Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results

By JUSTIN WOLFERS*

The "no-fault revolution" that swept the United States in the 1970s radically altered the parameters of family law. The new no-fault unilateral divorce laws allowed people to seek a divorce without the consent of their spouse, a dramatic departure from previous practice. This decade also saw radical changes in the structure of American families, with divorce rates rising dramatically across the nation. Are these two trends connected? This question has been argued at length, and each iteration of the debate has yielded new insights. H. Elizabeth Peters (1986) argued that divorce rates were unaffected by the change in legal regime, a finding rebutted by Douglas W. Allen (1992), and subsequently countered by Peters (1992). Parallel literatures in both sociology and law have also yielded fierce debate.[1] Practitioners also seem divided: a recent survey of members of the Family Law Section of the American Bar Association found that around two-thirds of respondents do not agree that there is a direct correlation between higher divorce rates and divorce law liberalization (Laura Gatland, 1997).

Leora Friedberg (1998) presented a seemingly appealing alternative to earlier studies. Her paper analyzed comprehensive administrative divorce data in a state-based panel. In response to concerns about the endogeneity of divorce reform expressed in the Peters-Allen exchange (divorce reform came first to those states with historically high divorce rates), Friedberg controlled for state and year fixed effects, as well as state-specific time trends in her specification. Friedberg interprets her results as suggesting that the adoption of unilateral divorce laws accounts for about one-sixth of the rise in the divorce rate since the late 1960s, a finding that has since been widely accepted.[2]

This paper argues that these conclusions are somewhat misleading. A major difficulty in difference-in-difference analyses involves separating out preexisting trends from the dynamic effects of a policy shock. Her approach appears to confound the two. This problem—that state-specific trends may pick up the effects of a policy and not just preexisting trends—is quite general. Slight modifications to standard procedures yield more directly interpretable estimates.

I find that the divorce rate rose sharply following the adoption of unilateral divorce laws, but that this rise was reversed within about a decade. There is no evidence that this rise in divorce is persistent. Indeed, some of my results suggest—somewhat puzzlingly—that 15 years after reform the divorce rate is lower as a result of the adoption of unilateral divorce, although it is hard to draw any strong conclusions about long-run effects.

The fundamental theoretical issue at stake in this empirical debate is the applicability of the

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[1] Related contributions in the economics literature include: Jonathan Gruber (2004), John H. Johnson and Christopher J. Mazingo (2000), and Stéphane Méchoulan (2006). In the law and economics literature, see: Margaret F. Brinig and Frank H. Buckley (1998), Ira Mark Ellman and Sharon L. Lohr (1998), and Ellman (2000); and in the sociology literature, see: Paul A. Nakonezny et al. (1995), Norval D. Glenn (1999), and Joseph L. Rogers et al. (1999).

[2] See Jane M. Binner and Antony W. Dnes (2001), Gruber (2004), Johnson and Mazingo (2000), and Robert Rowthorn (1999).

Coase theorem to marital relations. Gary Becker (1981) argues that unilateral divorce simply reassigns existing property rights between spouses. Under the consent divorce regime, both partners must agree to a divorce, whereas the unilateral regime requires only one spouse to desire a divorce. In Coasian terms, why should a reassignment of property rights—from the happily married spouse to their partner who would prefer a divorce—change outcomes? Peters (1986) and Betsey Stevenson and Wolfers (2006) discuss plausible reasons for the failure of the Coase theorem in marital bargaining. In this paper, my focus is primarily empirical, and I seek to evaluate whether divorce rates rose following the passage of unilateral divorce laws.

In Section I, I present Friedberg's results, and show that her estimates are replicable. Working through a simple example, Section II shows that in applications involving interesting dynamics, the standard difference-in-difference approach may produce misleading results if panel-specific trends are included as controls. This is a more general problem in differences-in-difference analyses, and one contribution of this paper is simply to highlight the bias that might result. Imposing minimal structure on the dynamic response of the divorce rate, I present a well-identified specification that suggests that divorce rates rose temporarily following the adoption of unilateral divorce laws. These results are not particularly sensitive to the inclusion of state-specific trends, and there is little evidence of a persistent impact. Section III finds complementary evidence in census data tracing the evolution of the stock of ever-divorced people. Section IV explores the empirical robustness of my findings, and Section V turns to interpretation.

I. Replicating Friedberg[3]

Between 1968 and 1988, 29 states changed their legal systems, from some variant of consent divorce to a unilateral system. Standard accounts of this period of legislative activity suggest that the timing of these changes was plausibly exogenous (see Herbert Jacob, 1988). Thus, state-based panel estimation of the effects of these changes seems natural. Friedberg collected administrative data on the *divorce rate* in each state and year from 1968 to 1988 from *Vital Statistics of the United States.* The divorce rate is defined as the annual number of new divorces per thousand persons in each state. These data cover virtually every divorce in the United States throughout this period. She estimated:[4]

$$\begin{aligned}(1)\qquad \textit{Divorce Rate}_{s,t} = {} & \beta \textit{Unilateral}_{s,t} \\ & + \sum_s \textit{State fixed effects}_s \\ & + \sum_t \textit{Time fixed effects}_t \\ & \Bigg[+ \sum_s \textit{State}_s * \textit{Time}_t \\ & + \sum_s \textit{State}_s * \textit{Time}_t^2 \Bigg] + \varepsilon_{s,t}.\end{aligned}$$

The variable *Unilateral* is a dummy, set equal to one when the state has a unilateral divorce regime, and zero under a consent divorce regime. The coefficient β is interpreted as the average rise in the divorce rate attributable to the legal change. Much of the earlier debate in this literature focused on coding these legal changes.[5] More precisely, this involved two debates: developing a taxonomy of legal regimes that yields economically meaningful distinctions; and, given this taxonomy, providing an appropriate classification of these laws. On the former, I follow Friedberg in focusing on the assignment of property rights between spouses (the distinction between unilateral and consent divorce), while on the latter, I take Friedberg's coding as a starting point, but test which of the main findings is robust to a range of different coding regimes.

Equation (1) is estimated using population-weighted least squares. Panel A of Table 1

[3] All of the data and programs used in this paper are available at: www.nber.org/~jwolfers.

[4] A range of indicator variables was also included to account for slight breaks in the various state divorce series. These have no important effect on estimated results, and hence while I include them in the replication in Table 1, for simplicity, I drop them in all subsequent analysis.

[5] In the economics literature, see the Peters-Allen exchange; in law and economics, see the dialogue between Brinig and Buckley and Ellman and Lohr.

TABLE 1—FRIEDBERG'S RESULTS
(*Dependent variable: Annual divorces per 1,000 persons*)

	(1) Basic specification	(2) State-specific trends linear	(3) State-specific trends quadratic
	Panel A. Friedberg (1998)		
Unilateral	0.004	0.447	0.441
	(0.056)	(0.050)	(0.055)
Year effects	$F = 89.0$	$F = 95.3$	$F = 8.9$
State effects	$F = 217.3$	$F = 196.2$	$F = 131.1$
State trend, linear	No	$F = 24.7$	$F = 9.3$
State trend, quadratic	No	No	$F = 6.5$
Adjusted R^2	0.946	0.976	0.982
	Panel B. Replication		
Unilateral	0.000	0.431	0.435
	(0.057)	(0.051)	(0.055)
Year effects	$F = 89.3$	$F = 95.3$	$F = 9.0$
State effects	$F = 216.5$	$F = 191.6$	$F = 129.1$
State trend, linear	No	$F = 24.4$	$F = 9.3$
State trend, quadratic	No	No	$F = 6.6$
Adjusted R^2	0.946	0.976	0.981

Notes: Sample: 1968–1988, $n = 1043$ (unbalanced panel). Estimated using state population weights. Standard errors in parentheses.

