J Popul Econ 21:485–503 (2008)
DOI 10.1007/s00148-006-0119-2

ORIGINAL PAPER

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# Divorce law and family formation

Received: 13 September 2004 / Accepted: 31 March 2005 / Published online: 22 December 2006


**Abstract** Several studies have investigated whether unilateral divorce laws raised divorce rates, with mixed results. This paper asks whether unilateral, and no-fault, divorce laws influenced family formation. Besides their interest to policy makers, such effects may help theorists understand the mechanisms through which laws affect behavior. The results suggest that no-fault laws slightly reduced fertility, and unilateral divorce modestly increased divorce and legitimacy. However, the pattern of effects is not consistent with any of the hypotheses reviewed, and the estimated magnitudes suggest that changes in divorce law were not a major cause of changing family structure.

**Keywords** Divorce Law · Marriage · Fertility

**JEL Clasification** J12 · J13

## 1 Introduction

It is well-known that divorce rates in the United States (and many other countries) rose sharply between the mid-1960s and 1980 and remain high by historical standards. Many people attribute this trend to changes in divorce law that occurred during the same period, but skeptics argue that other factors were more important and that the timing of the legal reforms was either coincidental or endogenous. An extensive empirical literature has attempted to assess those competing claims,

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but thus far, results have been mixed, and the issue remains unresolved even three decades later.

This paper brings new evidence to that debate by measuring the effects of divorce law on marriage, fertility, and legitimacy. In contrast to the multitude of studies that have examined the effect of divorce law (especially unilateral divorce) on the divorce rate, very little empirical work has investigated the relationship between divorce law and these family formation behaviors.[1] In addition to producing new information that may have some direct policy relevance, it is hoped that these estimates will guide future research in at least three ways. First, if we accept the idea that the decision to form families depends, in part, on the risk that those families may dissolve in the future, then the existence and magnitude of effects on family formation will provide evidence on the question of whether divorce law influences the divorce rate. (We shall also briefly address that question directly.) Second, the nature of any effects we find may shed some light on the best way to model family structure decisions and the mechanisms by which divorce law influences them. Finally, the magnitude of these effects will help us to evaluate whether changes in divorce law were an important reason for the trends over recent decades away from traditional family arrangements.

The discussion proceeds as follows. The following section briefly reviews the behavioral and legislative history. Section 3 discusses several hypotheses about the effects of divorce law on divorce rates and explains how those hypotheses extend to family formation decisions. Section 4 then presents the empirical methods and data to be used, and the empirical results are presented in Section 5. The final section evaluates the results and suggests some avenues for additional research.

## 2 Background

Before modern reforms, courts would grant a divorce only if (a) it found that one spouse committed a specific violation against the other (typically something like adultery, chronic drunkenness, or physical abuse), and (b) both spouses agreed to the divorce. Over the past century, all U.S. states have adopted “no-fault” divorce laws that eliminate the former requirement, so courts can now grant divorces without finding either spouse guilty of marital fault. In addition, 34 states now allow one spouse to obtain a divorce without the consent of the other—what is called “unilateral divorce.” Gruber (2004) has reviewed the legislative history in each state to determine the precise timing of these reforms, and his findings are summarized in Table 1. Perhaps surprisingly, in many states, no-fault divorce preceded unilateral divorce by several decades. For example, four states had passed no-fault reforms by 1910 (many decades before the first unilateral reforms), but three of them (Rhode Island, Texas, and Wisconsin) did not enact unilateral divorce legislation until the 1970s, and the fourth (North Carolina) never did. The unilateral divorce laws were also adopted over a much more concentrated interval; 29 of the 34 states to pass such a reform did so between 1967 and 1978. Only about half of the no-fault reforms occurred during that period.

[1] Binner and Dnes (2001) and Gruber (2004, Table 3) address the topic briefly, but their papers are primarily about other issues.

**Table 1** Timing of states' no-fault and unilateral divorce reforms

| Years | No-fault divorce | Unilateral divorce |
|---|---|---|
| pre-1912 | NC, RI, TX, WI | |
| 1913–1915 | | |
| 1916–1918 | LA | |
| 1919–1921 | WA | |
| 1922–1924 | | |
| 1925–1927 | | |
| 1928–1930 | | |
| 1931–1933 | AZ, NV, MN, NM | NM |
| 1934–1936 | AK | AK |
| 1937–1939 | AR | |
| 1940–1942 | | |
| 1943–1945 | UT, ID | |
| 1946–1948 | | |
| 1949–1951 | | |
| 1952–1954 | OK | OK |
| 1955–1957 | DE | |
| 1958–1960 | VA | |
| 1961–1963 | KY, TN | |
| 1964–1966 | HI, DC | |
| 1967–1969 | NY, KS, MD, SC, VT, WV | NV, DE, KS |
| 1970–1972 | CA, IA, AL, FL, NH, NJ, ND, OR, CO, MI, NE | TX, CA, IA, ID, AL, FL, NH, ND, OR, KY, HI, CO, MI, NE |
| 1973–1975 | CT, GA, IN, ME, MT, MO, OH, MA | AZ, CT, GA, IN, ME, MT, WA, MN, RI, MA |
| 1976–1978 | WY, MS | WY, WI |
| 1979–1981 | PA | |
| 1982–1984 | IL | |
| 1985–1987 | SD | SD, UT |
| Never | None | AR, DC, IL, LA, MD, MS, MO, NJ, NY, NC, OH, PA, SC, TN, VT, VA, WV |

Source: Gruber (2004).

In spite of these differences, much of the empirical literature does not distinguish between these two types of reforms, instead, using the terms "unilateral" and "no fault" almost interchangeably to refer to the large number of laws passed in the late 1960s and early 1970s. Although this approach often seems to confuse the issues,[2] it is, at least, somewhat sensible in that the reforms from that period are often blamed for causing the contemporaneous movement away from traditional family arrangements.

As Fig. 1 shows, family formation and dissolution behaviors have changed dramatically in the U.S. since 1950, with the largest changes occurring between the mid-1960s and the mid-1970s.[3] The divorce rate famously doubled over that period, and the birth rate fell about 25% between 1965 (usually considered the

[2] Whittington and Alm (2003, pp. 87–90) conclude that much of the confusion in the literature stems from imprecise terminology about the laws governing divorce.

[3] The divorce rate trend in Fig. 1 excludes data from California, Indiana, and Louisiana in all years because none of them has reported divorce data since 1990. The out-of-wedlock birth rate trend excludes data in all years from 18 states that regularly did not report data before 1980.

