

Did Unilateral Divorce Laws Raise Divorce Rates?

A Reconciliation and New Results

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Abstract

Application of the Coase Theorem to marital bargaining suggests that shifting from a consent divorce regime to no-fault unilateral divorce laws should not affect divorce rates. Each iteration of the empirical literature examining the evolution of divorce rates across US states has yielded different conclusions about the effects of divorce law liberalization. I show that these results reflect a failure to explicitly model the dynamic response of divorce rates to a shock to the policy regime. Taking explicit account of the dynamic response of divorce rates to the policy shock, I find that liberalized divorce laws caused a discernible rise in divorce rates for about a decade, with much of this effect concentrated in the first few years, and much smaller effects in following years. Estimates of longer-term effects are more fragile, and cannot reject either moderately positive or moderately negative changes. None of my estimates suggest that unilateral divorce laws can explain much of the rise in the divorce rate over the past half century. These results are suggestive of spouses bargaining within marriage, with an eye to their partner's divorce threat.

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Introduction

The “no-fault revolution” that swept the United States through 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. Peters (1986) argued that divorce rates were unaffected by the change in legal regime, a finding rebutted by 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 (Gatland (1997)).

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 pre-existing 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 pre-existing 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 Coase

¹ Related contributions in the economics literature include: Gruber (2004), Johnson and Mazingo (2000) and Mechoulan (2001). In the Law and Economics literature see: Brinig and Buckley (1998), Ellman and Lohr (1998), and Ellman (2000), and in the sociology literature: Nakonezny, Shull and Rodgers (1995), Glenn (1997), Rodgers, Nakonezny and Shull (1997), Glenn (1999) and Rodgers, Nakonezny and Shull (1999).

² See Binner and Dnes (2001), Gruber (2004), Johnson and Mazingo (2000), Rowthorn (1999).

Theorem to marital relations. 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 only requires 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 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 one I present Friedberg’s results, and show that her estimates are replicable. Working through a simple example, section two 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 three finds complementary evidence in census data tracing the evolution of the stock of ever-divorced people; section four explores the empirical robustness of my findings, and section five turns to interpretation.

1. Replicating Friedberg (1998)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 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 people in each state. These data cover virtually every divorce in the US throughout this period. She estimated:⁴

$$Divorce\ Rate_{s,t} = \beta Unilateral_{s,t} + \sum_s State\ fixed\ effects_s + \sum_t Time\ fixed\ effects_t \\ [+ \sum_s State_s * Time_t + \sum_s State_s * Time_t^2] + \varepsilon_{s,t} \quad (1)$$

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

³ All of the data and programs used in this paper are available for download from: www.nber.org/~jwolfers.

divorce rate attributable to the legal change. Much of the earlier debate in this literature focussed 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 are robust to a range of different coding regimes.

Equation (1) is estimated using population-weighted least squares. Panel A of Table 1 simply reprints Friedberg's results. The specification shown in column one 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 pre-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 of 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 two 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 thousand people per year, this translates to a rise of a little under 10%. 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

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.

shown in Panel B, my regressions also closely replicated detail on state and year effects provided in the appendices of Friedberg's paper. 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. However, the omission of these variables should only bias these coefficients 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 pre-existing 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 pre-existing 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 pre-existing 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 confound pre-existing 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.

2. 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

⁶ On computational procedures, see McCullough and Vinod (1999). Regarding replication, see Dewald *et. al.* (1986).

⁷ Controls are defined as those states that did not change their divorce laws during Friedberg's 1968-88 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.

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 eventually 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. However, during the transition to this new steady state, 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 $c\%$ most incompatible marriages to dissolve, with unilateral divorce laws that lead a further $\varepsilon\%$ of marriages to end. While these assumptions are sufficient to describe the long-run *stocks of divorcees*, 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 $\delta\%$ 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 $\alpha\%$ 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 ($\alpha=20\%$, $c=20\%$, $\varepsilon=0.4\%$, $\delta=2.5\%$).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. However, not all of this effect is immediate, 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 pre-existing 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. Figure 2 illustrates. 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 un-modeled dynamics. It is worth noting that it is not unusual in the labor literature to simply 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 mis-specified 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 there 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 pre-existing state trends.

To resolve these problems I extend Friedberg's sample back to 1956 (so as to allow for a credible identification of *pre-existing* 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 of the new legal regime, for years three and

⁹ Section 5 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, Duflo and Mullainathan, 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.

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 pre-existing trends.¹²

Table 2 shows my preferred set of estimates, running equation (2) on an unbalanced panel of divorce rates from 1956-88:

$$Divorce\ Rate_{s,t} = \sum_{k \geq 1} \beta_k Unilateral\ divorce\ has\ been\ in\ effect\ for\ k\ periods_{s,t} + \sum_s State\ fixed\ effects_s + \sum_t Time\ fixed\ effects_t + \sum_s State_s * Time_t + \varepsilon_{s,t} \quad (2)$$

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 rates spiked immediately following the adoption of unilateral divorce laws.¹³ This effect declines 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 for unobserved state heterogeneity, 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: 1968-88 (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 1970-1988 is fairly similar to that in the late 1960s (it is higher for a decade, and then lower for a decade), leading to

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 (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.

the conclusion that the average effect throughout the period was zero. Indeed, recall that the results in column one of Tables 1 and 2 yielded estimates for the US close to zero.

