rate may be influenced by and may itself influence female earnings. To make our results comparable with previous analysis we do not introduce these socio-economic variables into the analysis.

IV. Child Support Enforcement

The analysis presented in the previous subsection has left out the third wave of transforming aspects of law relevant to divorce that has occurred since the mid-1970s when the U.S. 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 (CSE) Program as Title IV-D of the Social Security Act in 1975.[14] This law created a separate division, the federal Office of Child Support Enforcement (OCSE), to oversee the operation of a Child Support Enforcement program and required each state to establish a Child Support Enforcement 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 the states to revise and expand CSE services and techniques.[15]

[14] 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.

[15] See Garfinkel et al. (1998) for a review of child support policies in the U.S.A.

The Child Support Enforcement Amendments of 1984 required every state's Child Support Enforcement Agency (CSEA) to 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 to implement various procedures for immediate and mandatory wage-withholding for all support orders being enforced by every State's CSEA. By 1994, states were required to provide for immediate withholding of wages for all support orders (regardless of whether IV-D services were used or payments are in arrears). The Child Support Recovery Act of 1992 imposed a federal criminal penalty for the wilful 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 the 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, which is a crucial first step in child support cases, 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 Child Support Enforcement 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 location 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 to 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 in interstate or foreign commerce 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 wilfully 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 will be 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 (see Figure 5), the incorporation of these reforms seems to be necessary to estimate precisely the effect of no-fault unilateral reform 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 were living with their mother and this fact did not considerably change in the period considered, see Figure 6. Therefore, changes in the financial obligation of non-custodial parents, i.e., child support, might play a more important role in divorce.[16]

It is possible that what is being captured by the dynamic response of divorce rates to divorce law reform is the application of Child Support Enforcement 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 in accounting 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 child support enforcement efforts are executed by state authorities (for a review of state statutes, see Sorensen and Halpern (1999)). 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 child support enforcement. Couples that live in states that passed joint custody law or where they cannot unilaterally divorce might fail less in their child support obligations due to the necessity of mutual consent in child custody. Therefore, those states that only introduced unilateral divorce would need to be stricter in putting child support enforcement into effect to achieve their objective of reducing child poverty and welfare costs.

[16] 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).

We use state-level administrative data provided by the Office of Child Support Enforcement (OCSE).[17] The status of the application of the child support enforcement in all states considered in the analysis is reported yearly from fiscal year 1977 by the OCSE.[18] Four different variables are used to represent the effectiveness of the child support enforcement program. As Nixon (1997) and Heim (2003) have done, we analyze the effect of enforcing child support orders and increasing collections by using the *collection rate* variable, defined as the percentage of CSE cases in which a collection was made by obligation, and by including the *average collections*, calculated as the dollars collected per CSE case divided by state per capita GDP. Following Heim (2003), we have also included two more variables to control for differential effects of the 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.[19] 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 the average state that passed any other divorce law reform. A similar pattern is also observed for both paternity rate and location rate. On average, those 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.[20] As can be seen in Table 4, the results

[17] Although OCSE data includes detailed information on CSE programs, parents not utilizing OCSE services are not included in its publications; see Guyer et al. (1996). This can affect our estimates if greater presence of OCSE non-users is associated with less restrictive divorce laws. Those states 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 with 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, there is no data set that contains information on both parents using OCSE and non-users. Our estimated effects of CSE efforts on divorce rate would not capture the CSE effect well, and we would expect that the CSE programs more negatively affect the divorce rate, thus the dummy variables capturing the effect of unilateral divorce reforms still pick up the unilateral divorce effect in addition to the CSE efforts.

[18] The data come from the third Annual Report to the U.S. Congress on the Child Support Enforcement 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 is not included in the analysis since it differs in the period covered, from January 4, 1975 to June 30, 1976. For the same reason, we do not include data from the special supplementa1 report which was issued to cover the period July 1 to September 30, 1976. Information from the second report is not included since the average annual child support enforcement caseload is not available.

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

[20] All those measures of CSE efforts take a value of 0 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 have also repeated the analysis by using only data from 1978 and the results do not change substantially, we observed no effect of unilateral divorce on divorce rate in the long run.

do not differ from those observed when we just introduce controls for custody reforms in all the specifications (Table 2). The dynamic response of divorce rates to unilateral divorce reform 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 decade. However, when controls for state-specific quadratic trends are added, the rise in divorce rate following the introduction of unilateral divorce reform seems to be permanent.