Sources: Divorce rate data coded by Friedberg (1998) from *Vital Statistics.* Divorce laws coded from Friedberg's Table 1. Population weights downloaded from www.census.gov.

simply reprints Friedberg's results. The specification shown in column 1 includes state and year fixed effects, yielding reasonably precisely estimated coefficients suggesting almost no change in the divorce rate. This finding is consistent with Peters (1986, 1992), who found that when one controls for existing differences in state divorce propensities, unilateral divorce laws did not affect divorce rates.

However, Friedberg argues (p. 611) that even this may be too restrictive a specification, and that "the factors which influence divorce may vary within a state over time, confounding the estimates of the state effects. ... Including state-specific trends allows unobserved state divorce propensities to trend linearly and even quadratically over time and reveals that unilateral divorce raised divorce rates significantly and strongly." Of course, these omitted factors bias the estimated effect of unilateral divorce laws only if they are correlated with divorce laws. Column 2 shows Friedberg's preferred specification, which includes state-specific linear time trends to account for slow-moving social and demographic trends in each state. This specification changes the point estimate dramatically, suggesting that the divorce rate rose by 0.447. Comparing this coefficient with an average rate of 4.6 divorces per 1,000 people per year, this translates to a rise of a little under 10 percent. Testing for robustness, Friedberg adds state-specific quadratic time trends in column 3, finding a similar effect. Thus, she concludes that unilateral divorce caused the divorce rate to rise significantly. In later tables, she includes leads and lags of the independent variable, and concludes (p. 608) that "the effect of unilateral divorce on divorce behavior was permanent, not temporary."

Panel B of Table 1 shows my attempts to replicate Friedberg's results. Replication was relatively simple because Friedberg generously shared her divorce data. In all columns, the results are extremely similar. Remaining differences are in the second decimal place and presumably reflect revised population estimates that are used as weights, or differences in computational procedures. Beyond the statistics shown in Panel B, my regressions also closely replicated detail on state and year effects provided in the appendices of Friedberg's paper.

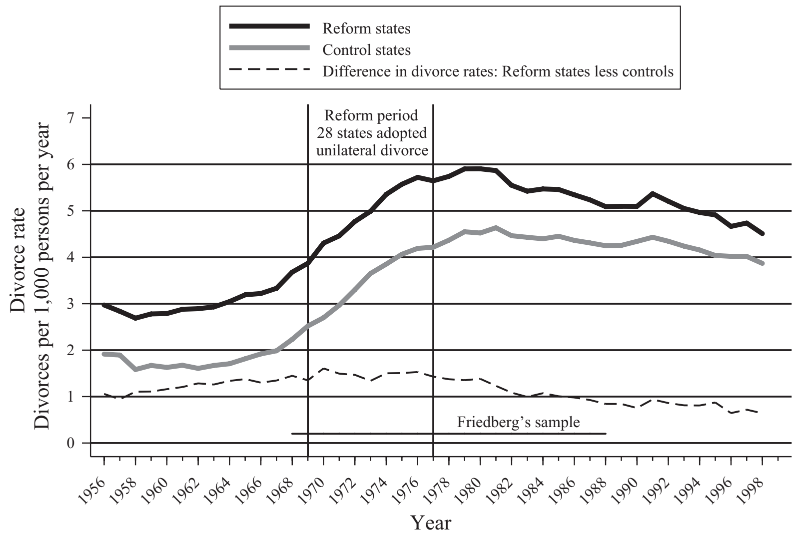

FIGURE 1. AVERAGE DIVORCE RATE: REFORM STATES AND CONTROLS

This seems to be as close to a complete replication as one can hope for.[6]

A worrying feature of the estimates in Table 1 is their sensitivity to the inclusion of state-specific trends. Friedberg's interpretation is that these trends reflect omitted variables, and thus their inclusion remedies an omitted variable bias. The omission of these variables should only bias these coefficients, however, if there is a *systematic* relationship between the trend in divorce rates and the adoption of unilateral divorce laws. Certainly, such a relationship seems at odds with the purported exogeneity of the timing of the adoption of these laws. Further, controlling for state time trends raises the coefficient on *Unilateral*, a finding that can be reconciled with an omitted variables interpretation only if factors correlated with a relative *fall* in divorce propensities led states to adopt unilateral divorce laws. This seems unlikely; if anything, one might expect factors associated with a rising divorce rate to have increased the pressure for reform.

Figure 1 shows the evolution of the average divorce rate across the reform and control states, respectively.[7] Clearly, higher divorce rates in reform states have been a feature since at least the mid-1950s, undermining any inference that these cross-state differences reflect the "no-fault revolution" of the early 1970s.[8] Thus, controlling for these preexisting differences—perhaps through the inclusion of state fixed effects—seems important (a point made by both Peters, 1986, and Friedberg, 1998). The dashed line shows the evolution of the difference in the divorce rate between reform and control states. This line allows a coarse comparison of the relative preexisting trends; if anything, it shows a mildly *rising* trend in the divorce rate in treatment states relative to the control states prior to reform, suggesting that adding controls for preexisting trends should reduce the *Unilateral* coefficient.

The next section reconciles these findings. In the context of a simple example highlighting stock-flow dynamics, I show that Friedberg's results are not robust to plausible specifications of the dynamic effects of changes in divorce laws. Specifically, it appears that her estimates

[6] On computational procedures, see Bruce D. McCullough and Hrishikesh D. Vinod (1999). Regarding replication, see William G. Dewald et al. (1986).

[7] Controls are defined as those states that did not change their divorce laws during Friedberg's 1968–1988 sample.

[8] See Allen (1992) and Johnson and Mazingo (2000) for papers that are identified off cross-state variation in divorce rates and divorce hazards, respectively.

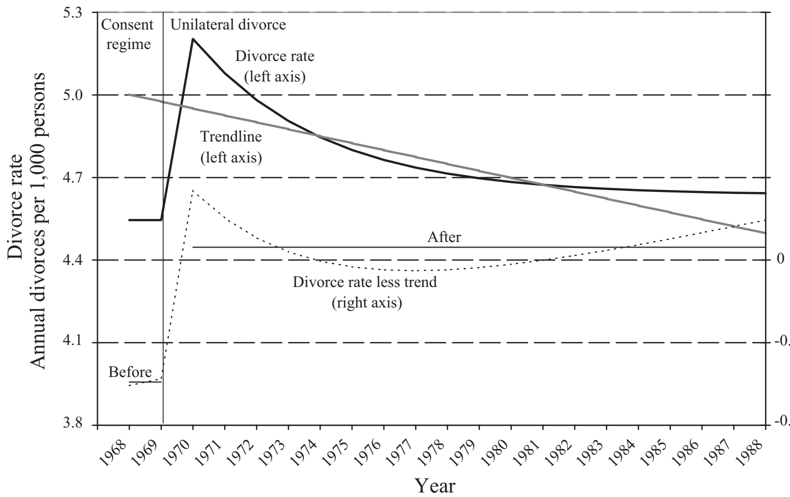

FIGURE 2. DYNAMIC RESPONSE OF THE DIVORCE RATE

Notes: This hypothetical response of the divorce rate is constructed under the following assumptions: each year 20 percent of the population assess whether to get divorced, given the current legal regime; under consent divorce laws the 20 percent most incompatible matches dissolve; under unilateral divorce, this rises to 20.4 percent; each year 2.5 percent of the population die, and are replaced by new marriages.

confound preexisting trends with the response of the divorce rate to the policy shock. More plausible specifications suggest that the divorce rate rose for a number of years following divorce law reform; no effect is discernible after a decade, and there is some evidence of a reversal over the ensuing period.