Friedberg finds a significant effect of divorce reform only when she adds state-specific trends (as in columns two and three 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 *pre-existing 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 the adoption of unilateral divorce laws, and that this effect persisted for twenty years. The thin line shows the residual variation identifying my regression (column two of Table 2). As one would expect from a casual inspection, there is not much of a pre-existing trend, and thus adding controls for state-specific trends does not much change my estimates.

These mis-identified state-specific trends are a ubiquitous problem in Friedberg’s specification, even when allowing for a longer pre-intervention sample. To provide a point of comparison, I estimated both my specification and Friedberg’s over the complete 1956-88 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.

3. 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 1960-1990. 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%) following the adoption of unilateral divorce laws. That is, for each sex Gruber ran:

$$p(\text{Currently Divorced}_{s,t,a}) = \beta \text{Unilateral}_{s,t} + \sum_r \text{Race}_{s,t,a} + \sum_s \text{State fixed effects}_s + \sum_a \sum_t \text{Age}_a * \text{Time}_t + \varepsilon_{s,t,a} \quad (3)$$

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 non-classical measurement error.

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 one 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 IPUMS sources.¹⁶ That said, remaining differences are extremely minor, and column two shows the corresponding estimates from my data yields 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 aged 25-50, only 49% of the *ever divorced* population are to be found in the *currently divorced* pool; a further 47% have remarried, 3% have remarried and are separated, while 1% have remarried and been widowed. Moreover, 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 only identifiable in the 1960-1980 censuses, I confine my attention to this period. Column three 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 four turns to analyzing the effect of divorce laws on the proportion of the population who have ever been divorced. The population *ever-divorced* 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

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 US-born adults aged 25-50 from the 1960 1% sample, the 1970 Form one state 1% sample, and the 1980 and 1990 5% 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 US 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.

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% of the female population aged 25-50 as *ever*

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 a corresponding period. Reconciling these findings hinges on the greater statistical precision of the flow estimates. The central estimates in Table 2 suggested that the divorce rate rose by about 0.2-0.3 divorces per thousand people 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 2-3 divorces per thousand men or women, or 0.4-0.6 percent of the population in reform states. My estimates in Table 3 yield a 95% 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 forty years.

4. 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.

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. However I can 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

divorced. Of these, 12.9% are currently divorced, 13.5% have been divorced, but are currently married, widowed or

reform. The census data, shown in panel B, yield complementary results, although for brevity I only show results 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 only increased the flow of new divorces for about a decade. Once again estimates based on census data are sufficiently imprecise that they cannot falsify a wide range of dynamic responses.

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 3-4 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 thousand people* is an inappropriate metric, and analysis should focus on *divorces per thousand married people*. Tempering this, even important changes to entry into marriage will only change the stock of existing marriages very slowly. Columns three and four of Table 4 analyze divorces per thousand married people.¹⁹ Note that variable is scaled differently – 68% of the population is married, and 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 one and two.

Beyond this, there may be changes at the quality margin that this quantity adjustment does not address. However these effects are difficult to measure 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.

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

separated, while only 0.3% are widows who remarried (and hence would be misclassified as ever divorced).

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 1950-2000, 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.

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.

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 three and four involve a simple control strategy, interacting a measure of the state’s historical divorce propensity (the share of that state’s population aged 25-50 that reported being ever divorced in the 1950 census), with a linear time trend, and time fixed effects, respectively. Column three confirms that the divorce rate spiked following reform, but highlights the fragility of the negative coefficients over the ensuing decade. Column four yields reasonably precise estimates suggesting no statistically significant effect of unilateral divorce laws. Column five 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.

5. 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 US divorces through the 1970s, these results suggest only a minor role for changing divorce laws. Figure 7 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 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% to around 70% 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 0.2-0.3 divorces per thousand people per year for a decade, this yields 4-6 more divorces per thousand men or women, or around half a percentage point).

Of course if bargaining occurs, then 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 so as to keep the couple together. Coasian bargaining is simply the limiting case in which the couple only gets divorced 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 to unilaterally exit the marriage. Wolfers (2003) develops this reasoning further, estimating that spousal bargaining saves around 98% 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).

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Table 1: Friedberg's Results
Dependent Variable: Annual divorces per thousand people

	(1) Basic Specification	(2) State-Specific Trends Linear	(3) State-Specific Trends Quadratic
Panel A: Friedberg (1998)			
Unilateral	0.004 (0.056)	0.447 (0.050)	0.441 (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 (0.057)	0.431 (0.051)	0.435 (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

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.

Table 2: Dynamic Effects of Adopting Unilateral Divorce Laws

<i>Dependent variable: Annual divorces per thousand people (Cell mean=3.9)</i>			
<i>Specification:</i>	<i>(1)</i>	<i>(2)</i>	<i>(3)</i>
	Basic Specification	State-Specific Linear Trends	State-Specific Quadratic Trends
First 2 years	.27 (.08)	.34 (.06)	.30 (.05)
Years 3-4	.21 (.09)	.32 (.07)	.29 (.06)
Years 5-6	.16 (.08)	.30 (.08)	.29 (.08)
Years 7-8	.16 (.08)	.32 (.08)	.35 (.10)
Years 9-10	-.12 (.08)	.08 (.09)	.16 (.12)
Years 11-12	-.32 (.08)	-.10 (.10)	.05 (.14)
Years 13-14	-.46 (.08)	-.20 (.11)	.03 (.17)
Year 15 onwards	-.51 (.08)	-.21 (.12)	.25 (.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*time²	No	No	F=16
Adjusted R²	.9310	.9732	.9822
Sample	1956-88, <i>n</i> =1631 <i>state-years</i>		

(Standard errors in parentheses)

Estimated using state population weights.