Another strategy to capture the effect of the Child Support Enforcement efforts consists in individually considering the effect of the child support reinforcement by 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 divorce rate by 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 on 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 ten 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 have also looked at the effect of other CSE policies, 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, 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 on the dynamic effect of divorce law reform, 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 quite robust. Therefore, the long-run effect of unilateral divorce on divorce rate observed by Wolfers (2006) seems to be capturing the effect of 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 variable the divorce rate 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 reform 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 differences can be due to second-order effects (Halla 2009). 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 picking 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 due to the fact that 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) showed, welfare benefits had a small effect on the probability that a married woman will become divorced, thus it is not a significant determinant of divorce decisions.

V. Permanent shocks in U.S. 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.[21] 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 of the approval of unilateral divorce laws is persistent. Our results are sensitive to the inclusion of state-specific trends.[22] Thus we use an alternative econometric technique that has been used to track the evolution of economic and social variables subject to public and legal interventions like the unemployment rate (Mitchell, 1993; Papell et al., 2000) or the rate of crime (Narayan et al., 2005), and to study the effect of policy interventions: the Boston Gun Project (Piehl et al., 2003) or Public Interest Litigation in India (Rathinam and Raja, 2008).

[21] Note that permanent means here that the change is still in effect given a sample of data, but not that the change will last forever.
[22] This weakness is also observed in Wolfers (2006); the effect of divorce law on divorce rate is also quite sensitive to the introduction of state-specific quadratic trends.

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 as 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 a policy reform has had a permanent impact on divorce rate, we simply compare the timing of the reforms with the break dates in the stationary divorce rate series.

Unit Roots in U.S. 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).[23] 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 will be a stationary time series and any shock will dissipate over time.[24] 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) will 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; and Gujarati, 1995).

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

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

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.[25] 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

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[23] 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 have 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.

[24] 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 actual time at which the covariance is calculated.

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

0, then the divorce rate series will follow a random walk. If, on the other hand, 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. Results suggest that the unit root scenario seems to describe the experience of the U.S. divorce rates best. When using Augmented Dickey-Fuller (ADF) tests, the null hypothesis of a unit root in the divorce rate is not rejected for four out of fifteen states, or 8 percent of the states, at the 10 percent level of significance.[26] For these four states, fluctuations are transitory but for the rest of the states any sudden shock has permanent effects on divorce rate. Although the ADF tests are widely used, these common tests are biased towards the nonrejection 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 in the next subsection.

Robustness Checks: Panel Unit Root Test

We have also considered 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 heterogenous panels with cross-section dependence. Pesaran's CADF eliminates the cross-dependence by augmenting the standard DF (or ADF) regressions with the cross-section averages of lagged levels and with first-differences of the individual series. Parallel 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 the 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 process. When using the Levin–Lin–Chu panel unit root test and the Im–Pesaran–Shin test, we cannot reject the null hypothesis of a unit root even at the 10 percent level. However, the Pesaran’s test shows that, when controlling for cross-sectional dependence, the null hypothesis of unit root is rejected at the 1 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 weaker.

[26] We also ran ADF tests incorporating a trend and the results are quite consistent.

Unit Roots in the Presence of a Structural Break

In the presence of a one-time structural break, the standard ADF tests are biased towards the nonrejection of the null hypothesis due to 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 a 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 nontrending data.[27]

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).[28] 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. 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 U.S.A. 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 quite 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 states. Thus, the results suggest that there is not a single scenario. These findings do provide evidence in favor of both unit root processes and stationary processes subject to a structural break.

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[27] Other papers in which the breakpoint selection is also endogenized are Banerjee et al. (1992) and Zivot and Andrews (1992).

[28] Since Wolfers (2006) found different short-run and long-run effects of 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 models (IO) which allows for gradual changes in divorce rates. Our results are quite similar, although some of the structural breaks are detected some years later than those determined when using the AO model.

Table 10 reports the results by state. The null hypothesis of a unit root is rejected for Arkansas, Delaware, Mississippi and South Dakota at the 1 percent level, for Hawaii, Michigan, Montana, New York, North Dakota, Oregon, Pennsylvania, South Carolina, Utah, Vermont and Washington at the 5 percent level and for Alabama, Georgia, Idaho, Iowa, Massachusetts, Minnesota, New Jersey, Texas and West Virginia at the 10 percent level. For these stationary states, most shocks cause temporary movements of the divorce rate around the equilibrium level, but occasionally a shock causes permanent changes in the equilibrium rate. All these structural breaks are positive, which reflects the rise in the divorce rate among the states, and all but two break dates are grouped around the late 1960s and the early 1970s. For the rest of the states, the nonstationary states, all shocks have permanent effect on the level of divorce, thus, there is no tendency to return to a stable value.