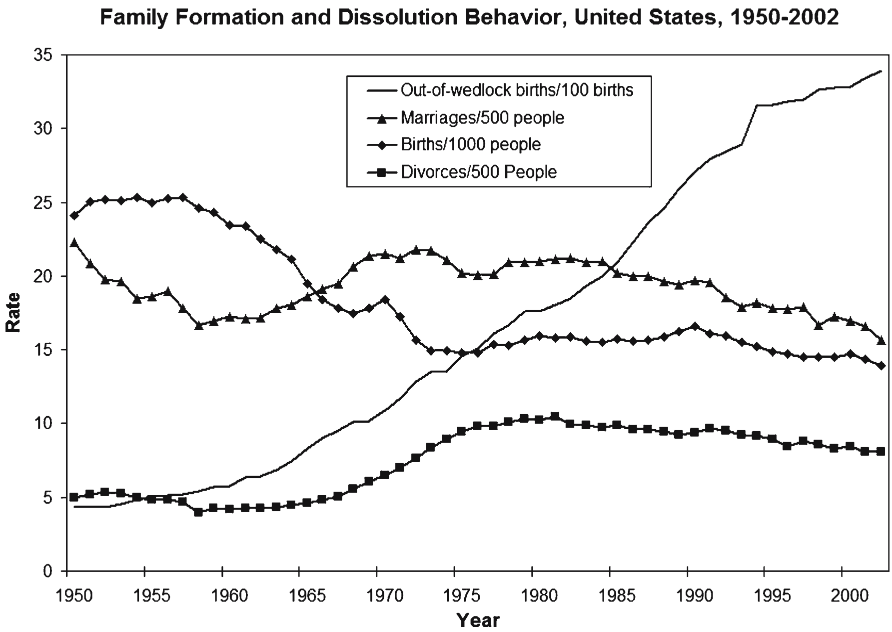


**Fig. 1** Trends in family formation and dissolution rates, United States, 1950–2002

first year *after* the Baby Boom) and 1975. The percentage of children born out of wedlock has been increasing for decades, but it grew the fastest between 1963–1975 (with an annual growth rate of 6.0% vs 3.5% in 1950–1962, 3.8% in 1976–1988, and 2.0% in 1989–2002). The marriage rate also grew about 18% over those years, but unlike the other variables, it has since returned to its level in the late 1950s.

Yet, while the near simultaneity of those behavioral trends with the flurry of reforms is certainly suggestive, there are some reasons for skepticism. For one thing, the behavioral changes seem to precede the legal changes by a few years, in contrast to what one would expect if the legal changes were causal. Different states also adopted reforms over several decades, so it seems strange to suggest that those reforms explain the rather sudden jump in divorce rates between 1965–1980. Indeed, the very fact that the reforms from that particular era receive so much attention, while ostensibly similar reforms in earlier decades are often ignored, might cause one to suspect that evidence is being cherry-picked to support a particular hypothesis. On the other hand, the timing of the unilateral reforms correlates more closely with the behavioral time series, suggesting a more important role for them than for the no-fault reforms.

Numerous empirical studies have investigated the effects of divorce law (especially unilateral divorce) on divorce rates, but results have been mixed. Several papers have concluded that unilateral divorce reforms caused a statistically significant rise in U.S. divorce rates (Allen 1992; Nakonezny et al. 1995; Rodgers et al. 1997, 1999; Friedberg 1998; Gruber 2004). Estimates vary, but the most often cited result

(Friedberg's) indicates that unilateral divorce raised annual divorce rates by 0.44 divorces per 1,000 people (about 10%), and that author's largest estimates would explain about one-sixth of the increase in aggregate divorce rates between 1968 and 1988. Nevertheless, several other studies have found that changes in divorce law had no effect on divorce rates (Peters 1986, 1992; Gray 1993, 1998; Glenn 1997, 1999). Results by Weiss and Willis (1997) suggest that the risk of divorce is affected by the legal regime at the time a couple marries (although only in some specifications of their model), but not by the current legal regime, and more recent estimates by Wolfers (2003) indicate that changes in divorce law had only a temporary effect that lasted for about 8 years. Conflicting results are not unique to American data, either. According to Smith (1997), changes in British divorce laws had only a temporary effect, but Binner and Dnes (2001) find a large permanent effect.

Given such mixed evidence and the relatively small size of most estimates, one might wonder whether the legal changes were simply too minor to make much difference. That does not appear to be the case—at least, for the unilateral divorce reforms, which have received more attention. An extensive empirical literature documents meaningful effects of unilateral divorce on the labor supply of husbands and wives and intrahousehold bargaining (Gray 1993, 1998; Chiappori et al. 2002), divorce settlements (Peters 1986; Weiss and Willis 1993) and domestic violence (Parkman 1992; Dee 2003; Stevenson and Wolfers 2003). In contrast to the studies of divorce rates, these results do not appear to be controversial, perhaps because every major economic theory of divorce reforms provides a reason to expect such effects. As we shall see in the next section, those theories do not agree about how divorce reforms should affect divorce rates—or family formation decisions.

## 3 Theory

The economics literature has generated four major models in which divorce reforms influence individuals' family formation and dissolution behaviors. The simplest of these models pertains only to unilateral divorce, arguing that it merely transfers property rights from the spouse who wishes to maintain the marriage to the one who wishes to end it (Becker 1991; Peters 1986). Applying the Coase theorem and assuming that the wealth effects are negligible, advocates of this hypothesis predict that there will be no change in the incidence of divorce—although there may be changes in behaviors associated with intrahousehold bargaining (e.g., the division of assets upon divorce, wives' labor supply, and domestic violence). It seems clear that this hypothesis would predict no change in marriage or fertility either.

A second model posits that divorce law reforms (either unilateral or no-fault) reduce the legal costs associated with divorce (Peters 1992). Indeed, this was one of the major arguments used to promote no-fault reforms (Gruber 2004). Reducing the cost of divorce would raise the risk of divorce among currently married couples, and as a consequence, it may also reduce their fertility, especially insofar as children are match-specific investments (Weiss and Willis 1985). Furthermore, if the legal change reduced the fraction of the population that is married at any point in time (Gruber finds a small effect), the birth rate would fall simply because fewer people would belong to the group with the higher birth rate.[4] This may amplify the initial effect if the cost of divorce is lower for couples with fewer children (Becker et al. 1977). The incentives facing unmarried persons do not seem to change so dramatically, so the proportion of births that are out-of-wedlock would likely rise. On the other hand, the reduced cost would also make marriage a somewhat less risky proposition, so singles may be more likely to marry.