Table 3: Effects of Unilateral Divorce Laws on the Stock of Divorces – Census Data

Dependent Variable	p(Currently Divorced)			p(Ever Divorced)
	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	.0128 (.0040)	.0101 (.0025)	.0104 (.0028)	.0009 (.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	.0095 (.0038)	.0082 (.0029)	.0082 (.0027)	.0004 (.0042)
Elasticity	11.6%	9.6%	12.1%	0.2%
Sample	1960-90 <i>n</i> =5304	1960-90 <i>n</i> =5304	1960-80 <i>n</i> =3978	1960-80 <i>n</i> =3978

(Standard errors in parentheses)

Weighted to reflect underlying microdata. Standard errors clustered by state.

All regressions based on IPUMS data from the 1950-90 Censuses: 1960 1% state sample, 1970 Form one 1% state sample, 1980-2000 5% state samples. Restricted to US-born population aged 25-50.

Each coefficient is from a separate regression, controlling for race, state of residence dummies, age dummies, year dummies, and age*year dummy interactions.

Table 4: Long-Run Effects of Unilateral Divorce Laws

Dependent variable	Divorce Rate (per thousand people per year)		Divorces per thousand married people aged 18 plus	
	1956-88 Sample <i>n=1631</i>	1956-98 Sample <i>n=2102</i>	1956-88 Sample <i>n=1631</i>	1956-98 Sample <i>n=2102</i>
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	.27 (.08)	.27 (.10)	.42 (.11)	.42 (.13)
Years 3-4	.21 (.09)	.22 (.10)	.37 (.12)	.37 (.13)
Years 5-6	.16 (.08)	.17 (.10)	.35 (.11)	.35 (.13)
Years 7-8	.16 (.08)	.17 (.09)	.39 (.11)	.40 (.13)
Years 9-10	-.12 (.08)	-.10 (.09)	.02 (.11)	.03 (.13)
Years 11-12	-.32 (.08)	-.29 (.09)	-.25 (.11)	-.23 (.13)
Years 13-14	-.46 (.08)	-.42 (.09)	-.44 (.11)	-.41 (.13)
Years 15-16 (Year 15+cols 1,3)	-.51 (.08)	-.40 (.09)	-.46 (.11)	-.35 (.13)
Years 17-18		-.47 (.09)		-.45 (.12)
Years 19-20		-.61 (.09)		-.66 (.13)
Years 21-22		-.68 (.09)		-.79 (.13)
Years 23-24		-.63 (.10)		-.68 (.14)
Year 25 plus		-.75 (.10)		-.83 (.14)
Panel B: Dependent Variable is Share of Population Currently Divorced (Census Data) <i>Female Sample</i>				
Dependent variable	p(Currently divorced)		p(Currently divorced Ever married)	
	1960-90 Sample <i>n=5304</i>	1960-00 Sample <i>n=6630</i>	1960-90 Sample <i>n=5304</i>	1960-00 Sample <i>n=6630</i>
Cell Mean	11.2%	12.2%	12.8%	14.1%
Law Change has been in effect for:				
1 to 10 years	.0101 (.0028)	.0102 (.0033)	.0104 (.0037)	.0104 (.0042)
11 to 20 years (11 years+ cols 1,3)	.0100 (.0034)	.0093 (.0035)	.0106 (.0031)	.0097 (.0034)
20 years plus		.0064 (.0053)		.0068 (.0047)

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

Table 5: Robustness Testing
Dependent Variable: Annual divorces per thousand people

	From Table 4 (column 2)	Control for Neighbor's reforms	Control for historical divorce rate * Time Trend	Control for historical divorce rate * Time FE	Reform states only
	(1)	(2)	(3)	(4)	(5)
Law Change has been in effect for:					
First 2 years	.27 (.10)	.28 (.09)	.36 (.09)	.15 (.09)	.40 (.14)
Years 3-4	.22 (.10)	.26 (.10)	.35 (.09)	.13 (.09)	.50 (.16)
Years 5-6	.17 (.10)	.25 (.10)	.34 (.09)	.12 (.09)	.58 (.18)
Years 7-8	.17 (.09)	.25 (.10)	.38 (.09)	.17 (.09)	.68 (.20)
Years 9-10	-.10 (.09)	-.02 (.10)	.14 (.09)	-.03 (.09)	.49 (.22)
Years 11-12	-.29 (.09)	-.21 (.09)	-.01 (.09)	-.17 (.09)	.34 (.23)
Years 13-14	-.42 (.09)	-.33 (.10)	-.10 (.09)	-.19 (.09)	.27 (.24)
Years 15-16	-.40 (.09)	-.31 (.10)	-.03 (.09)	-.10 (.09)	.32 (.26)
Years 17-18	-.47 (.09)	-.38 (.10)	-.06 (.09)	-.07 (.09)	.28 (.28)
Years 19-20	-.61 (.09)	-.51 (.10)	-.13 (.09)	-.14 (.09)	.17 (.29)
Years 21-22	-.68 (.10)	-.59 (.10)	-.20 (.10)	-.22 (.09)	.14 (.31)
Years 23-24	-.63 (.10)	-.54 (.10)	-.16 (.10)	-.12 (.10)	.36 (.35)
Year 25 plus	-.75 (.10)	-.65 (.10)	-.16 (.10)	-.04 (.10)	.47 (.39)
Controls					
% Unilateral (adjoining states)		-.30 (.09)			
Time*historical divorce rate^a			-.72 (.05)		
Time FE * historical divorce rate^a				Yes	
Sample	1956-98 n=2102	1956-98 n=2102	1956-98 n=2102	1956-98 n=2102	1956-98 n=1288 (31 states)