Multiple Structural Breaks

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).[29] For the case with no trending regressors, we first estimated the linear regression with only a constant as regressor:

$$DR_t = \mu + \delta DU_t + \eta_t \tag{8}$$

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.[30] We allow for heterogeneity and autocorrelation in the residuals. The method utilized is Andrews' (1991) automatic bandwidth with AR(1) approximation and the quadratic kernel. It is

[29] We have also repeated the analysis of unit root considering the presence of two endogenous break points by using the methodology developed by Clemente et al. (1998). The results are quite consistent and are available upon request. Because of the short timespan 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).

[30] For those U.S. states in which the sequential procedure found no break since the $\sup F_T(1)$ test is 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).

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

Table 11 presents the significant break dates at the 5 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 5 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 not more than three years of difference between the break dates chosen by the one-time break test of unit root and those found by the Bai and Perron procedure.

There are several aspects of these results to which it is worth drawing attention. Our findings provide strong empirical evidence against the view that all shocks have temporary effects on divorce. For all of 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 compensated with 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.[31] 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.

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[31] It is likely that the methodology applied here was unable to detect breaks in the late 1980s and 1990s due to the proximity of the end of the sample. Once we extend 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 sign of the breaks do not change; it is 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 the 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.[32] 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. We focus on comparing the timing of the main policy reforms and the timing of the structural breaks which 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 unit root. Of these 24, a total of 13 have a break that is located close to the time of the divorce law reforms that were passed beginning in the 1970s. Only for the case of South Dakota is no a structural break in the divorce rate detected close to the adoption of the unilateral divorce law in 1985. For five of the thirteen U.S. 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), the breaks are found before the reform although the reform dates are included in the confidence interval at the 95 percent. 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 reform in the period analysed.

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[32] 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 drop Nevada since the behavior of the divorce rate is clearly different to that of the rest of the states and may be driving the results. In another specification, we drop Kentucky since the divorce rate seems to have increased in this state during the 1980s which can affect our estimates of the dynamic effect of the divorce rate on the unilateral divorce reforms. The results are quite consistent and are available upon request.

The structural breaks chosen in the 1960s and 1970s for the other ten 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 those policy changes. One can conjecture that other major events caused all the permanent changes in that period since those changes are similar in all the states independently 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.[33] 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 the 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 due to the rise in the number of widows (Philips, 1988). Fourteen of the fifteen breaks are found in the Vietnam War and post-war period in those states without divorce law reform. 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 reform. 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 of the approval of custody reform. 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 previously, but it is close to the changes in the CSE program.[34] Thus, a decade after the unilateral divorce reform, what seems to conduct the behavior of the divorce rates are those reforms on the laws that govern the aftermath of divorce which can be associated with negative permanent shocks in the divorce rate.

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

[34] 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.

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. Finally, the last wave of changes is negative, as previously, tallying with the custody law reforms and with the increase in the CSE efforts.

VI. Conclusions

This paper aims to disentangle the effects of reform of laws that govern the aftermath of divorce from the effects of unilateral divorce in determining the behavior of U.S. 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 U.S.A. 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 Child Support Enforcement 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, according to the Coase theorem. Under joint custody, parents have to collaborate and cooperate in decisions affecting the child; this implies a backward step to a situation in which mutual consent is necessary. It is not possible to leave out of this analysis the Child Support Enforcement program either. The increase in the efforts to try to ensure child support payments is relevant in 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 negative evolution of the divorce rate since the 1980s is due to law reforms concerning the aftermath of divorce rather than a reverse response of divorce rates to the implementation of unilateral divorce laws. However, even after controlling for joint custody regime and Child Support Enforcement efforts, it is unclear whether the effect of the unilateral divorce reform on divorce rate is transitory or not. Using several techniques, we find evidence of both a lasting effect and a permanent effect. 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 have also developed a supplemental analysis to explore the frequency of permanent shocks in U.S. 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.