II. Stock-Flow Dynamics and Difference-in-Difference Estimates

A shift in divorce regimes is likely to have very different short-run and long-run effects. Immediately following reform, the divorce rate is likely to rise dramatically as the courts cater to pent-up demand for the new type of divorce facilitated by this change. Evolving norms and the slow diffusion of information about the divorce regime may keep the divorce rate high for a period. This may be further reinforced by developments in a thicker remarriage market. Eventually this "pent-up demand" will run its course, and the flow of divorces will move toward its new steady state. Further interesting dynamic patterns may be evident in the medium run: bad matches may be dissolved earlier, shifting the pattern of divorce across the life-cycle; differential selection into marriage will change the nature of the "at-risk" population, and so on. During the transition to the new steady state, it is likely that the corresponding stock variable—the *ever-divorced population*—will slowly approach its new level. During the transition to this new steady state, however, the flow of new divorces will not necessarily bear a simple relation to either its new steady-state level, or to the ever-divorced population.

This section shows that standard difference-in-difference estimates confound these stock-flow dynamics with panel-specific trends, yielding results that are difficult to interpret. To provide intuition, the bold line in Figure 2 shows the dynamics from a simple partial adjustment model. Specifically, I contrast consent divorce laws which lead the c percent most incompatible marriages to dissolve, with unilateral divorce laws that lead a further ε percent of marriages to end. While these assumptions are sufficient to describe the long-run *stocks of divorces,* the annual *flow of new divorces* is driven by the dynamics of marriages forming and dissolving as couples subsequently discover their incompatibility. Thus, each year δ percent of the population marry, replacing an equal proportion of the population who die. If couples continuously assessed the status of their relationship, this would yield instantaneous adjustment to the new steady state. Instead, I assume that there is an α-percent chance per year that a couple will assess their compatibility; if this couple subsequently discovers that they are better off dissolving the marriage, they will get divorced. The bold line in Figure 2 shows the resulting divorce rate, and its adjustment following the adoption of unilateral divorce laws for a set of plausible parameters ($\alpha = 20$ percent, $c = 20$ percent, $\varepsilon = 0.4$ percent, $\delta = 2.5$ percent).[9] Note that even a very small value of ε yields a large spike in the flow of new divorces. This immediate rise in divorce reflects a pent-up demand for divorce as the stock of dissatisfied spouses who take advantage of the liberalized divorce laws, while small, is large relative to the annual flow of divorces. Not all of this effect is immediate, however, because many couples do not consider the implications of the new regime for their marriage for several years, and hence the divorce rate stays high for several years. Not surprisingly, the long-run effect of such a small change in regime (ε), is small.

Empirically, my approach will simply trace out the full adjustment path. Note that Friedberg's preferred specification includes only the single *Unilateral* dummy to capture the full adjustment process. Because the dynamics are not well captured by this single variable, state-specific trends pick up not only different preexisting trends across states, but also differences in the evolution of the divorce rate between reform and control states subsequent to the adoption of unilateral divorce laws (see Figure 2). The bold line shows the hypothetical divorce rate. The fitted time trend is shown in gray. Friedberg's equation effectively partials this out, and the residuals are shown as the dashed line. The *Unilateral* coefficient compares the average difference between the divorce rate and the trend before and after the legal change. Thus, her regression compares the line segments titled "before" and "after." This difference is several times larger than the true effect evident in the bold line.

This critique applies beyond this specific stylized example—any dynamics beyond a discrete series break are not fully accounted for by the simple *Unilateral* dummy, leading the state-specific trend "controls" to partly reflect the dynamic *response* of the response variable to the policy shock. Thus, this problem arises in any context in which panel-specific trends are included as controls and where the response to the policy shock yields interesting unmodeled dynamics. It is worth noting that it is not unusual in the labor literature simply to add panel-specific trends in this manner as a "check."[10] More generally, any reduced-form or structural analysis that assumes an immediate constant response to a policy shock may be misspecified if actual dynamics are more complex than a simple series break. Beyond the stock-flow example highlighted above, real, nominal, expectational, or belief stickiness will also yield interesting dynamics.

In this case, this problem causes the estimated *Unilateral* dummy to reflect the difference between the actual path of divorces and a systematically biased estimate of its counterfactual. Including state-specific quadratic time trends might either exacerbate or ameliorate this bias, depending on the specific dynamic response.

These problems are exacerbated when only a few observations are available before the policy shock. Friedberg's sample begins in 1968, while the wave of divorce reform followed fairly immediately, leaving only a couple of observations with which to identify preexisting state trends.

To resolve these problems, I extend Friedberg's sample back to 1956 (so as to allow for a credible identification of *preexisting* state-specific trends),[11] and add variables that model the dynamic response of divorce quite explicitly. I pursue a specification that imposes very little structure on the response dynamics, including dummy variables for the first two years

[9] Section V provides evidence for the choices of the c and ε parameters; δ is chosen so as to yield an average life span of 40 years following marriage, and the choice of α, while arbitrary, is chosen to yield a plausible dynamic response to the change in divorce laws.

[10] Indeed, of the 92 difference-in-difference papers identified by Bertrand et al., they report that 7 include panel-specific trends. Only two of these papers report specifications that explicitly identify the dynamic responses to the policy change.

[11] Before 1956, the divorce data by state are rather patchy. Appendix A shows that my longer sample does not much change Friedberg's estimates. Thus, to the extent that our estimates diverge, differences in identification approaches, rather than differences in samples, are the cause.

TABLE 2—DYNAMIC EFFECTS OF ADOPTING UNILATERAL DIVORCE LAWS
(*Dependent variable: Annual divorces per 1,000 persons* (*Cell mean* = *3.9*))

Specification:	(1) Basic specification	(2) State-specific linear trends	(3) State-specific quadratic trends
First 2 years	0.27	0.34	0.30
	(0.08)	(0.06)	(0.05)
Years 3–4	0.21	0.32	0.29
	(0.09)	(0.07)	(0.06)
Years 5–6	0.16	0.30	0.29
	(0.08)	(0.08)	(0.08)
Years 7–8	0.16	0.32	0.35
	(0.08)	(0.08)	(0.10)
Years 9–10	−0.12	0.08	0.16
	(0.08)	(0.09)	(0.12)
Years 11–12	−0.32	−0.10	0.05
	(0.08)	(0.10)	(0.14)
Years 13–14	−0.46	−0.20	0.03
	(0.08)	(0.11)	(0.17)
Year 15 onwards	−0.51	−0.21	0.25
	(0.08)	(0.12)	(0.20)
Controls			
Year FE	$F = 145$	$F = 54$	$F = 71$
State FE	$F = 220$	$F = 468$	$F = 523$
State * time	No	$F = 49$	$F = 56$
State * time^2	No	No	$F = 16$
Adjusted R^2	0.9310	0.9732	0.9822
Sample		1956–88, $n = 1631$ *state-years*	

Notes: Estimated using state population weights. Standard errors in parentheses.

of the new legal regime, for years three, and four, five, and six, and so on. Thus, these variables should identify the entire response function allowing the estimated state-specific time trends to identify preexisting trends.[12]

Table 2 shows my preferred set of estimates, running equation (2) on an unbalanced panel of divorce rates from 1956 to 1988:

$$\text{(2)} \quad \textit{Divorce Rate}_{s,t} = \sum_{k \geq 1} \beta_k \textit{Unilateral divorce has been in effect for k periods}_{s,t} + \sum_s \textit{State fixed effects}_s + \sum_t \textit{Time fixed effects}_t \left[+ \sum_s \textit{State}_s * \textit{Time}_t + \varepsilon_{s,t} + \sum_s \textit{State}_s * \textit{Time}_t^2 \right].$$

The first column of Table 2 reports results from a specification including only state and year fixed effects as controls; the second adds state-specific time trends, and the third also includes quadratic state-specific time trends. Figure 3 shows the results graphically. All three specifications suggest that the divorce rate spiked immediately following the adoption of unilateral divorce laws.[13] This effect declines

[12] Friedberg analyzed the effects in the first two years, although her estimates—reflecting the identification problems discussed above—suggest that the effects of unilateral divorce laws were smaller in their first two years.