According to a third school of thought, divorce reform has reduced some of the benefits of marriage. For example, Allen (1992) has argued that these reforms may induce spouses to spend more resources on "monitoring" one another against behaviors that might make divorce more likely. Reduced benefits would make divorce more attractive, all else equal, and thus, we might expect lower fertility among married couples, lower aggregate fertility, and a higher ratio of births out of wedlock—for exactly the same reasons as in the second hypothesis. The difference is that a reduced benefit of marriage makes marriage less attractive, so singles should be less likely to marry, not more.

Finally, some argue that the reforms may raise the risk of divorce by altering intrahousehold bargaining. This hypothesis is similar to the first in that it sees unilateral divorce reforms as transferring rights from the spouse who prefers to maintain the marriage (or, ex ante, who is more likely to prefer to stay together) to the spouse who prefers to divorce.[5] (Again, the model seems less relevant for no-fault reforms.) However, this version emphasizes wealth effects that alter family formation and dissolution decisions. In particular, legal marriage is viewed as a type of insurance for the former spouse, but unilateral divorce weakens that insurance, making divorce more likely and marriage less likely (Grossbard-Shechtman et al. 2002). As in the preceding two models, the increased risk of divorce would also reduce fertility among married couples and increase the ratio of nonmarital births.

To summarize, the Coasian hypothesis predicts no effects on either marriage or fertility behaviors. All of the other models predict that fertility will fall and a higher share of births will be out-of-wedlock. However, the second model predicts that singles will be more likely to marry, while the last two predict that they will be less likely to marry. Thus, by measuring how divorce law affects family formation behaviors, we may test the first two schools of thought against one another and against the last two, collectively.

[4] Contrary to what some people imagine, the birth rate among married women is still more than double that among other women. According to my calculations from Census Bureau estimates (Downs 2003, p. 4), in the 12 months before June 2002, there were 82.8 births per 1,000 married women aged 15–44 in the United States (including those with absent spouses), but only 40.6 births per 1,000 unmarried women aged 15–44 (including both never married and formerly married women).

[5] Proponents of this hypothesis typically identify the wife as the spouse who prefers to maintain the marriage. They note, for example, that divorced women are more likely to be given primary responsibility for raising children, and many are poorly prepared to support themselves through the labor market, especially those who specialized in household production while married. Starnes (1993) presents evidence indicating that divorce law does not adequately compensate for those roles. At the very least, there is substantial evidence that divorce is quite harmful for many women (Arendell 1987; Hoffman and Duncan 1988; Bartfeld 2000).

## 4 Methods and data

The empirical strategy used here follows a difference-in-differences approach developed by Friedberg (1998) to identify the effects of states' unilateral divorce laws on their aggregate divorce rates. She estimates

$$d_{st} = a_s(t) + \tau_t + \theta L_{st} + \varepsilon_{st}, \quad (1)$$

where $d_{st}$ is the divorce rate in state, $s$, in year $t$, $\tau_t$ is a common year effect, $a_s(t)$ is a state-specific time trend, $L_{st}$ is a dummy variable that equals 1 if the state has a unilateral divorce law in year $t$, and $\theta$ is the parameter of interest.

Friedberg uses a quadratic state-specific time trend ($a_s(t) = a_{0s} + a_{1s}t + a_{2s}t^2$), rather than a simple fixed effect for each state ($a_s(t) = a_s$ for all $t$), because she lacks annual data on the states' demographic and other characteristics. The fear is that using simple fixed effects may lead to a biased estimate of $\theta$ if important unobserved characteristics changed in different ways in states with unilateral divorce laws and states without them, such as if states' divorce laws were endogenous. For example, suppose that unilateral divorce laws were more likely to gain legislative approval in states where existing families have fewer children, and consequently, the average age in those states may be rising compared to that in other states. Then, insofar as the age distribution of the population affects states' divorce rates, a model with only state fixed effects would produce a nonzero estimate of $\theta$ even if unilateral divorce actually had no effect on divorce rates. Although it is hardly a perfect solution, allowing for state-specific time trends reduces the chance of mistaking an unrelated trend for a divorce law effect or (as Friedberg's results suggest) failing to recognize a divorce law effect because it is confounded by an unrelated trend.

In a recent paper, Wolfers (2003) has extended Friedberg's specification to allow for the possibility that the effect of a change in divorce law is temporary:

$$d_{st} = a_s(t) + \tau_t + \sum_d \theta_d L_{std} + \varepsilon_{st}, \quad (2)$$

where $L_{std}$ is a vector of dummy variables that equal 1 if state, $s$, has had a unilateral divorce law for $d$ years at date $t$.[6] He argues that this model is preferable because it is reasonable to believe that the law may initially have a large effect due to a "pent-up" demand for divorce, but that the long-run effect may be negligible. As his model encompasses Friedberg's (1) as a special case, it is more flexible. One disadvantage is that a smaller number of observations identifies each $\theta_d$ parameter, leading to less precise estimates.

The following section reports results from regressions like those in Eqs. 1 and 2, using as dependent variables U.S. data on the states' marriage, divorce, birth, and legitimacy rates. Data were gathered for the years 1950–2002 from a number of sources, mostly government documents. See the Appendix for details. Figure 1 depicts the aggregate time series for each. Dummy variables for laws specifying no-fault and unilateral grounds for divorce were created based on Gruber's (2004)

[6] As a practical matter, Wolfers does not use a separate dummy variable for each number of years the law has been in effect, but rather, combines years into 2-year groups: one dummy variable for the first 2 years the law is in effect, another for the second 2 years, and so on. The results below use Wolfers' specification, but the results are virtually identical when a dummy variable is used for each individual year of the laws' duration.

"careful state-by-state review of the actual divorce laws," and following Friedberg, a set of state-specific dummy variables was created to indicate problems with the data.[7]

In addition to the variables indicated in the equations above, the regressions reported in the next section include a number of variables representing factors that may influence state-specific trends. These control variables include the states' real per capita personal income in each year and a series of demographic indicators that are linearly interpolated from the 1950–2000 census microdata (Ruggles et al. 2004): the percentage of the population that is male, the distribution of the population over seven-age groups (0–15, 16–25, 26–35, 36–45, 46–55, 56–65, and 66+), the racial composition of the population (white, black, and other), the distribution of education (less than high school, high school only, some college, and college graduates), and the fraction of the population that lives on a farm. Although many of these variables are statistically significant in most regressions, including these variables generally has only a modest effect on the estimated coefficients on the no-fault and unilateral divorce variables.