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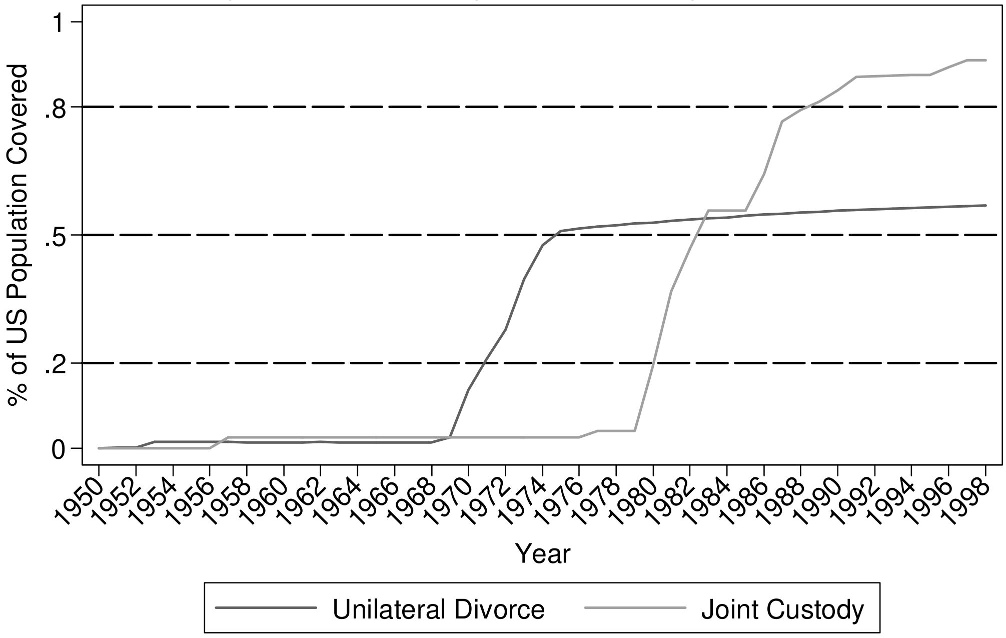
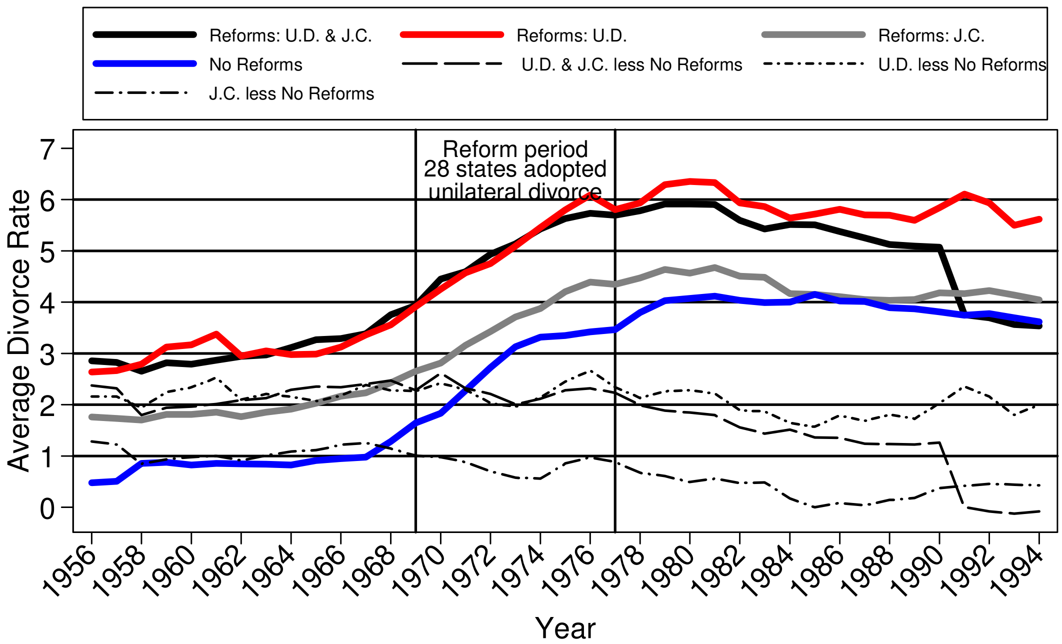
Figure 1: Coverage and Timing of Reforms



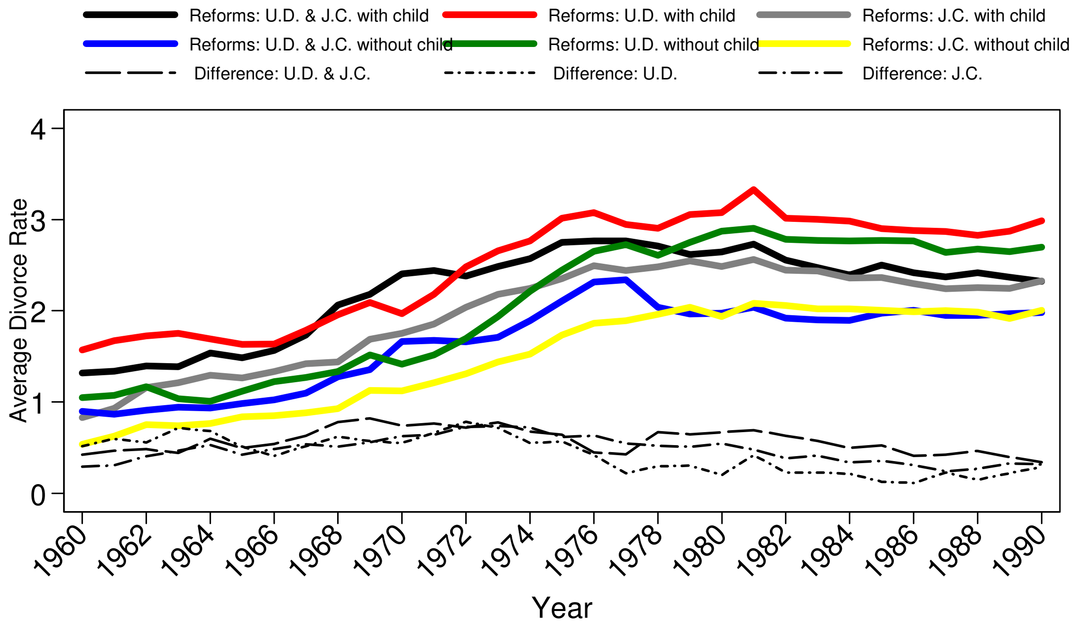
Source: US Census Bureau, Population Estimates. See a similar figure in Leo (2008)