[13] Part of the short-run up-tick in divorce rates likely reflects the fact that in certain states, waiting periods were shortened with the introduction of unilateral divorce (Robert

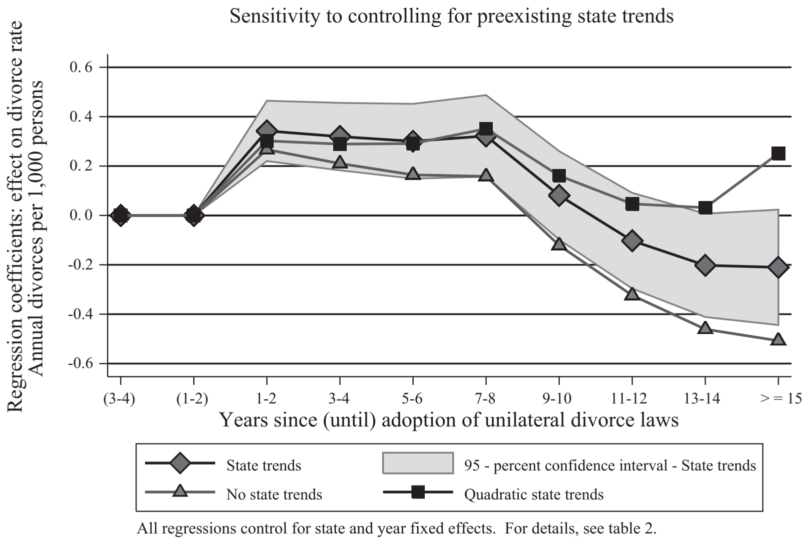

FIGURE 3. RESPONSE OF DIVORCE RATE TO UNILATERAL DIVORCE LAWS

over the ensuing decade, and the dynamic response is remarkably similar to that shown in the stylized example discussed above. A decade later, it is difficult to find any effects of divorce reform. Intriguingly, the coefficients become significantly negative after a little more than a decade in two specifications, although as one adds more controls, the long-run effects become less negative, and indeed are small, positive, and statistically insignificant when controlling for state-specific quadratic trends. The conclusion that divorce rose noticeably over the decade following reform appears quite robust. Evidence for a negative effect over the ensuing period is more fragile.

The fragility of the long-run estimates is a recurring theme throughout my robustness testing. For example, Figure 4 shows the results of similar regressions when analyzing several alternative taxonomies of family law regimes. The lack of precision in these estimates cautions against attempts to parse out a family of estimates corresponding to a more fine-grained coding of family law regimes.

Reconciling my results with Friedberg's is fairly simple, and California provides an illustrative example. The top panel of Figure 5 shows California's divorce rate after controlling for state and year fixed effects. The divorce rate clearly spikes following the 1970 reform, returns to its previous level by about 1980, and then drops to a lower level for the ensuing decade.

Friedberg focuses only on the shorter sample: 1968–1988 (highlighted in gray). Thus, the specification including only state and year fixed effects effectively compares the observations for 1968–1969 with those from 1970 onward. As can be seen, the average level of the divorce rate from 1970 to 1988 is fairly similar to that in the late 1960s (it is higher for a decade, and then lower for a decade), leading to the conclusion that the average effect throughout the period was zero. Indeed, recall that the results in column 1 of Tables 1 and 2 yielded estimates for the United States close to zero.

Friedberg finds a significant effect of divorce reform only when she adds state-specific trends (as in columns 2 and 3 in Table 1). To see why, note that her regression fits a strongly decreasing trend to California (the dashed gray line)—despite the fact that the *preexisting trend* appears to be flat or even slightly increasing. The gray line in the lower panel shows the residual variation identifying Friedberg's specification. By subtracting a decreasing trend, Friedberg is led to conclude that the divorce rate rose dramatically following

Schoen et al., 1975). There is also anecdotal evidence of couples delaying their divorce so as to take advantage of the non-adversarial no-fault procedures.

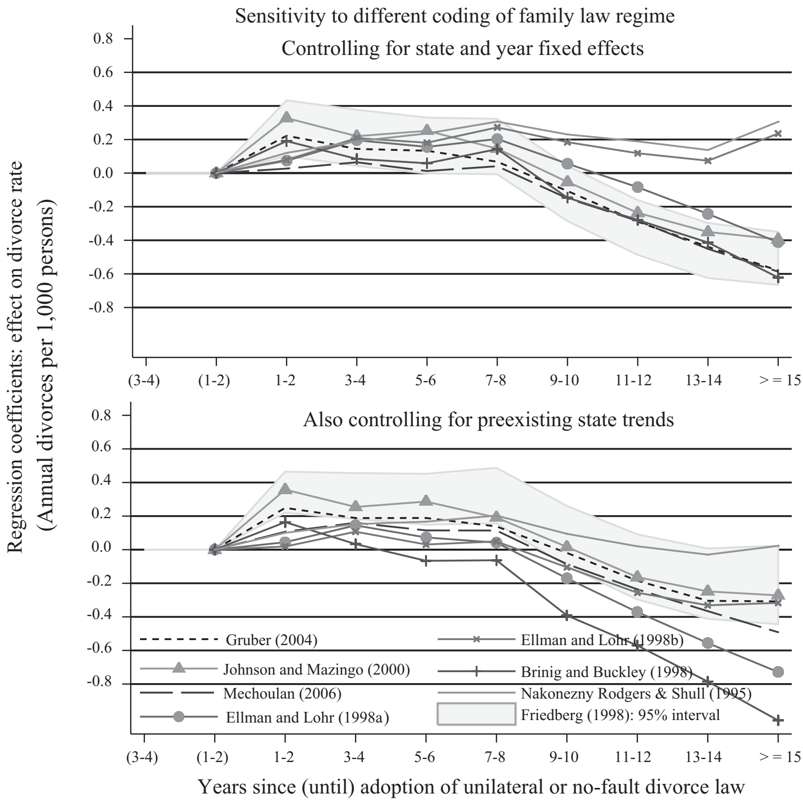

FIGURE 4. RESPONSE OF DIVORCE RATE TO DIVORCE LAW REFORM

Notes: Figure shows regression coefficients from a specification including state and year fixed effects (and also state-specific linear time trends in bottom panel), estimated over the 1956–1988 sample.

(a) Friedberg (1998) codes when unilateral divorce laws, with no separation requirements are adopted, using mostly secondary sources.
(b) Gruber (2004) codes unilateral divorce laws, with no separation requirements, using both primary and secondary sources.
(c) Johnson and Mazingo (2000) code unilateral divorce laws, citing Friedberg and Brinig as sources.
(d) Ellman and Lohr (1998i) code when each state adopted "irretrievable breakdown" as grounds for divorce, citing both primary and secondary sources.
(e) Ellman and Lohr (1998ii) code when each state adopted either "irretrievable breakdown" or "incompatibility/separation" as grounds for divorce.
(f) Brinig and Buckley (1998) code the date by which both no-fault grounds for dissolution and no-fault grounds for financial settlements have been adopted, citing both legislation and court decisions.
(g) Nakonezny et al. (1995) code the date of the state's adoption of no-fault grounds for either marital dissolution or financial settlements, citing mainly secondary sources.

the adoption of unilateral divorce laws, and that this effect persisted for 20 years. The thin line shows the residual variation identifying my regression (column 2 of Table 2). As one would expect from a casual inspection, there is not much of a preexisting trend, and thus adding controls for state-specific trends does not much change my estimates.