Although we are mainly interested in the effects of changes in the circumstances under which divorces may be granted, it should be mentioned that the broader literature also has some interest in the effects of laws governing the division of assets after divorce. While all states now grant divorces on a no-fault basis, many continue to consider fault for the purpose of dividing couples' assets. In addition, in the 1970s many states changed their laws to encourage a more equal division of assets between spouses. To test the effects of those distributional reforms, all of the regressions presented below were reestimated with additional dummy variables representing the legal regime governing the division of assets, coded in accordance with the dates compiled by Rasul (2004, Table 1). However, the results of this exercise generally do not support a strong role for these distributional laws. Nearly all of the estimates were small and statistically significant, the only exception being that laws promoting a more equal division of assets upon divorce were consistently associated with a statistically significant 5% reduction in the marriage rate. Furthermore, the estimated effects of laws allowing no-fault and unilateral grounds for divorce are quite robust to the inclusion of the distributional law dummies. Accordingly, the property settlement variables are excluded from the regressions reported below, but including them would not alter the results much.

One final caveat is worth noting before we turn to the results. While most theoretical predictions in Section 3 pertained to hazard rates (events per person in the eligible group—e.g., marriages per unmarried person), our dependent variables are crude rates (events per 1,000 population). In principle, the two measures may change in opposite directions if the stock of married persons changes markedly. For example, even if the marriage hazard fell, the crude marriage rate may rise if there is a larger percentage increase in the fraction of the population that is single. This is a problem throughout the literature, but one that is difficult to rectify because the

[7] The most common problem was incomplete reporting of marriage rates. For example, one dummy variable indicates years in which the state of Missouri reported a marriage rate that did not include data from all counties.

crude rates are much easier to obtain. Fortunately, Gruber's results indicate that unilateral divorce has only a small effect on the fraction of the population that is married (about a 1% decrease), so the results would probably not be qualitatively different if we used hazard rates rather than crude rates. Nevertheless, it would be preferable to examine the hazard rates directly if that were possible.

## 5 Empirical results

This section reports estimates of the key parameters from population-weighted regressions based on Eqs. 1 and 2, with robust standard errors computed using the clustering procedure recommended by Bertrand et al. (2004) to adjust for arbitrary within-state correlations. The regressions for marriage, birth, and legitimacy rates were performed using dependent variables in both levels and logs, but only the log specification is reported because results were similar for both specifications, and a Box–Cox analysis strongly favored the logarithmic specification in each case. However, the regressions on divorce rates were run in levels to facilitate comparison with the existing literature. Although the exclusion has little effect on the results, observations from Nevada are not included in the marriage and divorce regressions because a high percentage of marriages and divorces there are between nonresidents (Yang et al. 2001), and because in most years, Nevada's rates are rather extreme outliers.

### 5.1 Divorce rates

Unlike the other variables to be considered, there is already a literature that addresses the effects of unilateral divorce on divorce rates. Although this paper is primarily concerned with effects on family formation behaviors, let us briefly see whether those results differ when we allow for separate effects from no-fault divorce laws. Table 2 reports results from several regressions with divorce rates as the dependent variable. The results in Panels A and B are from regressions based on Eqs. 1 and 2, respectively, and the three columns represent three different specifications of the state-specific trends: no trends (fixed effects only), linear trends, and quadratic trends. In all cases, the data strongly support the inclusion of quadratic state-specific trends, but all three are presented for comparison.

Table 2 leads to two main conclusions. First, the estimated effects of no-fault divorce are mostly small and statistically insignificant. The only indication that it has any effect at all is the fact that the time-varying effects of no-fault divorce are jointly significant, but none of the individual estimates is significant, they vary in sign, and few of the point estimates indicate an effect larger than 1 divorce per 10,000 people in a state.

The other main result is that unilateral divorce laws lead to two to four additional divorces each year per 10,000 people in a state (6–10% of the mean over this period). However, when the effects are allowed to vary with the duration of the law, we find that the effect only lasts for 6–8 years. These estimates are substantially similar to previous results by Friedberg (1998) and Wolfers (2003), although their studies use fewer years of data and exclude variables for no-fault divorce laws.

**Table 2** Estimated effects of no-fault and unilateral divorce laws on states' divorce rates

| | No trend | | | Linear | | | Quadratic | | |
|---|---|---|---|---|---|---|---|---|---|
| | Est | SE | *P* | Est | SE | *P* | Est | SE | *P* |
| A. Time-invariant effects | | | | | | | | | |
| No fault | 0.076 | 0.116 | 0.515 | 0.023 | 0.123 | 0.851 | 0.071 | 0.091 | 0.440 |
| Unilateral | −0.014 | 0.158 | 0.929 | 0.382 | 0.158 | 0.019 | 0.243 | 0.139 | 0.088 |
| *P*: both=0 | 0.789 | | | 0.058 | | | 0.220 | | |
| B. Time-varying effects | | | | | | | | | |
| No fault | | | | | | | | | |
| Year of change | −0.167 | 0.146 | 0.258 | −0.111 | 0.132 | 0.402 | −0.076 | 0.104 | 0.467 |
| 1–2 years after | −0.001 | 0.124 | 0.995 | 0.040 | 0.119 | 0.738 | 0.091 | 0.085 | 0.285 |
| 3–4 years after | −0.016 | 0.124 | 0.900 | 0.003 | 0.126 | 0.981 | 0.080 | 0.096 | 0.406 |
| 5–6 years after | 0.073 | 0.106 | 0.493 | 0.071 | 0.111 | 0.522 | 0.177 | 0.107 | 0.104 |
| 7–8 years after | −0.002 | 0.111 | 0.984 | −0.017 | 0.108 | 0.872 | 0.135 | 0.134 | 0.318 |
| 9–10 years after | −0.057 | 0.112 | 0.611 | −0.099 | 0.145 | 0.499 | 0.085 | 0.143 | 0.557 |
| 11–12 years after | −0.081 | 0.11 | 0.470 | −0.148 | 0.141 | 0.299 | 0.069 | 0.165 | 0.681 |
| 13–14 years after | −0.050 | 0.123 | 0.686 | −0.141 | 0.157 | 0.371 | 0.098 | 0.173 | 0.573 |
| 15+ years after | 0.107 | 0.132 | 0.420 | −0.218 | 0.165 | 0.193 | 0.103 | 0.202 | 0.612 |
| Unilateral | | | | | | | | | |
| Year of change | 0.276 | 0.245 | 0.265 | 0.281 | 0.219 | 0.206 | 0.208 | 0.203 | 0.311 |
| 1–2 years after | 0.359 | 0.175 | 0.045 | 0.379 | 0.149 | 0.014 | 0.298 | 0.128 | 0.024 |
| 3–4 years after | 0.330 | 0.184 | 0.079 | 0.356 | 0.159 | 0.029 | 0.262 | 0.141 | 0.068 |
| 5–6 years after | 0.277 | 0.159 | 0.087 | 0.338 | 0.132 | 0.013 | 0.244 | 0.139 | 0.086 |
| 7–8 years after | 0.211 | 0.159 | 0.192 | 0.281 | 0.128 | 0.033 | 0.182 | 0.162 | 0.265 |
| 9–10 years after | 0.099 | 0.139 | 0.481 | 0.190 | 0.142 | 0.187 | 0.109 | 0.176 | 0.538 |
| 11–12 years after | −0.012 | 0.160 | 0.943 | 0.103 | 0.158 | 0.520 | 0.050 | 0.200 | 0.803 |
| 13–14 years after | −0.132 | 0.181 | 0.469 | 0.028 | 0.177 | 0.877 | 0.019 | 0.204 | 0.927 |
| 15+ years after | −0.447 | 0.196 | 0.027 | 0.038 | 0.179 | 0.833 | 0.099 | 0.191 | 0.607 |
| *P*: all no fault = 0 | 0.014 | | | 0.006 | | | 0.039 | | |
| *P*: all unilateral = 0 | 0.000 | | | 0.017 | | | 0.058 | | |
| *P*: all = 0 | 0.000 | | | 0.000 | | | 0.000 | | |