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

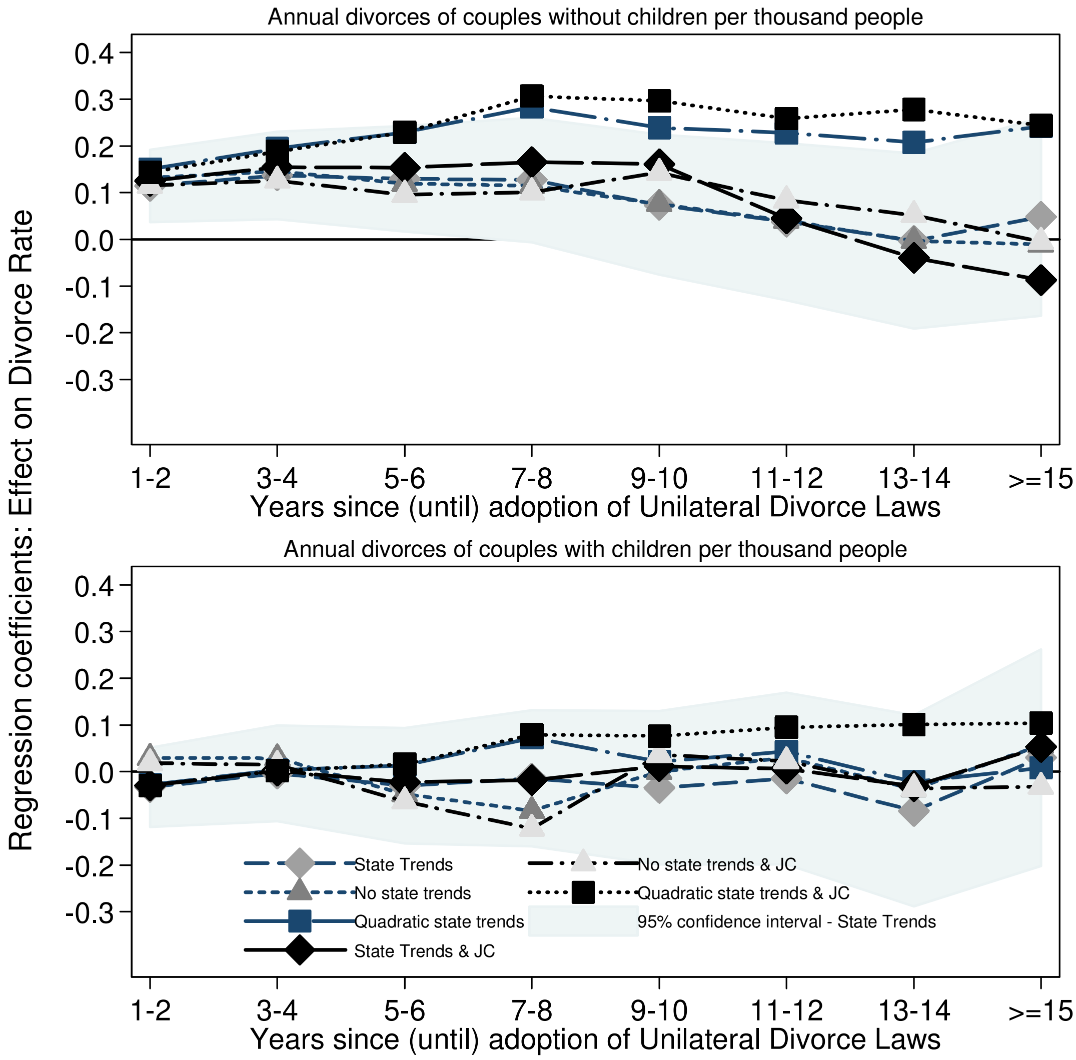