These misidentified state-specific trends are a ubiquitous problem in Friedberg's specification, even when allowing for a longer preintervention sample. To provide a point of

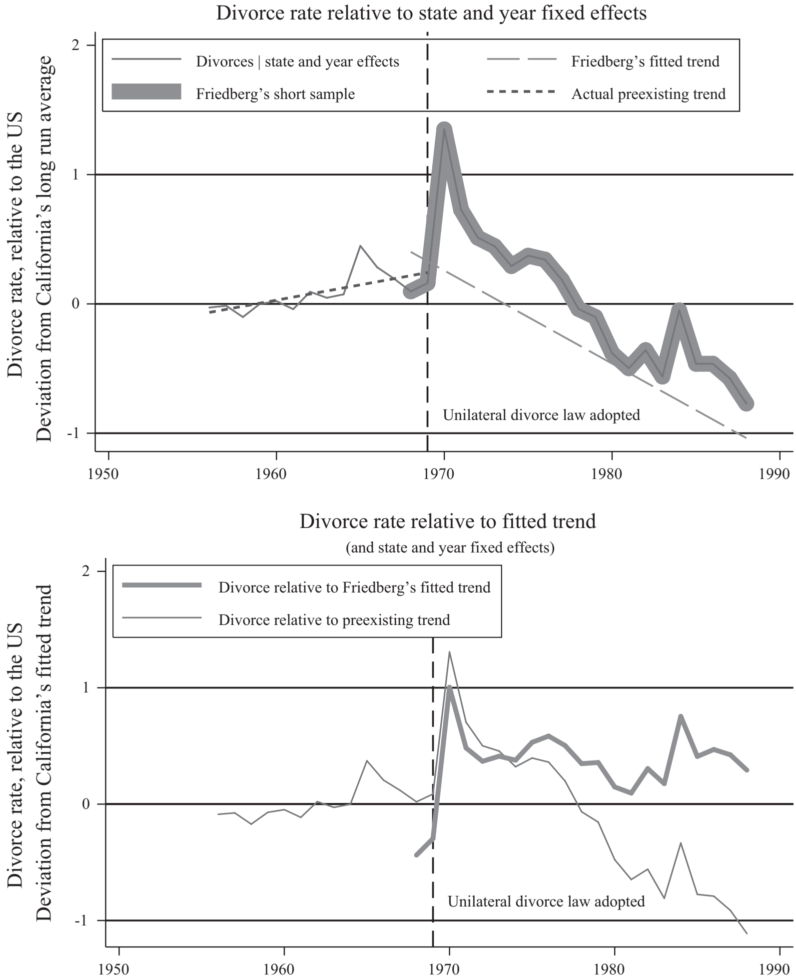

FIGURE 5. CALIFORNIA'S DIVORCE RATE

comparison, I estimated both my specification and Friedberg's over the complete 1956–1988 sample, controlling for state-specific time trends. Figure 6 plots my estimate of each state's time trend against that estimated from Friedberg's specification; the 29 states that changed their laws are marked with a cross, while the remaining 21 "control" states are shown with circles. The single *Unilateral* variable in Friedberg's specification picks up a shift in the level of the divorce rates following the reform, but leaves the subsequent downward trend following the initial post-reform spike in divorces to be picked up by state-specific trends. Thus, we see that her specification systematically estimates a more negative state-specific time trend in reform states. It is only when measured against this counterfactual of relatively falling divorce rates in reform states that Friedberg finds large and persistent effects of divorce laws on the divorce rate.

III. Implications for the Stock of Marriages: Census Data

Naturally these results on the *flow* of new divorces have testable implications for data on the *stock* of divorcees, and hence I turn to analyzing census data. I start by analyzing a specification suggested by Gruber (2004), focusing on census data from 1960 to 1990.

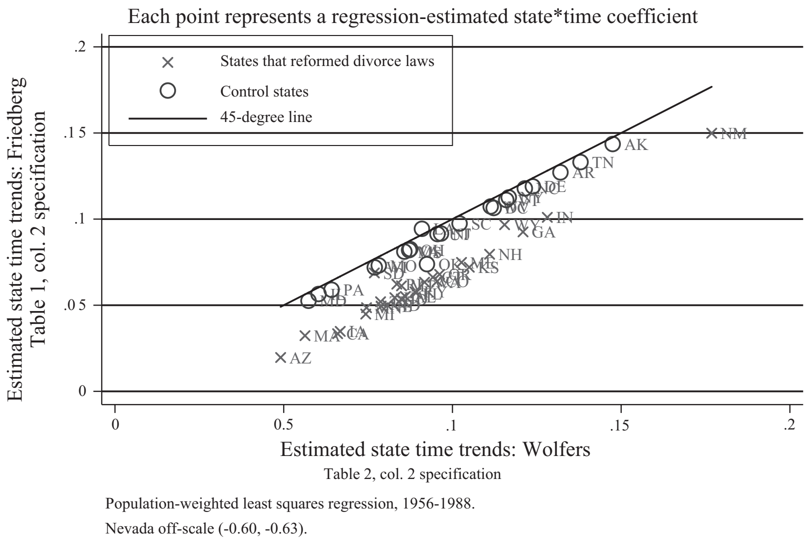

FIGURE 6. ESTIMATES OF STATE-SPECIFIC LINEAR TIME TRENDS

Gruber analyzes state-year-age-sex cells,[14] finding that the proportion of the population that is divorced at a point in time rises by about 1 percentage point (or 12 percent) following the adoption of unilateral divorce laws. That is, for each sex Gruber ran:

$$(3) \qquad p(Currently\ Divorced_{s,t,a}) = \beta Unilateral_{s,t} + \sum_r Race_{s,t,a} + \sum_s State\ fixed\ effects_s + \sum_a \sum_t Age_a * Time_t + \varepsilon_{s,t,a}.$$

The first column of Table 3 reprints Gruber's results.[15] As can be seen, the probability of being divorced on census day rose by around 1 percentage point following the adoption of unilateral divorce laws. While I was able to reconstruct these estimates from aggregate data provided to me by Gruber, I was not able to completely reconstruct these aggregate data from original Integrated Public Use Microdata Series (IPUMS) sources.[16] That said, remaining differences are extremely minor, and column 2 shows the corresponding estimates from my data yield very similar results.[17]

Unfortunately, these data describe those who are divorced at a point in time, while the majority of divorcees later remarry, and hence their divorces are not measured in these numbers. Indeed, data from the June 1995 CPS Marital History supplement reveal that of the female population age 25 to 50, only 49 percent of the *ever-divorced* population are to be found in the *currently divorced* pool; a further 47 percent

[14] Note that, in the census data, Gruber focuses on a person's current state of residence, which may differ from the state in which they divorced. Given that divorce may induce migration, this could induce nonclassical measurement error.

[15] For the sake of comparability, I revert to Gruber's coding of divorce laws when analyzing census data. Results using Friedberg's coding are similar.

[16] All of my data are from www.ipums.org. Following Gruber, I analyze data on U.S.-born adults age 25 to 50 from the 1960 1-percent sample, the 1970 Form one state 1-percent sample, and the 1980 and 1990 5-percent state samples. While divorce rates in each age-sex-state of residence-year cell are derived using person weights, regression weights reflect the number of observations used in constructing each cell, yielding estimates that are representative of the unbalanced microdata, rather than the U.S. population.

[17] Correspondence with Gruber suggests that these minor differences may reflect different treatment of observations with certain missing or imputed values, and persons in group quarters.