(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.

Figure 1

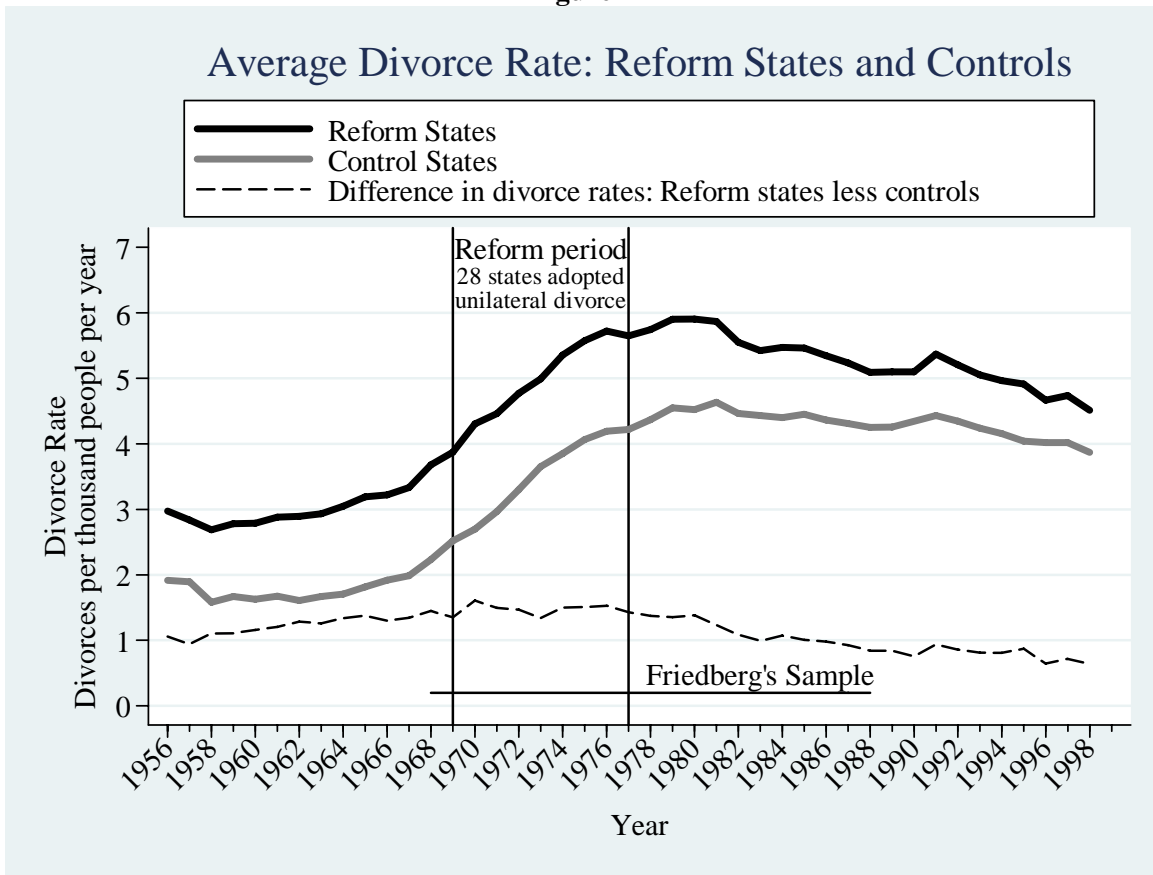
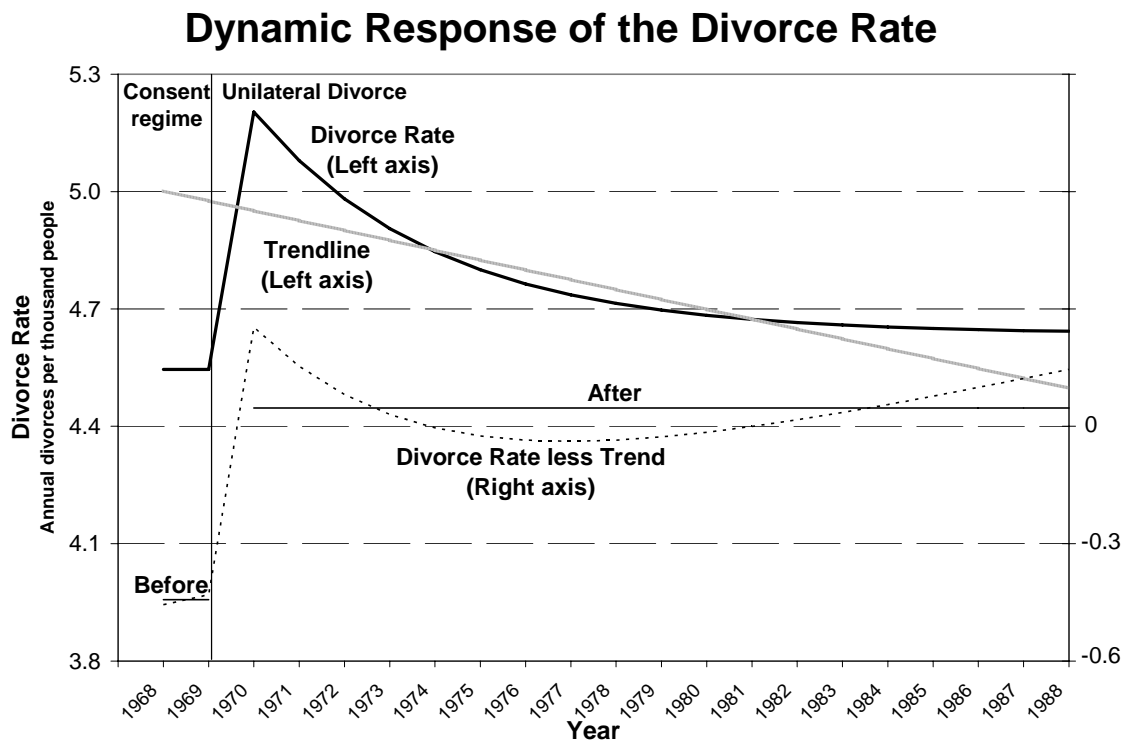