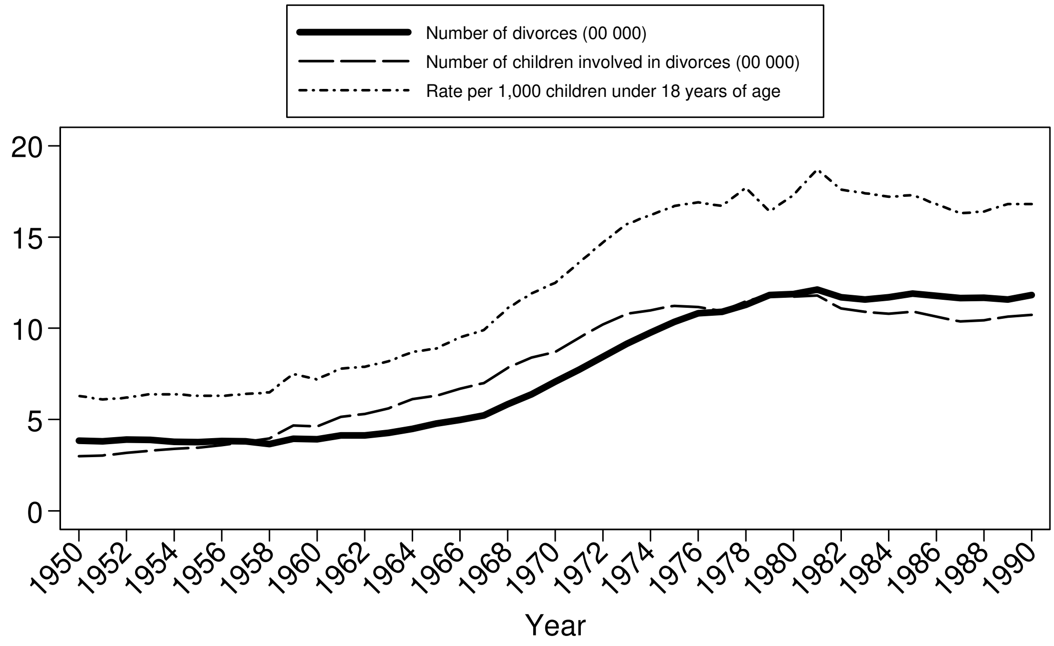
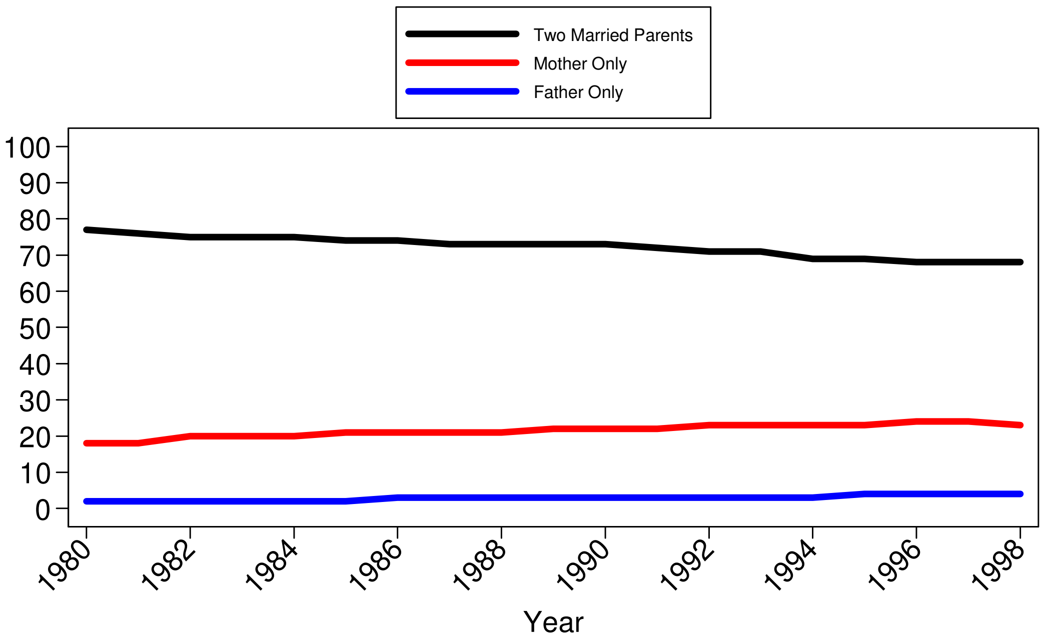