TABLE 3—EFFECTS OF UNILATERAL DIVORCE LAWS ON THE STOCK OF DIVORCES—CENSUS DATA

	p(Currently divorced)			p(Ever divorced)
Dependent variable	Gruber's results	Replicating Gruber (same sample)	Replicating Gruber (shorter sample)	Dependent variable is "ever divorced" (shorter sample)
		Panel A. Women		
Mean of dependent var	11.0%	11.2%	9.2%	22.5%
Unilateral coefficient	0.0128	0.0101	0.0104	0.0009
	(0.0040)	(0.0025)	(0.0028)	(0.0037)
Elasticity	11.6%	9.0%	11.3%	0.4%
		Panel B. Men		
Mean of dependent var	8.2%	8.5%	6.8%	19.3%
Unilateral coefficient	0.0095	0.0082	0.0082	0.0004
	(0.0038)	(0.0029)	(0.0027)	(0.0042)
Elasticity	11.6%	9.6%	12.1%	0.2%
Sample	1960–90	1960–90	1960–80	1960–80
	n = 5,304	n = 5,304	n = 3,978	n = 3,978

Notes: Standard errors in parentheses. Weighted to reflect underlying microdata. Standard errors clustered by state. All regressions based on IPUMS data from the 1950–1990 Censuses: 1960 1-percent state sample, 1970 Form one 1-percent state sample, 1980–2000 5-percent state samples. Restricted to U.S.-born population age 25 to 50. Each coefficient is from a separate regression, controlling for race, state of residence dummies, age dummies, year dummies, and age * year dummy interactions.

have remarried, 3 percent have remarried and are separated, while 1 percent have remarried and been widowed. Moreover, Imran Rasul (2004) shows that the propensity to remarry is a function of unilateral divorce laws, and that unilateral divorce led remarriage rates of divorcees to decline by around one-third to one-half.

By exploiting data on the number of times each respondent has been married, I construct a measure of the *ever-divorced* population.[18] Because remarriage is identifiable only in the 1960–1980 Censuses, I confine my attention to this period. Column 3 shows that restricting attention to this shorter sample yields similar results. Presumably this reflects the fact that only South Dakota adopted unilateral divorce laws after the 1970s. Further, this is consistent with the suggestion from Figure 3 that most of the rise in divorce occurred in the first eight years following legal reform.

Column 4 turns to analyzing the effect of divorce laws on the proportion of the population who have ever been divorced. The *ever-divorced* population includes both those *currently divorced* and those who had previously divorced but subsequently remarried. This broader measure reveals no effects of divorce laws on the propensity to divorce. Further, these results are about as precisely estimated as Gruber's. Taken together, the results in columns 3 and 4 suggest a change in the composition but not the size of the *ever-divorced* population, and specifically that divorcees became less likely to remarry following the adoption of unilateral divorce laws. This implied decline in remarriage is consistent with Rasul's analysis of the effects of unilateral divorce on the remarriage rate.

Thus, census data suggest that no effect of divorce laws on the *ever-divorced* population is evident by 1980. By contrast, the flow data suggest that divorce rates rose over the corresponding period. Reconciling these findings

[18] My measure of the *ever-divorced* population includes both those currently divorced and those who are currently married, separated, or widowed, but are on their second (or higher) marriage. Implicitly this measure assumes that those who have remarried were divorced—rather than widowed—by their first spouse. June 1995 CPS data suggest that this is largely true: my proxy measure would categorize 26.9 percent of the female population age 25 to 50 as *ever divorced.* Of these, 12.9 percent are currently divorced, 13.5 percent have been divorced, but are currently married, widowed, or separated, while only 0.3 percent are widows who remarried (and hence would be misclassified as ever divorced).

hinges on the greater statistical precision of the flow estimates. The central estimates in Table 2 suggested that the divorce rate rose by about 0.2 to 0.3 divorces per 1,000 persons per year, for around a decade. To a first approximation, this suggests that the ever-divorced population should have risen through the 1970s by around 2 to 3 divorces per 1,000 men or women, or 0.4 to 0.6 percent of the population in reform states. My estimates in Table 3 yield a 95-percent confidence interval for this prediction ranging from -0.6 to $+0.8$ percent of the population. That is, the administrative divorce data suggest a very precisely estimated—but small—effect of unilateral divorce laws on divorce rates. The size of this effect is sufficiently small that it cannot be rejected in census data on the *ever-divorced* population. Both datasets suggest that unilateral divorce laws explain only a very small fraction of the dramatic rise in divorce over the past 40 years.

IV. Interpretation

Data on the flow of new divorces suggest that the shift to unilateral divorce had important—albeit relatively small—effects on the divorce rate over the decade following its adoption, a finding that the census data do not reject. Moreover, the estimates suggest even smaller—and in some cases negative—long-run effects of these laws. In this section, I explore four possible explanations of these long-run estimates: the dynamics associated with a shift toward earlier divorce rather than more divorce; changes in marriage rates; contamination of divorce norms from treatment states to control states; and regression to the mean.

A. *Dynamics*

The increase in divorces for a decade following reform, and the subsequent decrease, may in fact be two sides of the same coin: unilateral divorce may have simply led to the earlier dissolution of bad matches, thereby shifting a number of divorces from the 1980s into the 1970s. Thus, extending the data by a further decade may yield something closer to the true long-run effects. For the divorce rate data, I extend the sample to 1998 using data reported in *Vital Statistics.* I cannot update data on the ever-divorced population beyond 1980 because the Census stopped asking about remarriage. I can, however, update data on the share of the population currently divorced by adding data from the 2000 Census.

The first two columns of Table 4 show the results over this longer sample—panel A shows the effects on the flow data, and the negative coefficients remain a feature even a quarter of a century after the reform. The census data, shown in panel B, yield complementary results, although for brevity I show results only from the female sample. The stock of divorcees rose strongly in the decade following reform, stayed high for a decade, and declined a little subsequently. While the results in Table 3 caution against the assumption that the evolution of the currently divorced population is representative of the number of divorces, these data are consistent with the finding that unilateral divorce laws increased the flow of new divorces for only about a decade. Once again, estimates based on census data are sufficiently imprecise that they cannot falsify a wide range of dynamic responses.

B. *Matching*

The quantity and quality of marriage market matches may change in response to divorce law changes. Indeed, Rasul (2004) shows that the marriage rate declined by about 3 to 4 percent following the adoption of unilateral divorce laws. As such, the size of the population "at risk" of divorce declined with unilateral divorce laws, possibly reducing the divorce rate. This suggests that *divorces per 1,000 persons* is an inappropriate metric, and analysis should focus on *divorces per 1,000 married persons.* Tempering this, even important changes to entry into marriage will only change the stock of existing marriages very slowly. Columns 3 and 4 of Table 4 analyze divorces per 1,000 married persons.[19] Note that variable is scaled differently—68 percent of the population is married, and

[19] To create my new independent variable, I divide the divorce rate by the proportion of the adult population in a state that is currently married. This latter series is calculated by linearly interpolating decadal estimates derived from IPUMS microdata for 1950 to 2000, and so misses some high-frequency variation. This deviates slightly from Friedberg (1998), who used CPS data to construct annual state-level estimates of the married population.