Nevada excluded. Panels A and B are based on Eqs. 1 and 2, respectively.

One might be tempted to conclude, based on the fact that the estimates become small and statistically insignificant after a few years, that the effect is temporary. However, that conclusion would be a bit premature because the estimates are not very precise. To be sure, as a cursory glance suggests, the results can support the hypothesis that the effect dissipates after a few years ($P$[no effect after year 10] = 0.707). Yet they can also support the hypothesis that the effect is permanent ($P$[all unilateral parameters are equal] = 0.694).

Unfortunately, the interpretation of the magnitude of this effect depends on whether one believes the effect is temporary or permanent. As Wolfers has shown, a temporary effect would have very little power to explain the largely permanent increase in divorce rates over the past 40 years. A permanent effect would be more meaningful, however. The average annual aggregate divorce rate rose from 2.3 in the pre-reform period (defined as 1950–1965) to 4.6 in the post-reform period (defined as 1980–2002), and about 60% of the U.S. population is now covered by unilateral divorce, so the point estimate for a permanent effect would predict about 6–10% of the actual increase in the divorce rate.

## 5.2 Marriage rates

Table 3 reports results from regressions on log marriage rates. Column 1 displays the estimates from a regression including dummies for both unilateral and no-fault divorce laws. As the reader can see, neither variable is estimated to have a large effect on marriage rates in either specification, and both estimates are statistically insignificant, both individually and jointly. Other regressions (not reported) in which only linear state-specific trends or simple state fixed effects were used also gave no indication that either type of divorce reform has had any effect on marriage rates.

As several states passed no-fault and unilateral divorce legislation in the same years, and the correlation between the dummy variables for the two laws is relatively high (0.571), one might wonder whether this result is primarily due to colinearity between the two legal variables. That does not appear to be the case, however. The results in columns 2 and 3 of Table 3 indicate that we find essentially the same thing when the two legal reforms are entered separately: no effect.

**Table 3** Estimated effects of no-fault and unilateral divorce laws on states' log marriage rates

| | (1) Robust | | | (2) Robust | | | (3) Robust | | |
|---|---|---|---|---|---|---|---|---|---|
| | Est. | SE | *P* | Est. | SE | *P* | Est. | SE | *P* |
| A. Time-invariant effects | | | | | | | | | |
| No fault | −0.029 | 0.021 | 0.165 | −0.025 | 0.017 | 0.152 | | | |
| Unilateral | 0.012 | 0.031 | 0.688 | | | | −0.003 | 0.027 | 0.919 |
| *P*: both=0 | 0.345 | | | | | | | | |
| B. Time-varying effects | | | | | | | | | |
| No fault | | | | | | | | | |
| Year of change | −0.007 | 0.017 | 0.706 | −0.006 | 0.014 | 0.667 | | | |
| 1–2 years after | −0.015 | 0.021 | 0.491 | −0.016 | 0.019 | 0.387 | | | |
| 3–4 years after | −0.031 | 0.026 | 0.229 | −0.028 | 0.025 | 0.261 | | | |
| 5–6 years after | −0.058 | 0.029 | 0.049 | −0.045 | 0.032 | 0.172 | | | |
| 7–8 years after | −0.046 | 0.035 | 0.191 | −0.041 | 0.039 | 0.295 | | | |
| 9–10 years after | −0.040 | 0.041 | 0.333 | −0.028 | 0.044 | 0.533 | | | |
| 11–12 years after | −0.057 | 0.044 | 0.208 | −0.039 | 0.048 | 0.418 | | | |
| 13–14 years after | −0.038 | 0.052 | 0.465 | −0.025 | 0.056 | 0.659 | | | |
| 15+ years after | −0.033 | 0.060 | 0.576 | −0.021 | 0.065 | 0.748 | | | |
| Unilateral | | | | | | | | | |
| Year of change | −0.000 | 0.022 | 0.996 | | | | 0.002 | 0.017 | 0.916 |
| 1–2 years after | −0.002 | 0.029 | 0.942 | | | | −0.004 | 0.024 | 0.877 |
| 3–4 years after | 0.010 | 0.039 | 0.791 | | | | 0.001 | 0.035 | 0.987 |
| 5–6 years after | 0.037 | 0.046 | 0.420 | | | | 0.010 | 0.048 | 0.836 |
| 7–8 years after | 0.024 | 0.048 | 0.611 | | | | 0.006 | 0.050 | 0.900 |
| 9–10 years after | 0.045 | 0.050 | 0.374 | | | | 0.030 | 0.051 | 0.444 |
| 11–12 years after | 0.063 | 0.058 | 0.283 | | | | 0.036 | 0.058 | 0.533 |
| 13–14 years after | 0.058 | 0.068 | 0.392 | | | | 0.045 | 0.070 | 0.520 |
| 15+ years after | 0.061 | 0.077 | 0.434 | | | | 0.051 | 0.082 | 0.532 |
| *P*-value: all = 0 | 0.112 | | | 0.137 | | | 0.749 | | |

Nevada excluded. Panels A and B are based on Eqs. 1 and 2, respectively.