Figure 5:
Number of divorces and children involved in divorces



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

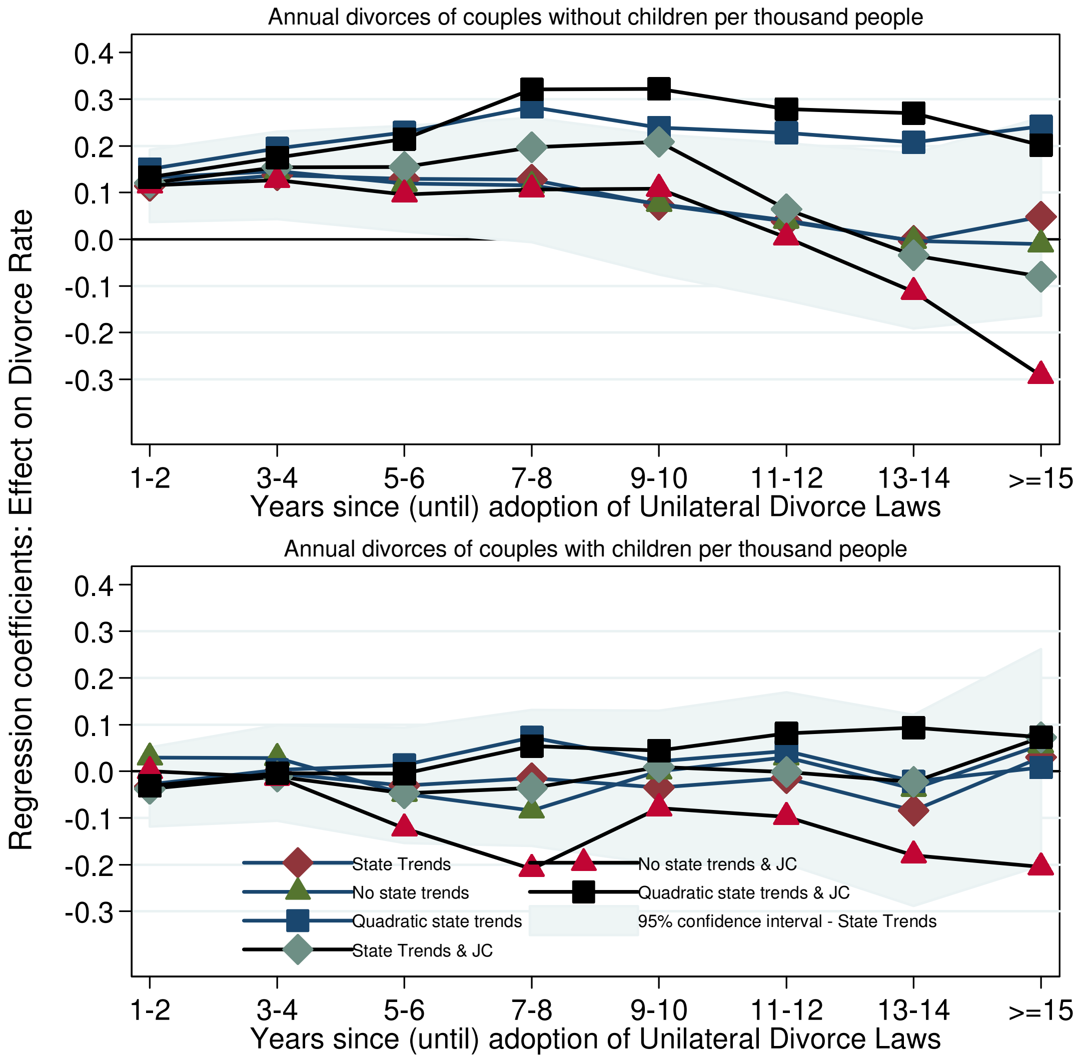


Table 1- WOLFERS' RESULTS AND DYNAMIC EFFECTS AFTER ADOPTING JOINT CUSTODY LAWS

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

	(1) Basic specification	(2) State-specific linear trends	(3) State-specific quadratic trends
Panel A			
First 2 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
Adjusted R^2	0.935	0.975	0.984
Sample	1956-88, n=1631 state-years		
Panel B			
First 2 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
Adjusted 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), see. http://bpp.wharton.upenn.edu/jwolfers/data.shtml Divorce laws coded by Wolfers (2006), see http://bpp.wharton.upenn.edu/jwolfers/data.shtml, and Joint Custody laws are coded by Leo (2008).

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 2 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
Adjusted 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), see. http://bpp.wharton.upenn.edu/jwolfers/data.shtml Divorce laws coded by Wolfers (2006), see http://bpp.wharton.upenn.edu/jwolfers/data.shtml, and Joint Custody laws are coded by Leo (2008).