TABLE 4—LONG-RUN EFFECTS OF UNILATERAL DIVORCE LAWS

	Divorce rate per 1,000 persons per year		Divorces per 1,000 married persons age 18 plus	
Dependent variable	1956–88 sample $n = 1,631$	1956–98 sample $n = 2,102$	1956–88 sample $n = 1,631$	1956–98 sample $n = 2,102$
Panel A. Dependent variable is divorce rate (administrative flow data)				
Cell mean	3.9	4.1	5.8	6.2
Law change has been in effect for:				
First 2 years	0.27	0.27	0.42	0.42
	(0.08)	(0.10)	(0.11)	(0.13)
Years 3–4	0.21	0.22	0.37	0.37
	(0.09)	(0.10)	(0.12)	(0.13)
Years 5–6	0.16	0.17	0.35	0.35
	(0.08)	(0.10)	(0.11)	(0.13)
Years 7–8	0.16	0.17	0.39	0.40
	(0.08)	(0.09)	(0.11)	(0.13)
Years 9–10	−0.12	−0.10	0.02	0.03
	(0.08)	(0.09)	(0.11)	(0.13)
Years 11–12	−0.32	−0.29	−0.25	−0.23
	(0.08)	(0.09)	(0.11)	(0.13)
Years 13–14	−0.46	−0.42	−0.44	−0.41
	(0.08)	(0.09)	(0.11)	(0.13)
Years 15–16	−0.51	−0.40	−0.46	−0.35
(Year 15 + cols 1, 3)	(0.08)	(0.09)	(0.11)	(0.13)
Years 17–18		−0.47		−0.45
		(0.09)		(0.12)
Years 19–20		−0.61		−0.66
		(0.09)		(0.13)
Years 21–22		−0.68		−0.79
		(0.09)		(0.13)
Years 23–24		−0.63		−0.68
		(0.10)		(0.14)
Years 25 plus		−0.75		−0.83
		(0.10)		(0.14)

Panel B. Dependent variable is share of women *currently divorced* (census data)

	p(Currently divorced)		p(Currently divorced|ever married)	
Dependent variable	1960–90 sample $n = 5,304$	1960–00 sample $n = 6,630$	1960–90 sample $n = 5,304$	1960–00 sample $n = 6,630$
Cell mean	11.2%	12.2%	12.8%	14.1%
Law change has been in effect for:				
1 to 10 years	0.0101	0.0102	0.0104	0.0104
	(0.0028)	(0.0033)	(0.0037)	(0.0042)
11 to 20 years (11 years + cols 1, 3)	0.0100	0.0093	0.0106	0.0097
	(0.0034)	(0.0035)	(0.0031)	(0.0034)
20 years plus		0.0064		0.0068
		(0.0053)		(0.0047)

Notes: Panel A: See notes to Table 2. Panel B: See notes to Table 3.

hence the dependent variable has a mean of 5.9 rather than 3.9. These results yield an initial increase in divorce that is slightly more pronounced, while the subsequent decline is roughly similar to that shown in columns 1 and 2.

Beyond this, there may be changes at the quality margin that this quantity adjustment does not address. These effects are difficult to measure, however, or even to sign. For instance, one might expect that reduced exit costs would lead to lower quality matches, which might raise the divorce rate. Against this, the benefits of marriage (tying your spouse to a contract) are reduced in a no-fault world, and hence the proportion of the population that is married may decline.

C. *Contamination*

It seems likely that unilateral divorce laws affect the divorce rate both directly through changing legal parameters, and indirectly by reducing the stigma associated with divorce. A thicker remarriage market may further reduce the cost of divorce. Reduced divorce stigma and enhanced remarriage prospects are unlikely to respect state boundaries. Thus, easier access to divorce in reform states may also reduce stigma in non-reform states, leading their divorce rates to rise, albeit with a lag. Further, it seems likely that legislative activism in reform states created pressure for more liberal judicial interpretation of ongoing consent divorce laws in other states (Rodgers et al., 1999; Glenn, 1999). Taking these factors together, it may be that the control states experienced de facto reform, leading the divorce rate to rise in the control states relative to that in the true reform states—possibly with a lag.

In the first column of Table 5, I attempt to control for the shock to local norms by adding a control for the proportion of neighboring states with unilateral divorce laws. While a norms-based story suggests that this variable will have a positive coefficient, it turns out to be statistically significant and negative, a result that is suggestive of migratory divorce (the administrative flow data reflect the state in which the divorce is obtained). The estimated effect of a state reforming its own laws is largely unchanged. That said, this strategy does not control for contamination effects to the extent that they represent national rather than local phenomena. I now turn to examining this issue further.

D. *Regression to the Mean*

While the basic method of this literature has been quasi-experimental—arguing that variation in unilateral divorce laws is exogenous—a close reading of the reform movement suggests that this is only partly true. While the timing of these reforms (among reform states) was probably random (Jacob, 1988), states with historically higher divorce rates were more likely to choose to reform their laws (see Figure 1, or Peters, 1992). This suggests that convergence in divorce norms, or regression to the mean, may explain why divorce rates rose faster in control states, yielding negative coefficients.

Table 5 shows three attempts to highlight this issue. Columns 3 and 4 involve a simple control strategy, interacting a measure of the state's historical divorce propensity (the share of that state's population age 25 to 50 that reported ever being divorced in the 1950 Census), with a linear time trend, and time fixed effects, respectively. Column 3 confirms that the divorce rate spiked following reform, but highlights the fragility of the negative coefficients over the ensuing decade. Column 4 yields reasonably precise estimates suggesting no statistically significant effect of unilateral divorce laws. Column 5 exploits only that variation that is clearly quasi-experimental, restricting the sample to reform states; thus, the equation is identified only off the variation in timing of reform across reform states. In none of these cases do the long run effects of unilateral divorce laws appear to be significantly negative. Adding state-specific trends to the regressions in Table 5 yields qualitatively similar results.

V. Discussion

A clear finding from this analysis is that the divorce rate exhibits interesting dynamics in response to a change in legal regime. As a result, standard difference-in-difference approaches are led to confound pre-existing trends with the effects of the policy shock. A more plausible specification that takes explicit account of these dynamics yields new results that appear somewhat more robust.

The data broadly indicate that divorce law reform led to an immediate spike in the divorce rate that dissipates over time. After a decade, no effect can be discerned. This basic insight is robust to a range of alternative interpretations of divorce laws. Further, it is consistent with census data on the ever-divorced population. More puzzling, certain estimates suggest that the divorce rate declined over the ensuing period. This eventual decline in the divorce rate is less robust, and a range of alternative specifications suggests that this decline may be illusory.

How should these results inform our theories about the family? In terms of assessing the causes of the dramatic rise in U.S. divorces through the 1970s, these results suggest only a minor role for changing divorce laws. Figure 7

TABLE 5—ROBUSTNESS TESTING
(*Dependent variable: Annual divorces per 1,000 persons*)

	From Table 4 (column 2) (1)	Control for neighbor's reforms (2)	Control for historical divorce rate * time trend (3)	Control for historical divorce rate * time FE (4)	Reform states only (5)
Law change has been in effect for:					
First 2 years	0.27	0.28	0.36	0.15	0.40
	(0.10)	(0.09)	(0.09)	(0.09)	(0.14)
Years 3–4	0.22	0.26	0.35	0.13	0.50
	(0.10)	(0.10)	(0.09)	(0.09)	(0.16)
Years 5–6	0.17	0.25	0.34	0.12	0.58
	(0.10)	(0.10)	(0.09)	(0.09)	(0.18)
Years 7–8	0.17	0.25	0.38	0.17	0.68
	(0.09)	(0.10)	(0.09)	(0.09)	(0.20)
Years 9–10	−0.10	−0.02	0.14	−0.03	0.49
	(0.09)	(0.10)	(0.09)	(0.09)	(0.22)
Years 11–12	−0.29	−0.21	−0.01	−0.17	0.34
	(0.09)	(0.09)	(0.09)	(0.09)	(0.23)
Years 13–14	−0.42	−0.33	−0.10	−0.19	0.27
	(0.09)	(0.10)	(0.09)	(0.09)	(0.24)
Years 15–16	−0.40	−0.31	−0.03	−0.10	0.32
	(0.09)	(0.10)	(0.09)	(0.09)	(0.26)
Years 17–18	−0.47	−0.38	−0.06	−0.07	0.28
	(0.09)	(0.10)	(0.09)	(0.09)	(0.28)
Years 19–20	−0.61	−0.51	−0.13	−0.14	0.17
	(0.09)	(0.10)	(0.09)	(0.09)	(0.29)
Years 21–22	−0.68	−0.59	−0.20	−0.22	0.14
	(0.10)	(0.10)	(0.10)	(0.09)	(0.31)
Years 23–24	−0.63	−0.54	−0.16	−0.12	0.36
	(0.10)	(0.10)	(0.10)	(0.10)	(0.35)
Year 25 plus	−0.75	−0.65	−0.16	−0.04	0.47
	(0.10)	(0.10)	(0.10)	(0.10)	(0.39)
Controls					
% Unilateral (adjoining states)		−0.30			
		(0.09)			
Time * historical divorce rate[a]			−0.72		
			(0.05)		
Time FE * historical divorce rate[a]				Yes	
Sample	1956–1998	1956–1998	1956–1998	1956–1998	1956–1998
	n = 2,102	n = 2,102	n = 2,102	n = 2,102	n = 1,288
					(31 states)