### 5.3 Birth rates and legitimacy

Table 4 presents estimates from regressions based on Eq. 1 for four different fertility variables: the crude birth rate (births/1,000 people), the illegitimacy ratio (fraction of births that are out-of-wedlock), the marital birth rate (marital births/1,000 people), and the nonmarital birth rate (nonmarital births/1,000 people). As in Table 2, the three columns represent different specifications of the state-specific trends—although, again, the data strongly support the inclusion of quadratic state-specific trends in each case. Table 5 presents comparable results for three of those dependent variables from regressions that allow the effects to vary over time (as in Eq. 2).

There are several things to note in these tables. First, there is consistent evidence that no fault reforms reduce total birth rates by about 2–4%. The estimated effects in Table 4 are virtually the same in all three specifications, and each is statistically significant at the 8% level or better. Some of the individual time-varying estimates in Table 5 are not statistically significant, but the entire set of estimates is jointly significant, and we cannot reject the hypothesis that there is a constant effect beginning 2 years after the law take effect ($P = .450$).

The effect appears to work primarily through marital birth rates. Although all of the individual estimates are statistically insignificant, the group of time-varying estimates is jointly significant at the 3% level, and we cannot reject the hypothesis that there is a constant effect starting the year after the law passes ($P = .217$). The magnitudes of the estimates are also generally similar to those for the total birth rate. In contrast, there is little to indicate any effect of no-fault divorce on nonmarital birth rates.

**Table 4** Estimated effects of no-fault and unilateral divorce laws on states' log birth rates and log legitimacy ratios

| Dependent variable (in logs) | Legal reform | Est. | SE | *P* | Est. | SE | *P* | Est. | SE | *P* |
|---|---|---|---|---|---|---|---|---|---|---|
| Total birth rate | No fault | −0.016 | 0.008 | 0.072 | −0.025 | 0.009 | 0.007 | −0.022 | 0.010 | 0.024 |
| | Unilateral | −0.016 | 0.014 | 0.236 | −0.001 | 0.019 | 0.945 | 0.013 | 0.018 | 0.452 |
| | *P*: both = 0 | 0.023 | | | 0.011 | | | 0.075 | | |
| Nonmarital births/All Births | No fault | 0.026 | 0.044 | 0.563 | 0.017 | 0.036 | 0.631 | 0.030 | 0.024 | 0.220 |
| | Unilateral | 0.095 | 0.053 | 0.077 | −0.067 | 0.036 | 0.071 | −0.065 | 0.028 | 0.024 |
| | *P*: both = 0 | 0.182 | | | 0.122 | | | 0.076 | | |
| Marital birth rate | No fault | −0.022 | 0.017 | 0.192 | −0.025 | 0.017 | 0.140 | −0.026 | 0.016 | 0.112 |
| | Unilateral | 0.033 | 0.016 | 0.048 | 0.027 | 0.024 | 0.265 | 0.033 | 0.021 | 0.115 |
| | *P*: both = 0 | 0.123 | | | 0.310 | | | 0.176 | | |
| Nonmarital birth rate | No fault | −0.038 | 0.041 | 0.360 | 0.003 | 0.033 | 0.928 | 0.017 | 0.022 | 0.453 |
| | Unilateral | 0.090 | 0.049 | 0.073 | −0.065 | 0.031 | 0.041 | −0.050 | 0.027 | 0.076 |
| | *P*: both = 0 | 0.194 | | | 0.051 | | | 0.200 | | |
| State-specific trends? | | None (fixed effects) | | | Linear | | | Quadratic | | |

**Table 5** Estimated time-varying effects of no-fault and unilateral divorce laws on states' log birth rates, by legitimacy status

| Dep. var.: (log birth rates) | All births Rob. | | | Marital births Rob. | | | Nonmarital Births Rob. | | |
|---|---|---|---|---|---|---|---|---|---|
| | Est. | SE | *P* | Est. | SE | *P* | Est. | SE | *P* |
| No fault | | | | | | | | | |
| Year of change | −0.001 | 0.006 | 0.837 | −0.011 | 0.010 | 0.301 | 0.024 | 0.020 | 0.240 |
| 1–2 years after | −0.020 | 0.010 | 0.040 | −0.024 | 0.015 | 0.113 | −0.006 | 0.023 | 0.791 |
| 3–4 years after | −0.031 | 0.011 | 0.006 | −0.033 | 0.020 | 0.101 | −0.016 | 0.026 | 0.534 |
| 5–6 years after | −0.038 | 0.015 | 0.018 | −0.037 | 0.024 | 0.119 | −0.012 | 0.030 | 0.694 |
| 7–8 years after | −0.031 | 0.018 | 0.082 | −0.028 | 0.028 | 0.313 | −0.013 | 0.032 | 0.674 |
| 9–10 years after | −0.033 | 0.018 | 0.070 | −0.031 | 0.031 | 0.311 | −0.027 | 0.036 | 0.460 |
| 11–12 years after | −0.037 | 0.020 | 0.067 | −0.033 | 0.032 | 0.301 | −0.046 | 0.039 | 0.243 |
| 13–14 years after | −0.038 | 0.021 | 0.079 | −0.035 | 0.033 | 0.293 | −0.052 | 0.044 | 0.237 |
| 15+ years after | −0.035 | 0.023 | 0.129 | −0.042 | 0.034 | 0.223 | −0.056 | 0.047 | 0.238 |
| Unilateral | | | | | | | | | |
| Year of change | −0.003 | 0.012 | 0.801 | 0.018 | 0.014 | 0.228 | −0.060 | 0.023 | 0.013 |
| 1–2 years after | 0.003 | 0.017 | 0.842 | 0.033 | 0.019 | 0.091 | −0.034 | 0.026 | 0.187 |
| 3–4 years after | 0.012 | 0.019 | 0.515 | 0.038 | 0.023 | 0.099 | −0.057 | 0.039 | 0.145 |
| 5–6 years after | 0.027 | 0.020 | 0.193 | 0.052 | 0.024 | 0.037 | −0.093 | 0.042 | 0.033 |
| 7–8 years after | 0.020 | 0.022 | 0.366 | 0.054 | 0.027 | 0.049 | −0.100 | 0.046 | 0.034 |
| 9–10 years after | 0.017 | 0.022 | 0.438 | 0.053 | 0.027 | 0.057 | −0.119 | 0.046 | 0.012 |
| 11–12 years after | 0.023 | 0.024 | 0.334 | 0.064 | 0.028 | 0.026 | −0.130 | 0.052 | 0.016 |
| 13–14 years after | 0.022 | 0.025 | 0.393 | 0.067 | 0.030 | 0.028 | −0.150 | 0.060 | 0.015 |
| 15+ years after | 0.015 | 0.028 | 0.600 | 0.070 | 0.032 | 0.032 | −0.177 | 0.069 | 0.014 |
| *P*: all no fault = 0 | 0.008 | | | 0.024 | | | 0.275 | | |
| *P*: all unilateral = 0 | 0.065 | | | 0.013 | | | 0.036 | | |
| *P*: all = 0 | 0.000 | | | 0.000 | | | 0.003 | | |

Regressions based on Eq. 2 with quadratic state-specific time trends.