Figure 2



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

Figure 3

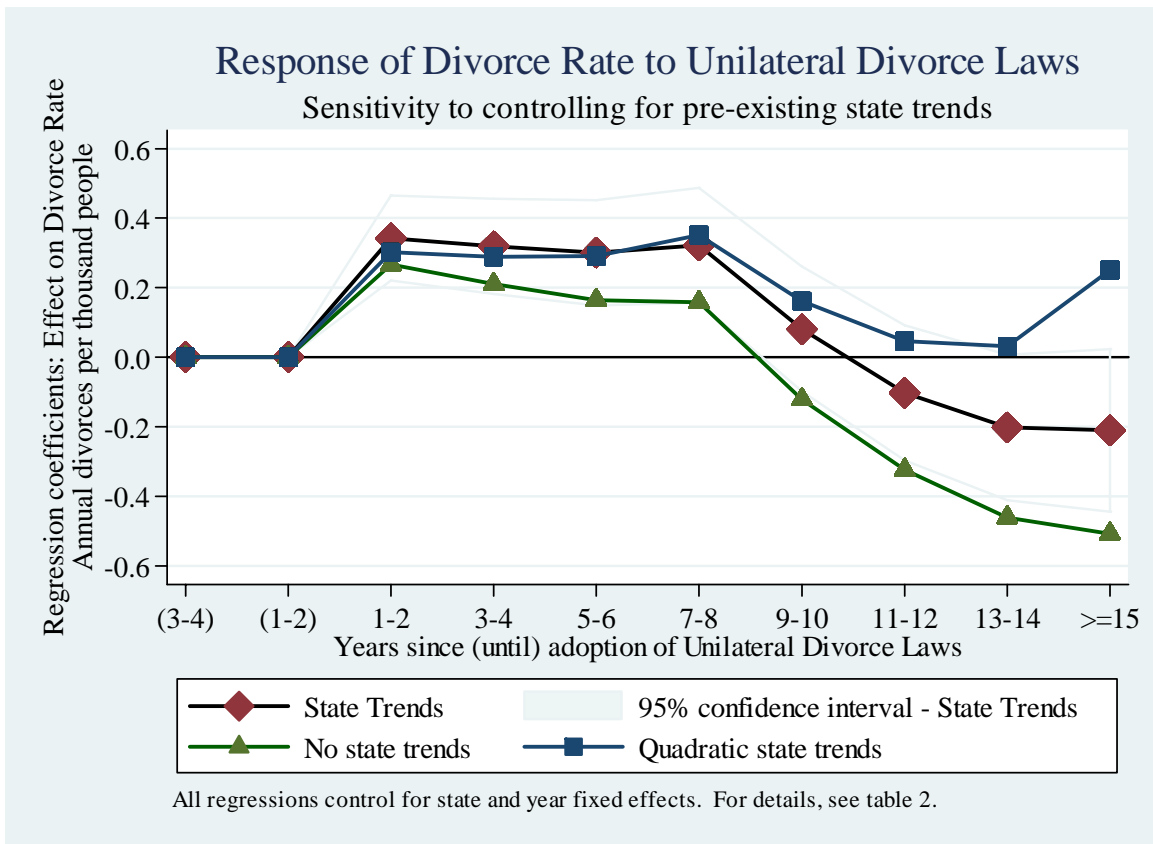
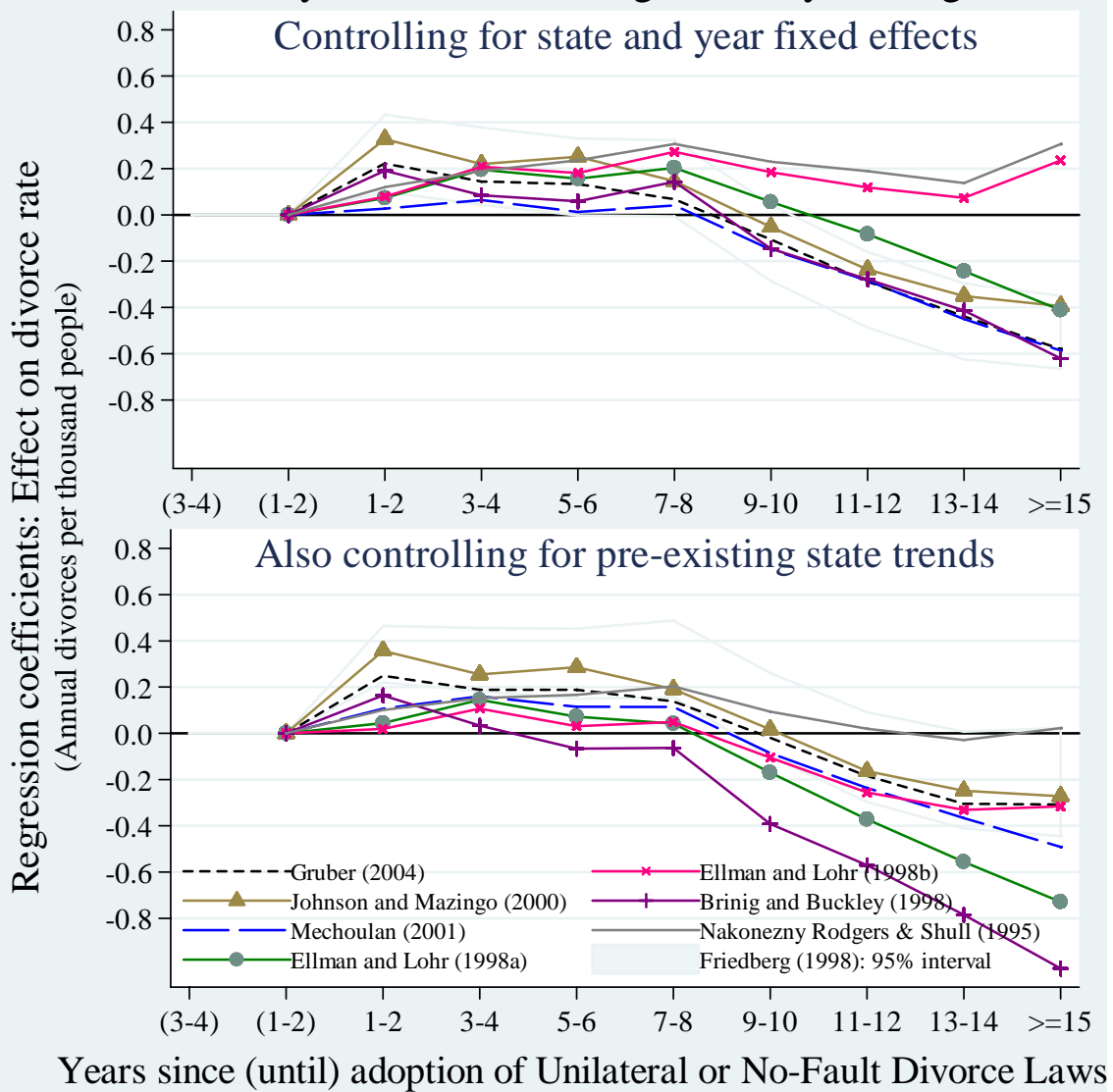


Figure 4

Response of Divorce Rate to Divorce Law Reform Sensitivity to different coding of family law regime



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 1956-88 sample.

- Friedberg (1998) codes when unilateral divorce laws, with no separation requirements are adopted, using mostly secondary sources.
- Gruber (2004) codes unilateral divorce laws, with no separation requirements, using both primary and secondary sources.
- Johnson and Mazingo (2000) code unilateral divorce laws, citing Friedberg and Brinig as sources.
- Ellman and Lohr (1998a) code when each state adopted “irretrievable breakdown” as grounds for divorce, citing both primary and secondary sources.
- Ellman and Lohr (1998b) code when each state adopted either “irretrievable breakdown” or “incompatibility / separation” as grounds for divorce.
- 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.
- Nakonezny, Shull and Rodgers (1995) code the date of the state’s adoption of no-fault grounds for either marital dissolution or financial settlements, citing mainly secondary sources.