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 comes 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 2 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
Adjusted 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), see. http://bpp.wharton.upenn.edu/jwolfers/data.shtml CSE variables are from the OCSE Annual Reports. Divorce laws coded by Wolfers (2006), see http://bpp.wharton.upenn.edu/jwolfers/data.shtml, and Joint Custody laws are coded by Leo (2008).

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 2 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
Adjusted 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), see. http://bpp.wharton.upenn.edu/jwolfers/data.shtml CSE variables are from the OCSE Annual Reports. Divorce laws coded by Wolfers (2006), see 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.

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 comes 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 2 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	Adjusted 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), see. http://bpp.wharton.upenn.edu/jwolfers/data.shtml CSE variables are from the OCSE Annual Reports. Divorce laws coded by Wolfers (2006), see http://bpp.wharton.upenn.edu/jwolfers/data.shtml, and Joint Custody laws are coded by Leo (2008).

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 2 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	-0.576***	0.016	0.054	-0.042	0.254*	0.123
Onwards	(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
Adjusted 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), see. http://bpp.wharton.upenn.edu/jwolfers/data.shtml CSE variables are from the OCSE Annual Reports. Divorce laws coded by Wolfers (2006), see http://bpp.wharton.upenn.edu/jwolfers/data.shtml, and Joint Custody laws are coded by Leo (2008).

Table 9- RESULTS OF 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–Lin–Chu (2002)	-1.109 (0.133)	
Im–Pesaran–Shin (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 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 off 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–Lin–Chu, Im–Pesaran–Shin and Pesaran 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 UNIT ROOT TESTS ON DIVORCE RATES BY STATE, ONE STRUCTURAL BREAK TEST.

State	δ	$(\hat{\rho}-1)$	Structural Break Year
Alabama	2.15027***	-0.391*	1973
Alaska	2.90367***	-0.079	1971
Arizona	1.36532**	-0.197	1958
Arkansas	3.35513***	-0.676***	1968
California	1.97197***	-0.208	1966
Colorado	2.24497***	-0.344	1968
Connecticut	2.16467***	-0.361	1971
Delaware	2.91339***	-0.691***	1970
District of Columbia	2.47464***	-0.196	1967
Florida	1.70587***	-0.105	1973
Georgia	2.81440***	-0.409*	1970
Hawaii	2.42453***	-0.659**	1968
Idaho	2.02264***	-0.407*	1970
Illinois	1.46082***	-0.110	1960
Indiana	3.10658***	-0.476	1969
Iowa	1.74796***	-0.463*	1969
Kansas	2.28588***	-0.316	1971
Kentucky	2.16189***	-0.264	1976
Maine	2.15592***	-0.258	1971
Maryland	1.55846***	-0.351	1969

Massachusetts	1.44719***	-0.482*	1969
Michigan	1.93480***	-0.425**	1969
Minnesota	1.99516***	-0.477*	1970
Mississippi	2.40508***	-0.722***	1970
Missouri	2.01833***	-0.358	1970
Montana	1.40091***	-0.211**	1977
Nebraska	2.02833***	-0.510	1970
Nevada	-12.98665***	-0.217	1972
New Hampshire	2.59753***	-0.361	1970
New Jersey	2.27435***	-0.521*	1969
New Mexico	3.07653***	-0.232	1965
New York	2.63238***	-0.607**	1970
North Carolina	2.84530***	-0.564	1971
North Dakota	2.03803***	-0.474**	1971
Ohio	2.10924***	-0.274	1971
Oklahoma	2.03977***	-0.337	1969
Oregon	1.49943***	-0.146**	1976
Pennsylvania	1.75424***	-0.536**	1971
Rhode Island	2.16108***	-0.589	1972
South Carolina	2.49858***	-0.433**	1972
South Dakota	2.20330***	-0.446***	1972
Tennessee	3.31939***	-0.629	1969
Texas	1.84297***	-0.149*	1967
Utah	2.28857***	-0.371**	1968
Vermont	2.81538***	-0.560**	1970
Virginia	2.21250***	-0.514	1972
Washington	2.33676***	-0.295**	1965
West Virginia	2.64602***	-0.567*	1972
Wisconsin	1.99106***	-0.444	1972
Wyoming	2.74816***	-0.287	1968

Notes: One-break test of Perron and Vogelsang (1992), AO model.

$(\hat{\rho}-1)$: Ho: Unit root, rejected at ***1% level, **5% level, *10% level

Structural Break Year dummy variable coefficient d_1: Significant at the ***1% level, **5% level, *10% level.

Table 11- 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				
Hawai	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 timespan 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 the 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.