It should also be noted that the estimated effect is not large relative to the overall trend. About 77% of the population became subject to no-fault divorce between 1960 and the present, so the largest estimate from Table 4 would suggest that the no-fault reforms caused a 1.9% reduction in the total birth rate. In actuality, the average annual aggregate birth rate fell 35.5% from (1950–1965) to (1980–2002), so even if these effects are real, they would account for only a small fraction of the actual change.

Another hypothesis that finds some support in Table 4 is the idea that unilateral divorce laws are associated with a decreased share of births that are out of wedlock. Considering that all of the theories discussed earlier predicted that legal reforms would either leave the legitimacy ratio unchanged or increase it, this result is quite unexpected. Nevertheless, the finding receives additional support when we allow the effects to vary over time. The results in columns 2 and 3 of Table 5 indicate that unilateral divorce is associated with both an increase in the marital birth rate[8] and a decrease in the nonmarital birth rate (but, if anything, a small increase in the aggregate birth rate). Most of the individual estimates are statistically significant, and the group is jointly significant at the 4% level in both cases.

[8] Stevenson (2003) also finds evidence that married couples in states with unilateral divorce are slightly more fertile, although her results are somewhat mixed.

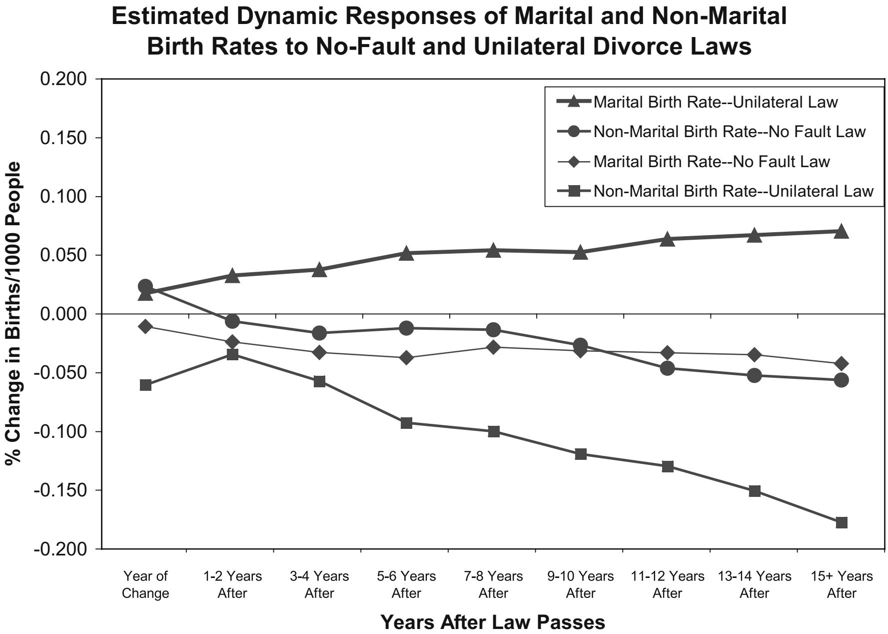


**Fig. 2** Estimated dynamic effects of divorce reforms on marital and nonmarital fertility

Figure 2 plots the point estimates from columns 2 and 3 of Table 5. As the reader can see, the effects of unilateral divorce seem to grow with the duration of the law,[9] especially in the case of nonmarital births. There is also some indication that the effects of no-fault reforms grow over time, although the trend is much less dramatic. One might suspect this pattern of estimates is related to the inclusion of state-specific trends, but that does not seem to be the case. For marital births, the results for unilateral divorce are fairly similar (though somewhat less pronounced) when only state fixed effects are used, and for nonmarital births, the trendless specification also indicates an effect that increases with the duration of the law.

Even so, the magnitude of these estimates is, once again, minute in comparison to the actual trends, and in this case, they are in the opposite direction. Since 1960, about 59% of the population has become covered by unilateral divorce, so even our largest point estimates (from column 2 of Table 4) imply that the legal reforms changed (reduced) the illegitimacy ratio by about 4%. In actuality, the average annual aggregate illegitimacy ratio increased by 375% between the pre-reform period (1950–1965) and the post-reform period (1980–2002). Regardless of which specification one prefers, it would seem impossible to argue that divorce reforms were responsible for anything more than a tiny fraction of the change in legitimacy.

As a final point, it is interesting that the sign of the estimated effects of unilateral divorce on the illegitimacy ratio and the nonmarital birth rate changes when state-specific trends are included, as one can see in Table 4. The results using quadratic trends are similar to those using a linear trend, but both are markedly different

[9] Wolfers (2003) finds that effects on divorce rates also seem to grow larger over time, at least in some specifications.

from the regression with only state fixed effects. A similar pattern also emerges in regressions (not shown) that allow for effects that vary over time (as in Eq. 2). This observation lends credence to the concern that state laws are correlated with underlying trends in the data (quite possibly because divorce legislation responds to the needs of the electorate, as Broel-Plateris (1961) concludes). Accordingly, it seems best to treat estimates that do not allow for state-specific trends with some suspicion.

## 6 Conclusion

As previous work has focused almost exclusively on the relationship between unilateral divorce and divorce rates, most of the estimates produced here are among the first measures of their respective effects. It is reassuring that the few results with counterparts in the literature are comparable in magnitude to those benchmarks. Nevertheless, given that the analysis has been limited by the availability of data (especially for the pre-reform years), the reader is cautioned to consider the vulnerability of these estimates to future improvements in data and methods.

With that caveat in mind, let us briefly summarize what we have found before interpreting it. First of all, there was little to indicate that either no-fault or unilateral divorce had any effect on marriage rates. As in the existing literature, there was some indication that unilateral divorce causes a modest increase in divorce rates, at least during the first 5 or 10 years after the law passes, but no-fault divorce does not seem to have any meaningful effect on divorce rates. However, the results consistently indicated that no-fault divorce lowers birth rates by about 2–3%, with the effect apparently concentrated among married couples. On the other hand, we find—contrary to the theoretical predictions discussed earlier—that unilateral divorce seems to increase marital birth rates and decrease nonmarital birth rates, and both of those effects seem to grow the longer the law is in effect.