Figure 5

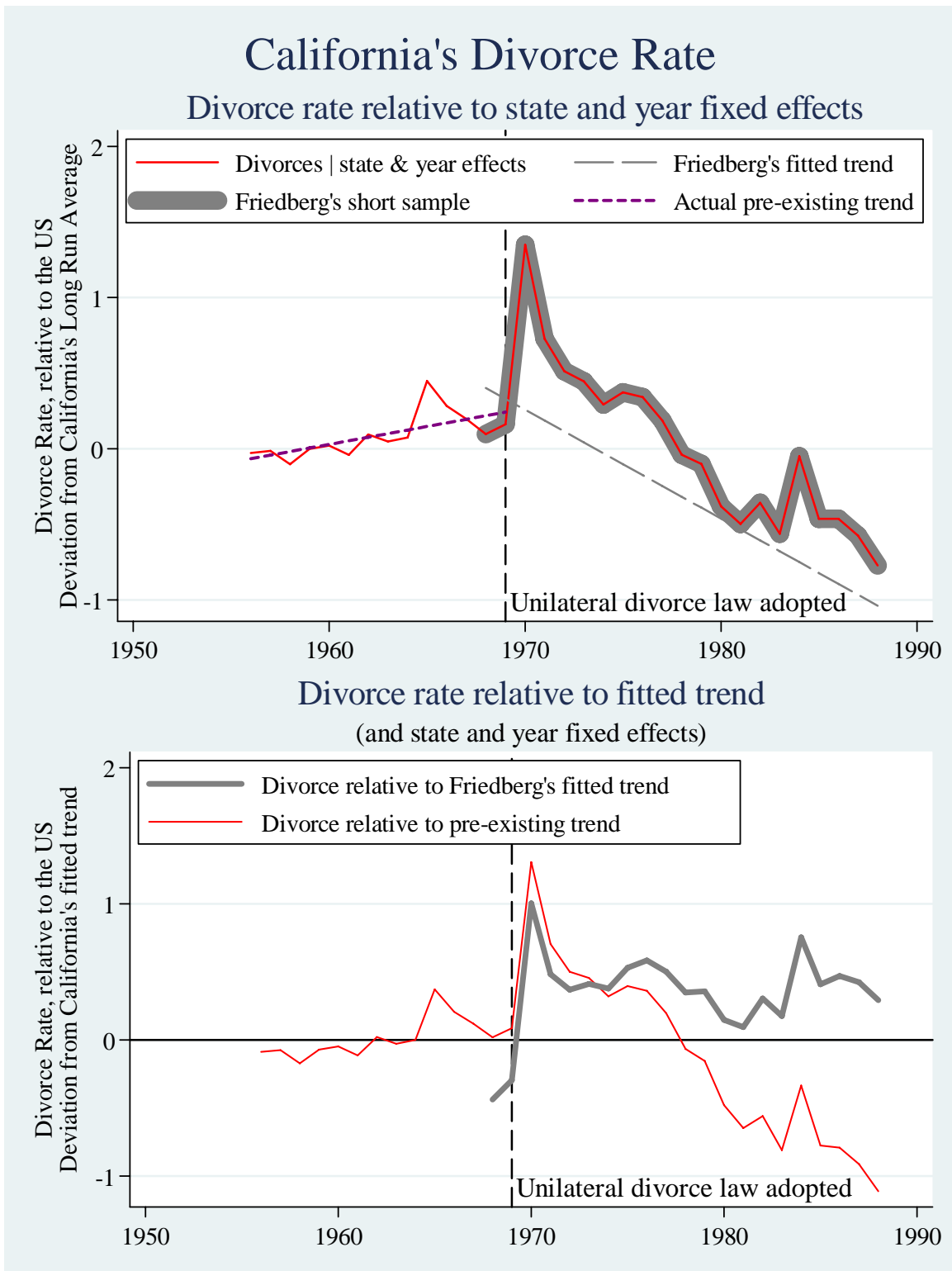


Figure 6

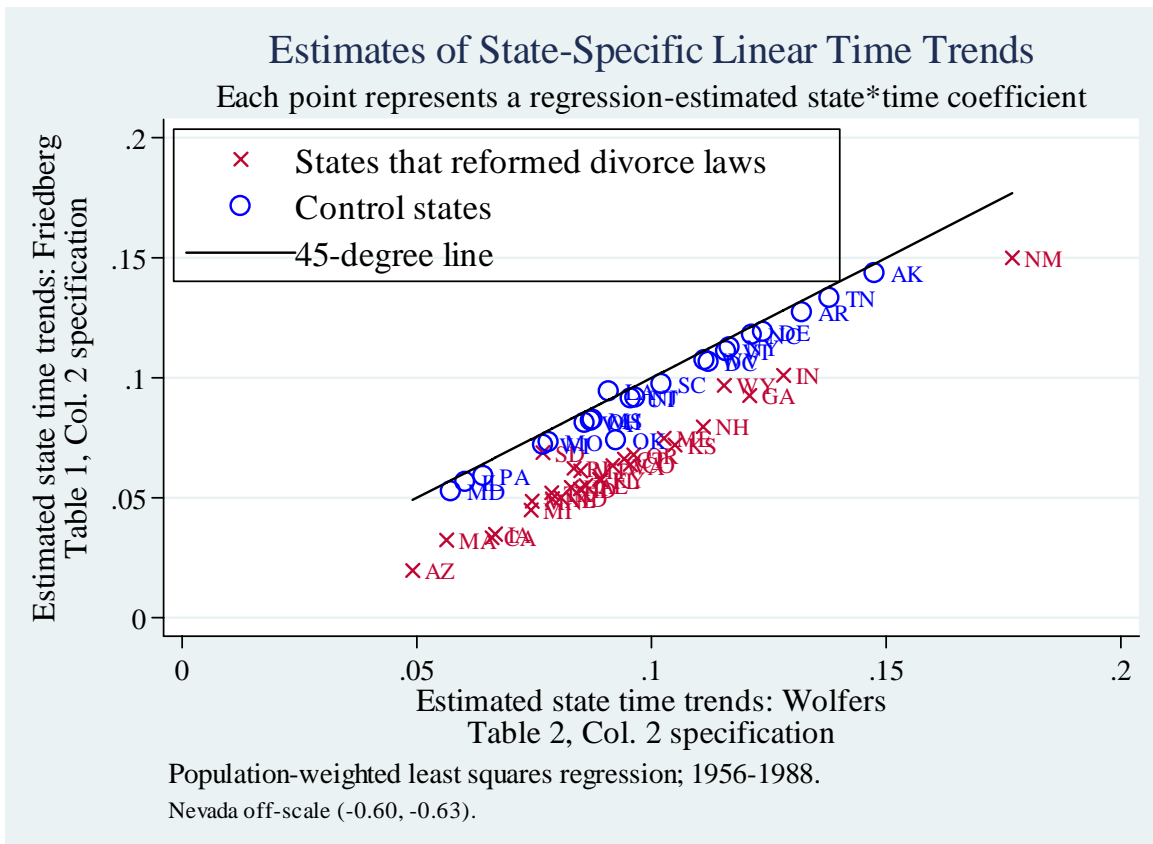
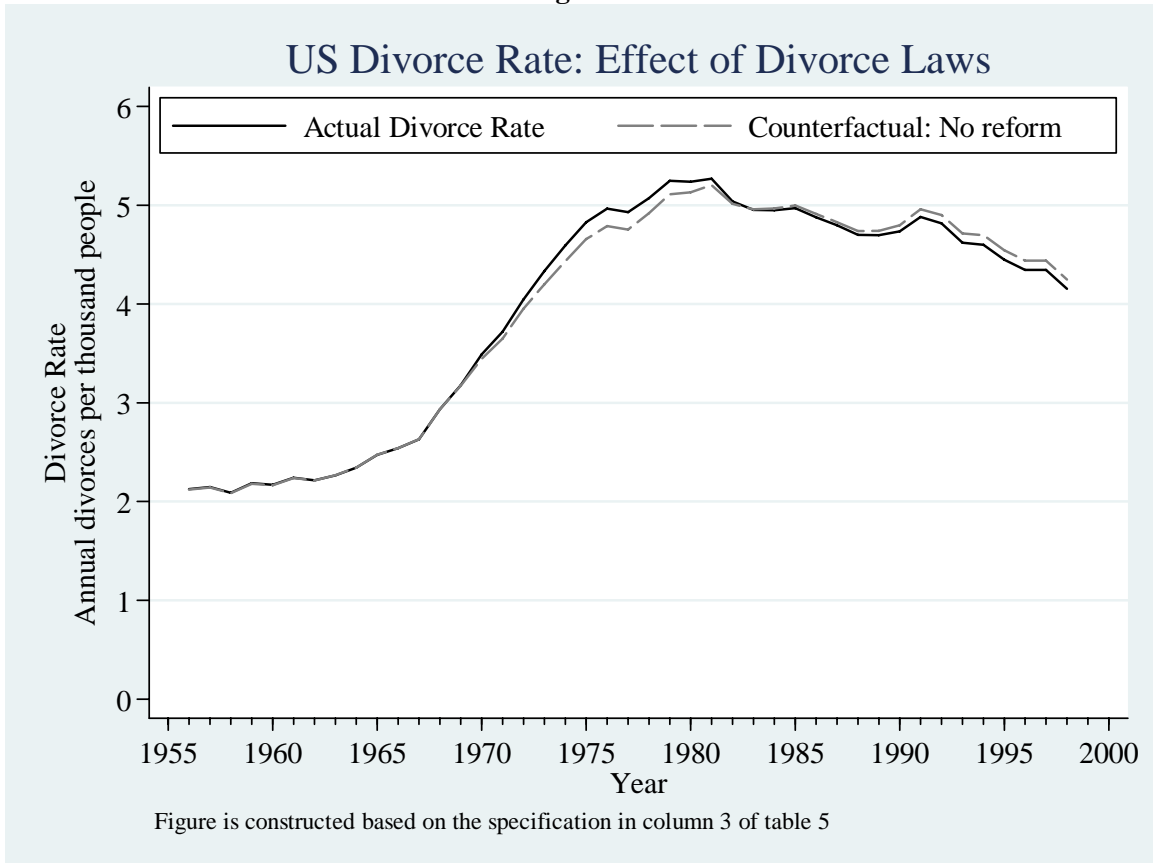


Figure 7



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

Appendix A: 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 ($n=1043$)			
Unilateral	.004 (.056)	.447 (.050)	.441 (.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 R²	0.946	0.976	0.982
Panel B: Wolfers' Sample: 1956-88 ($n=1631$)			
Unilateral	-.055 (.050)	0.477 (0.054)	0.334 (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 R²	0.927	0.972	0.982

(Standard errors in parentheses)

Estimated using state population weights.

Panel A reproduces the results from Friedberg's 1968-88 sample. In Panel B, I extend 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 pre-dating 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 pre-intervention data offsets the bias issues described in the text. Simulations indicated that while adding centuries of pre-intervention data would indeed yield consistent estimates, the inclusion of another twelve years of data probably yields slightly *more* biased estimates.