Notes: Standard errors in parentheses. Estimated using state population weights. All regressions include state and year fixed effects.

[a] The "historical divorce rate" is the share of the population aged 25–50 in each state ever divorced in the 1950 census. For Alaska and Hawaii, 1960 values are substituted.

maps the aggregate divorce rate against a counterfactual in which no states adopted unilateral divorce laws. It should be clear that unilateral divorce laws explain very little of the rise in the aggregate divorce rate.

These results do not yield a particularly clear answer to the motivating theoretical question of whether Coasian bargaining occurs between spouses. It is clear that divorce law has an effect on the divorce rate; it is less clear that this effect is persistent. While the finding of an effect (even if temporary) on divorce rates strictly falsifies the predictions of efficient Coasian bargaining, the more relevant question is: How important are bargaining frictions?

The results from the divorce law experiments analyzed in this paper suggest that the Coasian assumption of efficient bargaining arguably provides a more useful guide than the polar opposite assumption of no bargaining. To see why, consider the implications of the following simple arithmetic: if there is a probability p that married

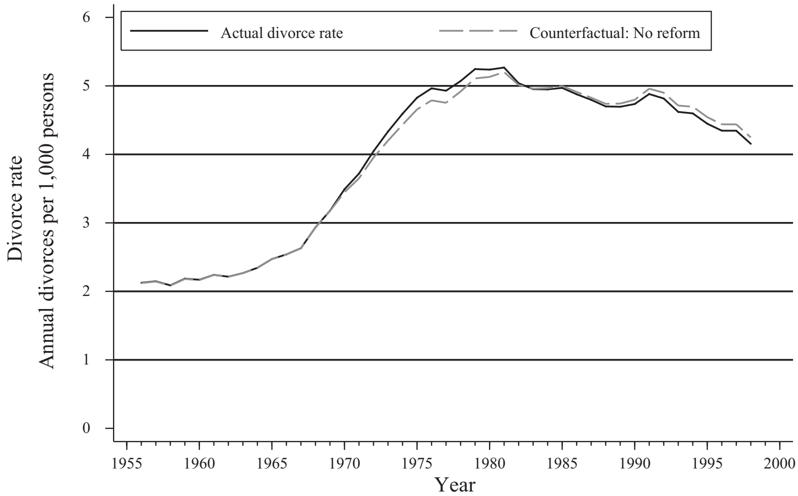

FIGURE 7. U.S. DIVORCE RATE: EFFECT OF DIVORCE LAWS

Notes: Figure is constructed based on the specification in column 3 of Table 5.

life will be sufficiently worse than expected that it leads a spouse to prefer divorce, and if these forecast errors are independent, then—in the absence of bargaining—both spouses will desire divorce in p^2 marriages (thereby meeting the requirement for a consent divorce), while at least one spouse will desire divorce in $2p - p^2$ marriages (thereby meeting the requirement for a unilateral divorce). In 1960, around one-fifth of all people living in consent divorce states had been through a divorce by age 50, suggesting that $p = 0.45$. Thus, in the absence of bargaining, one would have expected the proportion of marriages ending in divorce to rise from 20 percent to around 70 percent, while my results suggest a rise of only around one-half of a percentage point, around one one-hundredth as large as suggested by the no-bargaining approach. (If the divorce rate rose by 0.2 to 0.3 divorces per 1,000 persons per year for a decade, this yields 4 to 6 more divorces per 1,000 men or women, or around half a percentage point.)

Of course, if bargaining occurs, in many cases in which one spouse finds their marriage to be less happy than expected, their partner may be able to redistribute the spoils of marriage to keep the couple together. Coasian bargaining is simply the limiting case in which the couple gets divorced only if it is jointly optimal. That the observed rise in divorce is so small relative to that suggested by the no-bargaining null suggests that spousal bargaining is sufficiently close to efficient that, in the vast majority of cases, couples are able to effect sufficient transfers to stay married even when the law would allow the unhappy spouse unilaterally to exit the marriage. Wolfers (2003) develops this reasoning further, estimating that spousal bargaining saves around 98 percent of those marriages in which the change in divorce laws may have otherwise led one spouse to leave the marriage unilaterally. This analysis rests heavily on the assumption that each spouse has independent forecast errors, although the main insights drawn above are robust to allowing even quite substantial correlation in these errors.

Of course, the truth probably lies somewhere between these two extreme assumptions of independent shocks, or no spousal bargaining, and the safest conclusion is that the data suggest either that there is substantial agreement between spouses as to whether or not to seek a divorce, or that transaction costs are relatively small, facilitating considerable bargaining over rents. Further insight into this issue can be gained by examining changing distribution within marriage subsequent to the adoption of unilateral divorce laws, as in Stevenson and Wolfers (2006).

APPENDIX

Panel A reproduces the results from Friedberg's 1968–1988 sample. In panel B, I extend

TABLE A1—EXTENDING THE SAMPLE
(*Friedberg's specification*)

	(1) Basic specification	(2) State-specific trends linear	(3) State-specific trends quadratic
Panel A. Friedberg's sample: 1968–88 (n = 1,043)			
Unilateral	0.004	0.447	0.441
	(0.056)	(0.050)	(0.055)
Year effects	F = 89	F = 95	F = 8.9
State effects	F = 217	F = 196	F = 131
State trend, linear	No	F = 25	F = 9.3
State trend, quadratic	No	No	F = 6.5
Adjusted R^2	0.946	0.976	0.982
Panel B. Wolfers' sample: 1956–88 (n = 1,631)			
Unilateral	−0.055	0.477	0.334
	(0.050)	(0.054)	(0.046)
Year effects	F = 137	F = 69	F = 76
State effects	F = 207	F = 454	F = 511
State trend, linear	No	F = 51	F = 54
State trend, quadratic	No	No	F = 17
Adjusted R^2	0.927	0.972	0.982

Notes: Standard errors in parentheses. Estimated using state population weights.

Friedberg's sample back to 1956. Divorce data were hand-entered from annual editions of *Vital Statistics*. Extending the data on divorce laws was relatively simple. In two cases, Friedberg codes the adoption of unilateral divorce as predating her sample—Alaska and Oklahoma. Gruber codes these reforms as having occurred in 1935 and 1953, respectively, and hence I simply follow his coding.

These results are not particularly sensitive to extending the sample. At first glance, this is surprising—intuition suggests that the inclusion of a longer stretch of preintervention data offsets the bias issues described in the text. Simulations indicated that while adding centuries of preintervention data would indeed yield consistent estimates, the inclusion of another 12 years of data probably yields slightly *more* biased estimates.

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