The implications of these results for economic models of divorce law are less clear than one might like. As we found few substantial effects of no-fault divorce, it may be tempting to conclude that the results support the Coasian hypothesis advocated by Becker and Peters. However, that model only seems appropriate for unilateral divorce, not no-fault divorce, and such a conclusion would also contradict our finding that no-fault divorce reduces fertility. Rather, what may be more likely is that no-fault divorce laws did little to change the actual experience of divorce. Remember, while the reform changed the formal legal requirements for divorce to be granted, nearly every discussion of the legal history notes that earlier couples had routinely circumvented fault requirements through fraud and/or side-agreements. If the cost of making those arrangements was small, in comparison to the other benefits and costs associated with divorce, or if there has been a comparable increase in lawyers' fees or court costs, it may explain why the switch to a no-fault system had such small effects.

The results for unilateral divorce are even more perplexing. The finding that it increases divorce rates but does not affect marriage rates is inconsistent with all four of the theories discussed in the Section 3—only the Coasian hypothesis predicted no effect on marriage rates, but it also predicted no effect on divorce rates, nor can any of those theories explain why unilateral divorce would increase birth rates and reduce the illegitimacy ratio (indeed, the result is opposite to what any of the models predict) or why the effects would grow over the lifetime of the law. Clearly, these results suggest a need for additional work on the theory of unilateral divorce. In light of the results on legitimacy, perhaps, it may be fruitful to consider the law's effects on the legitimation of premarital conceptions.

Although these results pose a challenge for theorists, they present a fairly clear answer to those who seek to explain trends in family formation and dissolution behaviors over the last half century. The point estimates explain less than 10% of the increase in divorce rates between the pre- and post-reform era, and the estimated effects on birth and legitimacy are small in comparison to the actual trends and often opposite in sign. In short, there is little here—or in the previous literature, for that matter—to suggest that divorce reform was anything more than a minor factor in the broad movement away from traditional family arrangements. On the contrary, our estimates suggest that researchers interested in those trends might more fruitfully turn their attention to the many other hypotheses that purport to explain them.[10]

By the same token, the evidence offers little hope that reinstating fault-based and/or mutual divorce laws would dramatically reverse the so-called "breakdown of the family." Nevertheless, it does not necessarily follow that policy makers should be unconcerned about these laws. As noted earlier, previous work has shown that unilateral divorce laws have a meaningful effect on important issues like intrahousehold bargaining, family labor supply, and spousal abuse. Moreover, one might reasonably argue that the potential social cost of underestimating the effects of these laws is larger than that of overestimating them, and of course, even small aggregate effects may be important to the individuals affected.

**Acknowledgements** This paper benefitted from comments by Scott Adams, Robert Pollak, Kamil Tahmiscioglu, Nicholas Williams, participants of workshops at the Midwest Economics Association annual meetings and the University of Wisconsin—Milwaukee, Deborah Cobb-Clark (the editor), and two anonymous referees. I am also grateful to Sean Flanagan for gathering data on some of the variables used in the empirical analysis. All remaining flaws are solely my responsibility.

## Appendix: Data

Several sources were used to gather data on state populations and marriage, divorce, birth, and illegitimacy rates. Most of the data is from federal vital statistics reports, including both periodic publications (*Vital Statistics of the United States*, *Statistical Abstract of the United States*, *Monthly Vital Statistics Reports*, and *National Vital Statistics Reports*) and occasional reports that compiled data across several years (Grove and Hetzel 1968; Plateris 1973, 1978; Clague and Ventura 1974; Ventura 1980, 1995). Some data missing from the federal reports were obtained from reports issued by the vital statistics agencies of individual states (Louisiana Department of Health and Hospitals, 1997–1999; Vermont Department of Health 2001; Yang et al. 2001; Beitsch 2003; Colorado Department of Public Health and Environment 2003).

Data were gathered for the years 1950–2002, including pre-statehood marriage, divorce, and birth rates for Alaska and Hawaii. Most numbers are "final," but in some cases, only preliminary numbers were available, including all data for 2002. Some rates were not published directly, but could be calculated from published event and population figures (e.g., marriage rate$= 1,000\times$ marriages/population). No significant deviations were found when similar calculations were compared to published rates when all three measures were available. Recent marriage and divorce rates for Oklahoma (2000–2002) are estimated by the author based on exact counts from all but a small number of counties (never more than three) and the most recent data from the missing

[10] Drewianka (2003) reviews several alternate theories for these trends and the evidence for each.

counties (Beitsch 2003). The variation in the totals from those counties over the years, since 1995, suggests that the estimated marriage rate is likely accurate to within 1% (less than 0.1 marriages/1,000 people) and that the estimated divorce rate is likely accurate to within about 4% (0.1–0.2 divorces/1,000 people). Birth rates before 1960 were adjusted by the Public Health Service to correct for underregistration (Grove and Hetzel 1968); after that, no adjustments were made because the registry was estimated to be over 99.1% complete (Vital Statistics of the United States, 1970, Volume I—Natality 1975, Section 4, p. 13). In keeping with Friedberg's (1998) methodology, dummy variables were created to signify data anomalies reported in the original data sources, the primary example being underreporting.

The data on birth rates is complete, and marriage rates are missing for only two state-years, CA (1991) and MA (1953). The divorce data is 97% complete (100% complete between 1959–1983); the following state-years are missing: AR (1951–1952), CA (1991–2002), CO (1995–1997), IL (1954–1957), IN (1957, 1971–1972, 1988–2002), KY (1951–1958), LA (1951–1958, 1984, 1986–1995, 1999–2002), MA (1956), NM (1953), NY (1951–1957), NC (1951, 1953–1957), OK (1951), RI (1954), SC (1952) WV (1951–1957), WY (1952).

The legitimacy data is 84% complete (100% since 1980). The missing cases are for AK (pre-1959), AZ (pre-1968), AR (pre-1968), CA (pre-1980), CO (pre-1968), CT (pre-1980), GA (1959–1979), HI (pre-1960), ID (pre-1978), MD (pre-1980), MA (pre-1978), MI (1979), MT (1960–1979), NE (pre-1968), NV (1971–1979), NH (pre-1968), NM (pre-1980), NY (pre-1980), OH (1969–1979), OK (pre-1968), SD (1963), TX (1977–1979), VT (1951–1977), WI (1961).

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